The Nature of Physical Reality

A PHILOSOPHY OF MODERN PHYSICS

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PREFACE

THIS BOOK has been written from a profound conviction that men engaged in the development of physical theory can profit from philosophical reflection about the meaning of their research, and that modern physics holds a message for philosophy. This latter argument is not a novel one, and in espousing it the author enters with some diffidence an arena that teems with gladiators of distinction. He should therefore state his challenge. It is that he believes the attitudes of uncritical realism, unadorned operationalism, and radical empiricism, which pervade most of the discussions and much of the thinking on subjects of science, to be outmoded and in disharmony with the successful phases of contemporary physics. He starts by analyzing *all* experience, not only the peripheral part called *empirical knowledge* in a narrow sense. He ends with an epistemology which is in keeping with both classical physics and the quantum theory, a philosophy of science which allows this reputedly heterodox new discipline, this breeding ground for paradoxes, to be seen as a culmination of methods long present in natural science.

Brief explanations of several features which are apt to evoke objections from the philosophic reader will be offered here. The early chapters of the book may seem needlessly discursive and may appear to deal with traditional philosophic problems which it is not wholly proper for scientists to raise. These chapters were nevertheless included not only because they prepare the way for the nonphilosophic reader, but more particularly because the later portions of the book will argue that "traditional" questions have greater relevance for science than is frequently believed. However, omission of the first three chapters will not preclude understanding of the book's main points.

An explanation should also be made for the occasional use of mathematics, albeit a very limited use. No major conclusions are drawn from purely mathematical arguments, and the reasoning can be followed in its main outline by readers who do not wish to encumber themselves with burdensome details. But the clarity that comes from mathematical comprehension of certain issues, when set against the qualitative understanding available through verbal means, is so overwhelmingly profitable that I did not wish to surrender it altogether. Therefore a compromise was made. All mathematical arguments were reduced to a form in which a reader reasonably conversant with (not necessarily skilled in) the ordinary infinitesimal calculus can follow them. This simplification entailed nothing worse than restrictions on subject matter and required no sacrifice of precision.

The physicist is likely to find statements that are provocative, indeed, perhaps mildly shocking. I doubt if he will take this amiss, for his own recent discoveries have been sufficiently stunning to render him fairly immune to shocks from philosophic considerations.

Most subjects chosen for study will interest every person of fundamental concerns who also has an interest in science. I believe it likely that he will be stimulated, and thus perhaps benefited, by the present treatment, even though he may disagree with some details of it.

There is a dearth of serious books on the methodology of the exact natural sciences and a similar want of such courses in America. It is my hope that the present volume may partly fill this need. If used as a textbook, all parts of which are to be thoroughly digested, it will probably require some basis of preliminary training both in physics and in philosophy. It would appear that seniors in American colleges, who are majoring in one of these fields and have taken a course or two in the other, could use the book with profit. To serve their needs, lists of books for collateral reading have been assembled at the ends of chapters. These have been selected without bias, and every list contains references in which are presented points of view entirely at variance with my own.

Obviously, I owe a general debt of gratitude to many authors, but a most specific one to my colleagues of the Philosophy Department at Yale University, for kindling in me a genuine interest in their problems and for leading me with patience and understand-

ing to many literary treasures in their field. Professor Harold S. Burr of the Department of Anatomy has done me the kindness of reading the entire manuscript and offering advice and criticism during its preparation; Professor F. S. C. Northrop, himself the author of remarkable books on similar subjects, has honored my efforts by reading proof and by suggesting improvements. Finally, I would acknowledge my indebtedness to Adolf Grünbaum, who read the first draft and proposed numerous changes in presentation.

HENRY MARGENAU

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CHAPTER 1

Preliminary Survey of Reality

1.1. Introductory Remarks

To gain knowledge of general principles by way of abstract exposition is a possible but not an expeditious course. It seems wiser to approach them with a large but well-defined project in view, a project which calls for continued and varied application of the principles to be studied, and which may serve at once as goal and as illustration. The principles under discussion in this book make up the subject known as the *epistemology* of science, or more simply as the philosophy of physics and its related disciplines. The project selected as the specific goal of our investigation is to determine the meaning of *physical reality.*

We do not start with a fixed set of ideas. In the earlier chapters of the book an attempt is made to condense the vague and unformed matrix of popular and semiscientific concepts that surround the problem of reality into increasingly definite concerns and into progressively specific questions. The terminology will be diffuse at the beginning but will attain sharpness as the work proceeds. A philosopher may, therefore, without harm to understanding (and perhaps to save himself annoyance) pass lightly over the first two or three chapters of the book. In the present chapter we desire to focus attention upon three rather obvious components of the real: the *enduring*, the *thing-like*, and the *efficacious.*

A quest for the real inspires most of the efforts of our race. It fills the scientist with curiosity and zeal for new adventures; it sets the mind of the philosopher to a contemplation of past pinnacles of thought; it leads the historian to scrutinize the recorded deeds of man for constant patterns. It flares in the exuberance of the mystic and congeals to dogmatism in the reliant knowledge of the practical man; it sings in the symphonies of great composers and vibrates through the vision of poets. It may attain the stature of Promethean defiance or reduce itself to the humility of a sinner seeking divine grace. In one way or another it is of peculiar concern to us.

The concern may be as casual as the sight-seer's, or it may reach the ecstasy of amazement which culminates in the outcry: Why am I, and why is there a reality at all? Why was *I* destined to have this fate? Between these lie all degrees of emotional response. A concept that invokes so great a variety of reactions is not an easy one to define, nor could it be presumed to have a single meaning.

Sometimes, in looser discourse, reality is set rather close to truth. When a tale is true, its characters are real; the truth about an episode is its real course. What is true is said to be a fact, and facts somehow constitute reality. But for the purposes of our present investigation we wish to disregard this popular identification of reality and truth, the specious character of which becomes apparent upon brief reflection. For truth, as it is here understood, is a property of statements, and statements may or may not have reference to reality. A theorem of mathematics can be true and yet have no bearing upon reality, while a statement about real objects may well be untrue. Hence we shall always regard truth as a logical term and never allow it to predispose us toward a confusion of *formally true* and *material* fact. Failure to make this distinction leads straight to the position of the medieval realists, for whom logical truth implied reality.

This book strives for a clear, though perhaps only a partial, understanding of what is real; it does not start with any of the notions that are vaguely current. Nor can it analyze and criticize them one by one, since we have as yet no criterion for such judgment, and also because these notions are too numerous for review. What finally emerges from our inquiry must, to be sure, conform in a general way to the dominant usage of the term *real* if such usage is discernible; but our analysis will purge it of inconsistencies and vain pretenses. By coupling mere inspection of usage with an account of what is rationally and empirically meaningful, our report can rise above the level of semantic analysis and present an outline, however tentative, of what reality should comprise.

In this volume an attempt is made to illuminate only *physical* reality. But the metaphysician need have no fear of bias, nor should he raise a protest against this limitation as prejudging the issue. It is our conviction that a careful study of any field will reveal its limitations, provided that this study is conducted in good faith and with all the facilities it offers. The history of our culture is full of instances in which successful disciplines recognize and acknowledge their boundaries; physical science and mathematics are the most self-critical among them. For example, the possibility of strange kinds of geometry with perplexing properties was discovered by men working with, and vitally concerned over, Euclidean geometry, not by those who disliked the subject. In a similar way a study of *physical* reality may open our eyes to other and possibly larger kinds. More will be said about the possibility and significance of nonphysical reality in a later chapter.

For the present, then, it seems indicated that we survey the more obvious aspects of our problem, viewing it first from afar and then approaching it for closer inspection. And thus we perceive at once a minor confusion which is occasioned by different historical components of the reality idea.

1.2. The Enduring

From the Greeks we have inherited a preoccupation for a sort of "principle of being," for some ultimate reality discoverable in, through, or beyond our sensory experience but not identical with it. Like Parmenides and Plato we feel dissatisfied with the messages delivered to us by external perceptions, for these messages are peculiarly incoherent, full of surprises, and cryptic in their meaning. The mind prefers to behold conditions that expose themselves to leisurely and careful view; to it the changeable external world is a perpetual offense. Thus arises the suggestion that the sensory world may, after all, not be wholly real, for it violates the cherished postulate of permanence.

There are few thinkers today who would go as far as Parmenides in their insistence upon a static quality of the real, but there are many whose attitude is strongly influenced by his famous arguments. To seek general principles and laws behind the phenomena of nature is to pay tribute to the genius of Plato, and the success of science in its reduction of all different forms of matter to a few elementary particles bears witness to the essential correctness of the Greek ontological sense. The matter can, of course, be overdone. Professor William Lyon Phelps, in his charming informal lectures to the undergraduates at Yale, insisted that physics had far less to say about truth and reality than did poetry. And to prove his point he asked them: "Would you now read a physics text that is 100 years old? Of course not. But you still read Shakespeare!"

Perhaps one should ask here whether *being* and *reality*, more particularly physical reality, are indeed the same thing, as we have here tacitly assumed. Without prejudice to ontology, the legitimacy of which will be discussed in due course, let us take the word *being* in its literal sense and withhold from it the mystifying and ominous qualities of its Greek counterpart. We then perceive it to be an auxiliary verb, rather bare of meaning, a verb inflated into a most independent noun. To be *something* is usually comprehensible and definite—but, to be? Perhaps it was in answer to this query that Lewis Carroll invented the grin of Alice's vanished cat. The only alternative to a denial of meaning in the word being is to identify it with reality. At any rate, this will here be done.

The term *existence* will be dealt with in a similar way. Aside from its very legitimate and perfectly definite uses in mathematics and in logic, which are excluded from consideration in this book, existence and reality are here taken to be synonymous.

Before dismissing the Greek ontological admixture in our modern view of reality, must we not commit ourselves as to its legitimacy? How permanent and inflexible does the real have to be if we are to accept it? The complete answer to this question cannot be given at the beginning of our inquiry; it results from a careful study of the methods by which we acquire knowledge of reality. But a few hints may serve the purposes of preliminary orientation. Certainly, we want reality to be more permanent than our fleeting sense impressions: the tree, to be real, must be in front of my window even when I am not looking at it.

On the other hand I acknowledge that the tree, while real, will grow, change with the seasons, and ultimately die. Our knowledge of what is real also changes in time. In fact this knowledge populates the world with entities whose lifetime may be long or short: the Greek elements, phlogiston, the ether, and now the electron and other so-called "elementary" particles—are they to be rejected as constituents of reality because of the transitory role they play in physical theories?

1.3. The Thing-like

Leaving aside such questions and ignoring for the moment the ontological, the static side of the concept under study, we now examine our Roman heritage. Real is that which partakes of the nature of a *thing* as distinct from *thought.* To the unsophisticated the distinction is obvious; to the careful thinker it presents thorny problems, to be dealt with later. At any rate our domination by the thing doctrine goes so far that we of the Western Culture are prone to reject offhand a philosophy which fails to give significance to this distinction.

What is to be meant by a thing or an external object in a critical sense forms the major object of inquiry of this book. But to clear the path let us set down here a few vague and unsystematic thoughts concerning the nature of externals, lest they trouble us later in our more disciplined study. Let us provisionally accept the view that, if there be a class of real things, then whatever assails or coerces us from without must belong to it. For the origin of these actions upon us is independent of our thought and is therefore real in the Roman sense.

But the linguistic fate of the word *res* itself indicates that the situation just described is far from clear. That word soon denies its humble origin, takes on the abstract meaning of *res publica*, and ultimately reverses its original sense in such phrases as Leibnitz' *res cogitans*. These verbal vagaries reflect uncertainties within this idea of the real itself, uncertainties which offer launching places for devastating attacks upon it. Must the real have properties which are themselves real? If not, if some of the attributes or parts of the real are projections from the realm of thought, then the independent, unexpectedly impinging qualities that served to define it are at once drawn into question. Now the dilemma is apparent: no one can reasonably hold that all attributes which characterize even the simplest variety of thing are external and are given by sensory perception alone.

Two lines of evidence serve to corroborate this assertion, one empirical and scientific, the other epistemological. The first notes that external things are divisible, perhaps indefinitely divisible. This, though superficially contradicting the naïve version of the atomic hypothesis, is the seeming verdict of modern physics, which indicates that even reputedly elementary particles can be divided or forced to change their identities under sufficiently energetic treatment. Whether this expectation is borne out by experimentation or not, the fact is that particles of atomic magnitude according to present conception—protons, neutrons, electrons, mesons—are not perceptible in the same sense as the objects they compose, and if present theories are correct, they will never be thus perceptible. In the face of this circumstance we are forced to recognize that the parts of the real are not real themselves or at any rate are real in some other sense. But this concession tends to dissolve the allegedly irreducible quality of whatever it is that assails us from without.

Perhaps one ought not to expect the physical, the spatial parts of a thing to be themselves of the nature of things. Let us see, therefore, what result may be obtained by analyzing its perceptible properties. Here we come face to face with the age-old problem of distinguishing between the primary and the secondary qualities of objects, the former attaching uniquely and significantly to things, the latter being injected more or less spuriously by the perceiving subject. The history of this problem is an interesting one and could be developed most fruitfully by considering what properties men at different stages of science have imparted to their *elements*, the most basically real constituents of the world in the "Roman" sense. Anaxagoras chose size, color, and taste as primary qualities, Empedocles seized upon size, shape, and position, rejecting color and taste as anthropomorphic. From here on the idea of an element associates itself closely with that of an *atom*, whose dominant features remain size, shape, and position.

Newtonian mechanics, in focusing its sights upon the *particle*, a somewhat more general concept including the atom, casts all its emphasis on mass, position, and velocity as essential attributes. The departure from the familiar plane of sensory awareness becomes more and more evident, until finally modern physics is forced even to deny the meaning of such terms as *exact velocity* and *position* in connection with small particles. There is a progressive conversion of primary into secondary qualities which culminates in the quantum theories of the present century, a development which points ominously to a possible state in the near future in which all primary qualities will have been resolved and our description of physical happenings will have become wholly abstract. Whether this will happen one cannot predict. But the tendency, which quite patently pervades the history of scientific thought, should give pause to him who places all his reliance upon the sensory, the external.

The more customary arguments on this point need not detain us long. It is a commonplace, illustrated by the fact of color blindness, that things appear differently to different persons; this removes color from the range of qualities that are objective, and hence presumably from the realm of reality according to this simple version. By continuing the argument, doubt might be cast upon the opaqueness of a stone, the shape of the human body, the very presence of a thing since we know of radiations which penetrate all these, and since we may well imagine beings whose eyes are sensitive to such radiations instead of ordinary light rays. Then there is, of course, the disturbing fact that all features of an external thing are not perceived or perceptible at once and *in situ*, that memory always intrudes itself in substantiating what amounts to a sensory object. We attribute an interior to a stone because we remember seeing a broken one, color to the flowers at night because we have observed them in the daytime,[1] a back to the moon because we know of no physical objects without one—although in this latter instance the situation is perhaps a little

[1] There are exceptions to this rule. President Coolidge, while riding through Detroit, is credited with the following dialogue with an aide:

Aide: "I see they have painted the streetcars in Detroit."

President Coolidge: "Yes. At least on one side."

more complex, and we go even beyond memory in our constructive imagination. Reality, if it is to be built from sense impressions, must at least include with actually present impressions all those carried in memory, if not a great many more.

What is real in the sense under investigation is often more keenly felt than known; the criteria for being a thing are applied intuitively rather than with analytic care. And our standards change appreciably from early youth to adulthood. Within the limited experience of a child, fairies are certainly as real as galactic nebulae in the experience of an adult. The utter rigidity of our idea of the real, its inexorability, are gradually acquired in life; and we may wonder whether this rigidity does not belong to the lifeless crust of habits with which our indulgent and unquestioning mind has incased itself. The cruelly beautiful myth according to which fairies must die when a child ceases to believe in them often creates in the seven-year-old a sort of twilight attitude to existence, in which he feels keen concern for reality and yet is willing to continue his belief in fairies to keep them from dying. Of the blind reliance upon reality which later overwhelms him, of this peculiar faith of the unbelieving, he is not as yet capable. To him, finding reality is partly discovery and partly invention. Can it be maintained that existence can ever be found through discovery alone?

Parmenides and Plato looked for generality and permanence when seeking reality; Lucretius, typifying what has here been none too accurately called the Roman view, attempted to stabilize the real by tying it to the deliverances of our external sense, hoping thus to keep it independent of the human observer. Both views are reasonable starting points; both lead to difficulties; both color our present attitude toward the problem and must be examined.

We shall not burden this book with specific comments on the various traditional types of realism, from naïve to critical. Our stand concerning them will be made clear as we proceed and develop a positive formulation of the problem; to ask all the old questions involves us in some risk of getting all the old answers. But there is one basic difficulty in every one of the known forms of realism which have come to us as the legacy of the thing doctrine, a difficulty which must be noted. It is that the realist cannot avoid having *two* objects when experience is only about one. The act of seeing a tree involves him in a sort of give-and-take between the real tree and what he perceives as the tree. What he calls the two entities does not matter here; nor will it change the situation if he brands one of them unknowable, like a Kantian *Ding an sich.* The ghost is still there to haunt him, and experience as a unique adventure is always present to disown the ghost and to trouble the realist's conscience.[2]

1.4. The Efficacious

Aside from concern for the general and the permanent (Greece), aside from an orientation toward the thing-like (Rome), our idea of existence is dominated by a lively measure of pragmatism. The real and the actual are close together; indeed the German word *wirklich,* though not implying anything like *res,* means nevertheless the same as real. Literally, *wirklich* is that which acts, that which is capable of having an effect.[2] In detail, the meaning of the word is loose, for it fails to signify whether the effect is to be on another object or on the mind. What is not real in the Roman sense may well be real in this. An idea is pragmatically real inasmuch as it may have important effects; God, according to this version, is real to the person who believes in Him.

Although this formula appears at first to have no application to *physical* reality because it opens the floodgates to unprincipled speculations, a little reflection will nevertheless show it to be of crucial significance in science. Why did the chemist of the eighteenth century believe firmly in the physical existence of a heat stuff called phlogiston? Why did the luminiferous ether function so long as a physical entity? Why do we now believe in an expanding universe? The simplest answer is in each case: Because we observe its unmistakable effects. In fact every entity that cannot

[1] Whitehead ("The Concept of Nature"), arguing against the bifurcation of nature, puts the matter well by saying that on the view here criticized, "There would be two natures, one is the conjecture and the other is the dream."

[2] This is at least the modern understanding. The word *wirken* itself originally meant to weave.

be directly observed—and such entities are more numerous in modern science than is commonly believed—owes its existence to an application of the pragmatic definition of reality which looks to workability.

Our culture has built a shrine to the real, a shrine supported by the three pillars described: one signifies the constant and permanent aspects of experience, one the thing-like, the external aspects, one symbolizes the practical, the efficacious. Among them we are accustomed to worship, though often without much discrimination; for the hold which the real has upon our minds is strong indeed, and whatever is settled before its altar is incontrovertible and final. For most of us, there is no higher instance of appeal.

The role played by reality in our thinking, in our lives, is indeed an important one. Many, perhaps fearing the fate of Lessing's youth who died in contemplation of the goddess of truth whose veil he had the temerity to lift, are loath to meet reality face to face, at least in an irreverent attitude. Let us dispense with reverence and with the genteel prejudice engendered within us by the study of philosophic tradition; let us set reality in the midst of the other problems which concern us today and apply to its study the methods which have proved useful in other realms.

Summary

An appraisal of the meaning of reality, as the word is commonly understood, recognizes three vague criteria: the permanent, the thing-like, and the efficacious in human experience. These criteria are cursorily examined and a number of questions are raised with respect to the adequacy of theories based upon them.

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CHAPTER 2

Ways of Arriving at Reality

2.1. Metaphysics of Natural Science

The analysis of this book will lean rather heavily upon science; to present an apology for the scientific slant is ridiculous at a time when everyone marvels at the material success of science in all fields. It is in fact obvious that science should be pressed to say all it can about any problem which is at all susceptible of scientific treatment. But we shall encounter a strange paradox as we turn in that luminous direction: science will tell us what things are real but will refuse to say what is *reality*.

This need not occasion surprise, for it is well known that scientists, at least in those fields which we call the exact sciences, agree on matters falling in their specific domain but hold widely differing views with regard to reality. Some, like Planck and Einstein, are critical realists, others, notably Eddington and Weyl, are moderate idealists, while Bohr and Heisenberg vaguely display the colors of positivism and rest somewhat indifferent toward our problem. Yet they all would hold that electrons are real and that the luminiferous ether is not. One can practice science without ever committing himself as to reality, without ever using the word *real*; indeed, as a rule, the less said about reality, the better the quality of the science. Within limits, even a solipsist can be a successful physicist.

The reason for all this is, of course, that the problem is not a physical but a metaphysical one, despite the fact that we are concerned with physical reality. To deny the presence, indeed the necessary presence, of metaphysical elements in any successful science is to be blind to the obvious, although to foster such blindness has become a highly sophisticated endeavor in our time. Many reputable scientists have joined the ranks of the exterminator brigade, which goes noisily about chasing metaphysical bats out of scientific belfries. They are a useful crowd, for what they exterminate is rarely metaphysics—it is usually bad physics. Every scientist *must* invoke assumptions or rules of procedure which are not dictated by sensory evidence as such, rules whose application endows a collection of facts with internal organization and coherence, makes them simple, makes a theory elegant and acceptable. Ask an investigator why he prefers a simple explanation, why he hangs his knowledge of the universe upon a *continuous* and undifferentiated reference frame of space and time when his immediate experience is strongly accented by peaks of attention amid valleys of boredom. Ask him why he invokes a principle of cause and effect when his experience presents him with nothing more than temporal succession. He may answer that he seeks the most economical representation of his experience, or that the constitution of our minds imposes such rules for understanding, or that he believes in the essential neatness of creation; all these answers refer to metaphysical convictions, not perhaps of a grandly ontological sort, but certainly epistemological in character. The only answer which carries no metaphysical flavor is that given by the radical empiricist who claims that we *infer* continuity, simplicity, causality, elegance, and all the rest from immediate sense experience; and his answer is palpably wrong, as we hope to show.

We should be foolish to leave science aside merely because it fails to speak directly about reality, for what it says is so strong with significance as to make it worth while for us to examine what it implies metaphysically. Most interesting is the *way* in which it ascertains its truths. In a sense to be made clear by the detailed subsequent discussion, reality cannot be abstracted from finite existence; as it is the number-generating *process* that points to and defines infinity, so it is the *methodology* of science that defines physical reality. A considerable part of this book, therefore, must be devoted to methodology, viewed as a part of metaphysics.

Numerous other treatments have had the same aim, and some of them have succeeded admirably in doing partially what is undertaken here. A new discussion is indispensable, however, because the methodology in at least one of the sciences, physics, has changed so radically of late that a reexamination is urgent, and it happens that the modifications affect most directly the idea of reality. Few have seen this more clearly than the late Professor E. Cassirer,[1] with whom the author had the pleasure and the good fortune often to discuss his views. Human experience is not limited to science, nor do other disciplines possess the same methodology as science. Doubts can therefore arise on the grounds that our final answer, obtained by very special means, may lack relevance in a wider field of inquiry. True, it might; but if our results, though arising at the end of a particular and detailed analysis, point beyond themselves and permit establishment of relations with possible realities in other spheres, as indeed they do, then they carry credentials which recommend them very strongly for acceptance.

One may arrive at a choice of method for determining what is real in another way. Ignoring science, ignoring all organized knowledge as artificial and unrepresentative of immediate existence, one may set himself to the contemplation of any single instance of reality and deeply probe its meaning. Mystics and Hegelians have often expressed the attitude that any particular real carries within itself, and presents to the penetrating observer, all the qualities which constitute it as real. Further, it is claimed, one's judgment becomes insecure if he allows his gaze to wander and rove all over creation, while the mystery to be solved is confined within a single piece of rock or a single flower.

Where this process of abandonment to the particular leads seems to depend greatly upon the temperament of the observer. It convinced Cusanus, who saw in every object a *parvus mundus* in which all reality is reflected, of the cosmic unity of existence, of the perfection of God, and ultimately of the earth's rotation about its axis. It leads Jean Sartre to conclude the insignificance, absurdity, and uselessness of all existence. Precisely because the number of facets of reality exhibited by a particular is small, the conclusions which may be drawn under so few constraints are divergent and variable. Only a flavor of reality, strong but vague, can thus be captured.

[1] See his "Determinismus und Indeterminismus in der modernen Physik," Elanders Boktryckeri Actiebolag, Göteborg, 1937.

What has just been described might, with more fairness to the existentialist, be regarded as only an extreme variety of the more reasonable endeavor to extract from a *limited class* of instances the essence of existence, the essence being what these instances possess in common. Traveling along this road, our destination is almost certain to be the ontological terminal which affords so beautiful a view over the Greek philosophical landscape but obscures other vistas. Properties of different objects, when judged without a *methodological* background, tend to be so flagrantly contradictory that the common logical remainder shrinks to insignificance. Indeed what is common reduces to mere incidence, to that impressive singleness of being which subsumes itself under the greatest of all logical generalizations, known technically as the law of contradiction. Now it is certainly a valid conclusion, and one which must never be lost from view, that the law of contradiction shall rule over the realm of existence; but to say that it suffices to describe it is a patent delusion. What follows is a well-known commonplace, known in logic as the inverse relation between the extension and intension of terms and often illustrated in the exact sciences, namely, that the quest for laws which shall be invariant under a maximum variety of conditions ends with universal principles which, while nearly always valid, say next to nothing about any particulars. We therefore abandon this route, having taken due note of its destination.

It should be said before going on that the conclusion just stated affects in a sense all attempts at probing the real exclusively by logical means. Far from disparaging the importance of logical analysis as a potent device for stabilizing the bases of mathematics, and thus for stabilizing science itself, the author nevertheless feels that logic's preoccupation with classes and sets, that is, with purely static entities within the larger realm of existence, debars it from competence to argue before the tribunal of physical reality, except in so far as it regulates the manner of argument that is permitted. We shall see in what subtle ways the methodology of science employs constructive and imaginative devices and how curiously selective it is in its processes of verification. It is within neither the purview nor the competence of logical principles to give an adequate portrayal of the dynamic component of experience, as is done so simply and clearly by such devices as the laws of motion. We are speaking, of course, of modern logic, not of the generalized and diffusive discipline designated by this term in Hegel's day.

It is argued here that science, when viewed as a living methodological system, will yield the clue to the puzzle of reality. But are there not many sciences, and do they not employ very different methods of procedure? They do, and this admission itself poses a problem, to be dealt with in the remainder of the present chapter. The sciences must be classified according to their methods. If, despite their individual differences, they show signs of converging toward some common basic procedure; if furthermore this basic procedure includes as special cases the ways in which we instinctively certify objects of our experience to be real—then one can hardly deny the importance of such procedures for our problem.

2.2. Sciences vs. Humanities

To be sure, there is no agreement among philosophers or even among practicing educators as to the disciplines that are truly sciences. Few will argue that the study of literature is a science or that physics is not a science, but disputes arise when this question is raised with regard to sociology or history. Such disputes may rise above the fruitless quarrels over terminology and attain considerable importance, as in the question whether ethics is, ought to be, or cannot be a science.

Usually the sciences are distinguished from what is sometimes called the humanities, sometimes the liberal arts. Two misconceptions seem to be current about this distinction: one is that it is rooted firmly in the history of our culture and our schools, the other that it is obvious from the methods employed in the two fields. The second of these we shall attempt to remove at length while examining the procedures of the sciences; the first, being not very relevant to our query, will be passed over briefly, though we cannot leave it entirely unconsidered.

Historically, the liberal arts signified the secular part of the curriculum taught in the medieval *scholae*, the church schools founded by Charlemagne in the eighth century. When this curriculum became standardized it was divided into the so-called *trivium* (logic, grammar, rhetoric) and the *quadrivium* (arithmetic, geometry, music, astronomy); the latter included the one natural science of that day, astronomy. No separation of science as such was at all apparent until the Renaissance, when through a rather extraneous set of circumstances a temporary division was imposed.

Scholasticism flourished in monasteries and in schools attached to the castles of the knights. When cities developed and the crafts arose, a new class of society gave birth to a new secular knowledge of great power. Craftsmen of superior skill made inventions, perfected and standardized their art. These men, who spoke the vernacular, whose urban life and interest set them apart from the Scholastics both geographically and culturally, met with the haughty indifference of the Latin-speaking clerics in the scholae. They included men like Leonardo da Vinci, whose unpolished diaries, written in the popular language, elicited little interest in their day. Among these unrenowned craftsmen were the architects of great cathedrals; their obscurity among the teachers of the time accounts for our ignorance even of their names. Among them were the anonymous European inventors of the magnetic needle and of gunpowder. Empirical natural science was an offspring of such unknown parents; its early development took place under the stigma of illegitimacy. And this is the fact which many of our humanists today will not forget.

History was fair, however, for it corrected its error quickly by according to natural science the dignified status of natural philosophy. Scientists themselves gratefully acknowledged this gesture and transferred part of their interest to the philosophic and cultural foundations of their disciplines. Unfortunately, this trend was reversed when, in the present century, the extreme specialization of scientific research forced a partial abandonment of this interest. Certainly this is no more than a temporary aberration, humanly understandable in view of the dazzling successes of many concrete sciences, and already painful to the conscience of the scientist. But it keeps reminding the humanist of science's lowly rebirth and obliterates from his vision the long centuries of harmonious cooperation. Perhaps this bias is also aggravated by some slight resentment at a peculiar exchange of tongues: today it is the humanist who uses the vernacular while the scientist speaks the esoteric, the mathematical language.

But enough of complaint! Let it also be said that the situation has another side, that scientists are often ignorant of the philosophic and indeed the social implications of their work, and that they sometimes overstate their competence with blatant self-delusion. This bias is known and widely criticized. What matters here is that history, seen in the large, provides no sanction for a conflict between the sciences and the liberal arts. Let us now turn to the sciences.

2.3. Classification of the Sciences

Strong voices perpetually remind us of the essential unity of the sciences; they make us wonder at times whether anything useful can be gained by an attempt at classification. The French Encyclopedists of the eighteenth century, the proponents of the Unified-Science movement [1] today, tend to minimize distinctions, concentrate attention on, and glorify the certainty of, the results of science. To them, all research worth the name is a grand endeavor toward achieving reliable answers to meaningful questions, with no methods favored and no holds barred. Common aspects in the work of chemists and physicists, for example, reveal themselves not so much in how these men proceed but in the acceptability of their products. The correct form for presenting the unique features of all sciences and the adequate exhibit of their unity is, in Neurath's view, the encyclopedia.

Much practical good can come from careful cataloguing of reliable knowledge, but the pretense of ultimacy implied by the movement, whose adherents are with few notable exceptions not scientists in the strictest sense, must be exposed as spurious. Science is more than a record of results which can be stated with

[1] For a clear statement of the aims of this group, see O. Neurath, Unified Science as Encyclopedic Integration, "International Encyclopedia of Unified Science," Vol. 1, No. 1, 1938.

precision; indeed facts can be so fully certain as to be trivial and uninteresting to science, as most facts are. Perhaps the greatest thrill for a scientist comes not when he has demonstrated a conjecture to be valid but when a departure from expectations convinces him that an accepted theory is wrong. At present the physicist's concern over the unknown behavior of queer mechanical objects, such as weighted rods moving under arbitrary constraints, is very mild despite the prospect that he could calculate their motions reliably, using known principles. Instead he is greatly agitated over such things as mesons, entities which hover elusively on the brink of existence and challenge the power of his *method*. The most interesting work is done at the threshold of scientific certainty, and it is almost true that the facts which emerge above it cease to be of concern. Nowhere, of course, will science excite itself over issues which by their nature are prevented from becoming certainties; but one must not mistake this avowed restriction of scientific activity for its final end.

Characteristically, the proponents of the Unified-Science movement picture science as a kind of surface, as a two-dimensional structure. They are fond of calling it a mosaic, a picture puzzle into which the missing pieces must skillfully be fitted.[1] Such metaphors are woefully weak even in suggesting what the scientist is doing, inasmuch as they describe him as sorting existing alternatives when he is creating novelties in facts and knowledge; they break down altogether when their implications are further pursued. A picture puzzle is done when its pieces are all assembled—a scientific problem is never done. There is a region below every problem which is illuminated and exposed to view by its very solution, and in this region new problems are always found. Investigation goes deeper and deeper into this third dimension, which the encyclopedists conveniently neglect. It will never do to picture science as a two-dimensional surface; by ignoring its depth we falsify its nature. If a simile must be found, Neurath's two-dimensional mosaic should be replaced by something like a three-dimensional crystal, slowly acquiring structure in an infinite, amorphous matrix.

[1] Neurath, *op. cit.*, pp. 3–5.

For similar reasons we are compelled to reject the view, recently expressed by Le Corbeiller,[1] which portrays the evolution of the sciences as a predictable and finite process. According to this view there is only a finite amount of knowledge to be gained in every field of endeavor, and as discipline after discipline fills its measure, mankind proceeds toward a millennium in which all facts are understood, all scientific and social problems solved.

Against this prospect stands the fact that never, except in the erroneous comprehension of certain stigmatized epochs of history, has a field of endeavor been regarded as closed. The renowned presumption of the physicist at the end of the nineteenth century, who thought his job essentially done and resigned himself to computing the constants of nature with greater accuracy, was shattered by the discoveries of the last decades, not to be tolerated again. Men wholly uninterested in the pursuits of science saw this point; even Kierkegaard wrote, "With everyone engaged in the 19th century in making things easier everywhere, someone was needed to make them difficult again." If the perfect state of society can be realized only when natural science has run its course, as Le Corbeiller seems to affirm, our hope for it is surely a vain one. Natural science will go on forever.

2.4. Marxism and Science

The founders of communism [2] have shown much preoccupation with the sciences and have had a good deal to say about their organization. Maintaining a focus on social matters they felt, as did the early positivists, that all activity must be judged against the background of the economic system in which it develops, and particularly against the economic needs of society. When applied to the sciences, this judgment brings into clearer view many historical facts, many interesting motives for discovery which

[1] P. Le Corbeiller, Man in Transit, *The Atlantic Monthly*, May, 1947.

[2] F. Engels, "Dialectics of Nature" (Translated by C. Dutt), International Publishers Co., New York, 1940.

V. I. Lenin, "Materialism and Empirio-criticism" (Translated by A. Fineberg), Foreign Languages Publishing House, Moscow, 1947.

See also Sidney Hook, "The Meaning of Marx: A Symposium," Farrar and Rinehart, New York, 1934.

previously tended to be forgotten. It has much merit in broadening popular appreciation of scientific research. But the socioeconomic picture presents hardly more than a historical panorama, not a classification in keeping both with the specific contents and the methods of the various sciences. To provide this, the Marxists fell back upon the Hegelian dialectic.

Their procedure is well illustrated by J. B. S. Haldane.[1] He shows by numerous examples how science rises from one level of interpretation to another by three steps: asserting a thesis, negating that thesis, and finally providing a synthesis. In early mathematics, for instance, the dialectic process is evident in the discovery of rational fractions, which was followed antithetically by the discovery of irrational numbers. From the apparent conflict between the two notions there arose, much later, the embracive theory of the number continuum in its more modern form. The characteristic feature of the dialectic process is the temporary reign of two rival theories, neither of which can be wholly right or wholly wrong.

In physics, such situations arise frequently. Before the special theory of relativity was known, two events were either simultaneous or not. We have now learned that many conceivable events are neither, or both; two occurrences for example, one taking place on earth and one on the sun, with a time interval of less than seven minutes between them, can also be said to be simultaneous [2] because it is possible to find a system of reference in which both occur at the same time. Another physical example is the dualism between the particle and the wave. Thirty years ago the physicist conceived that light, if it be anything at all, was of necessity either a particle or a wave. Now he has performed the remarkable synthesis of viewing both alternatives as partial aspects of a single entity.

Chemistry developed the idea of valence bonds and explained the stability of molecules in terms of them. But it became apparent that a single assignment of bonds to the atoms in a given molecule could not account for the facts of observation, and it

[1] J. B. S. Haldane, "The Marxist Philosophy and the Sciences," George Allen & Unwin, Ltd., London, 1938.

[2] See Sec. 7.8 for further explanation.

seemed as though the theory were false. It was rescued by Pauling, who devised the theory of resonance and showed how, on a higher plane, the bond and no-bond alternative ceased to be exclusive.

As a final example, also taken from Haldane, we mention as thesis the biological assertion, which is undeniably true: Man is an individual. But equally undeniable is its negation in the form: Man is a machine in constant interaction with its surroundings. Here the synthesis is more obvious than in the former cases: Man is both.

The Marxist holds that by frequent repetition of the dialectic, waltzlike movement, science rises to ever higher levels of integration, and he usually arrives at a hierarchy similar to that of Comte. The method is impressive in its simplicity, and what it implies is true. It holds a special appeal for the working scientist because it gives him the comforting assurance that he will always conquer his difficulties, for every antithesis will yield to a synthesis as surely as day follows night.

But a philosophy can be so true as to be without positive content, and we wonder whether this must not be said of the Marxist dialectic view. Despite its apparent adroitness it claims something utterly obvious with a pretense of profundity. To show this we take the dialectic formula to a simpler field in which its triviality can be more clearly perceived. We consider the motion of a point in a plane. If someone announced as a great discovery that *the point may progress in nonlinear motion*, we should hardly celebrate his ingenuity, for it is a trivial observation that motions can proceed along curves. Let us call this statement proposition A.

Now examine proposition B, which says the following: *All motions, unless they are rectilinear, proceed in three steps. The first is toward a certain line, the second away from it, the third again approaches the line but under a smaller angle than the first.* For the present purpose we regard these steps as counterparts of thesis, antithesis, and synthesis. Proposition B seems to be saying something interesting, something rather specific. It may be seen, however, that it is identical with proposition A; it merely succeeds in putting an analytic statement (a moving point progresses either linearly or nonlinearly) in seemingly synthetic form.

We shall show that *every* motion in a plane satisfies proposition B. In Fig. 2.1 we have taken two extreme cases: (*a*) represents motion along a path whose curvature does not change sign; in (*b*) the curvature changes sign. In both instances the motion has been approximated by three rectilinear segments, *p*, *q*, *r*, and a line *L* has been drawn. Proposition B is clearly true with respect to the segments *p*, *q*, and *r* in both extreme instances here illustrated, and a little reflection will show that it is a fortiori valid

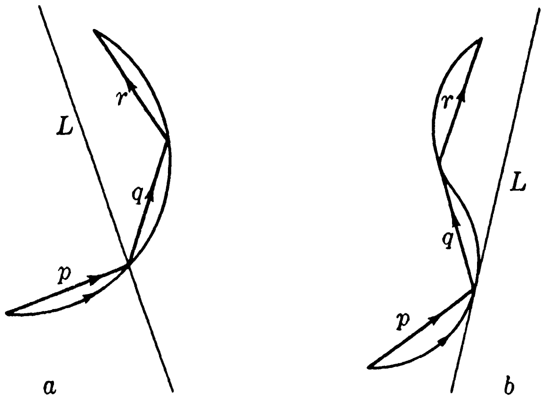

Figure 2.1

in all intermediate types of motion. Hence it is trivial. A line *L* can always be drawn so as to make it true. It is nontrivial, and it ceases to be true, *when the line L is specified in advance.*

Marx and Hegel do not specify the line *L* in advance. Wherever they have to draw it in order to make the dialectic formula true, there is the direction of progress. The only exception occurs, perhaps, in the fields of history and economics, where they forecast a general trend. No line is ever drawn in advance when dialectic reasoning is applied to a problem of natural science. Their whole complex formalism therefore reduces itself to the modest assertion which is the counterpart of proposition A: Science may progress deviously.

Our logical analysis may do an injustice to the emotional qualities of the Marxist view, for it seems common for people to become converted to it on grounds other than rational. The claim is made by scientific men as outstanding as Langevin and

J. B. S. Haldane that Marxist philosophy aided them in their scientific work. In the light of the preceding analysis it is difficult to see how the dialectic principle can accomplish this except through an indirect effect upon their social attitudes. After all, the fact of death is a commonplace, too, but contemplation of it can make an important difference in a person's life.

2.5. Comte's Classification

One of the most notable classifications of the sciences is contained in Auguste Comte's "Cours de philosophie positive." Based on a powerful and sweeping analysis of human history, Comte's theory envisions a "natural hierarchy," among the special disciplines, whose development parallels that of all human thought. Mankind has gone through two stages, the theological and the metaphysical, and is about to enter its third and last, the positivistic stage. The first is characterized by a belief in supernatural agencies and engendered fetishism and theistic religions; the second seized upon abstract ideas in its attempts to explain events and gave rise to philosophic systems. In the positivistic era man contents himself with perceiving the relations between phenomena and formulating them in an invariant way. His interest is no longer reflective but utilitarian: "voir pour prévoir, prévoir pour prévenir." In a similar manner the sciences proceed from the general and formless to the specific and the articulate; their logical structure is reflected in their history. The basis of the system is mathematics; going upward, one encounters astronomy, physics, chemistry, biology, and finally sociology, the science founded by Comte.

It is a striking but reassuring fact that this arrangement, which recommends itself at once as complete and natural, could have been made without the benefit of Comte's philosophy and perhaps without reference to history. For it is also in accord with the degree of subtlety or refinement which prevails in the various sciences. And this is probably the more significant observation. Somehow, generality of application seems always to involve refinement of procedure. Whether mathematics and physics enjoy greater refinement than the other components of Comte's natural hierarchy because they are more general, or whether they are more general because they are more refined, need not be decided here; at any rate they are not outmoded, as the positivistic doctrine would require them to be. With this shift of emphasis from content to structure we have come away from the philosophy, good or bad, which underlay the classification, and again we are facing the question of method. What, then, are the differences in the method or methods practiced in the various sciences?

2.6. Correlation vs. Theoretical Explanation

By methods we do not mean primarily the experimental procedures in vogue. Important as they are, they can be studied only against the conceptual background from which they arise and from which they draw their validity. The techniques of organic chemistry have no meaning at all to the person who does not know or accept the theory of molecular structure; to study the techniques by themselves with the aid of laboratory manuals is a necessary but only a preliminary task. It seems idle to dwell on this truism—and yet our whole society suffers from a lopsided appreciation of external techniques which culminates in the veneration of gadgets. Experimental procedures are basic theory plus skill. We propose to inspect carefully the first component, though always in its relation to the second.

One simple thing can be said about all sciences: They collect data, and they compare. Data are quite different in different sciences; what is meant by them is very much in need of clarification (see Chap. 4), but we assume here that the term is understood. The process of comparing, on the other hand, is thought to be much the same everywhere. The sociologist connects one datum, *e.g.*, church attendance, with another, *e.g.*, incidence of war, and finds that one often accompanies the other. The physicist "compares" the appearance of lightning in the sky with the occurrence of thunder and finds that one usually accompanies the other. This form of sentence can be applied to innumerable scientific activities and may seem, at first glance, to describe them well. In a deeper sense, however, it contains a glaring inadequacy, vaguely felt when the word *compare* is used in connection with such invariable associations as the rising of the sun and the emergence of daylight. Comparison seems to hide a multitude of judgments, and these must be made explicit. It is fashionable to settle the matter by saying that the coupling of data is directly causal and hence invariable, or multiply linked to other data in causal fashion and hence probable, or simply accidental. But this analysis is so superficial as to warrant no further comment, for science, particularly recent science, is full of causes which cannot be classed among the *data* that are being compared.

What we regard as the correct solution of the problem will gradually evolve as this study progresses. Our hope at this point is to make clear, by reference to a few examples, some purely formal differences between the results of comparative acts in various branches of science. This is to prepare the way for the more systematic study contained in the following chapters.

In technical language, the simultaneous or successive occurrence of different phenomena (our previous "data") is called a *correlation*, and this correlation is measurable. For simplicity, we consider the correlation between *two* types of observations, although the study could be made with many. But since we should be far from the heart of science if we did not endow our observations with quantitative properties, we shall at once introduce some quantitative features. Thus, instead of merely noting the coupled incidence of lightning and thunder, we also specify the intensity of the lightning and the loudness of the ensuing thunder. Statisticians have devised numerous ways of expressing quantitatively the closeness of a correlation; such expressions are called correlation coefficients. The rules for computing them from a given set of observations, being familiar to most scientists, need not be stated here. Suffice it to say that there are many such rules, each leading to a numerical measure which is appropriate to certain types of data. We shall employ one which yields the value 1 when the two measured quantities, A and B, are proportional to one another, the value 0 in the other extreme when there is no correlation at all. Thus in the example lightning-thunder, this correlation coefficient would have its maximum value 1 if the loudness of thunder (acoustic intensity, to be more exact) were always proportional to the intensity of the lightning. But since no observations on lightning and thunder are available, we consider others.

The first is admittedly artificial; it is chosen because the result is almost obvious to the reader. Suppose we write in one column the natural numbers from 11 to 30, in a column next to it their squares. Then we erase the first digit of all numbers in the first column, the first and last digits of all numbers in the second column. What remains is two columns of integers, and we wonder whether there is a correlation between the horizontal pairs of numbers.

There is none discernible to the naked eye, but this is no proof of the absence of correlation. When we compute the coefficient, it turns out to have the value 0.104, tantalizingly close to zero. There is almost no correlation, yet there is a little, introduced by the manner in which the numbers were generated.

The next example is taken from Charlier,[1] who presents some interesting meteorological data collected over a period of years. Measurements were made on: A, the yearly amount of precipitation in a certain area in Sweden; B, the volume of water carried by the rivers in that area. If the correlation coefficient between A and B is calculated, it has the value 0.705, indicating quite definitely the presence of some connection.

As a third example, we take a series of accurate physical measurements performed on pressures P and corresponding volumes V of a gas at constant temperature (Boyle's law). On computing the correlation between P and $1/V$ one finds, perhaps to his astonishment, a value 0.9999918. The limit, 1, is approached so closely that speculation might arise as to whether in this last example a new kind of connection, not adequately expressible by means of a correlation coefficient, has not made its appearance. We suspect a "law of nature."

But is a law of nature the mere statement of a reasonably invariant connection between phenomena? It is possible indeed to take the position that science is the establishment of a universal catalogue of correlation coefficients between all perceptible phenomena. This is not an uncommon attitude among statisticians

[1] C. V. L. Charlier, "Vorlesungen über die Grundzüge der mathematischen Statistik," pp. 88*ff*., Lund, 1920.

and is rather close to those taken by Mach, Mill, Pearson, and the Vienna circle. We should acknowledge at once that it is logically unassailable, and, being a minimum pronouncement, it represents by all odds the safest view. But it does violence to certain sciences.

What we hope to show is that there are some sciences for which this view is proper, others to which it is foreign, and that the former are constantly striving toward the state of the latter. In accordance with this situation we distinguish between *correlational* and *theoretic sciences*, or, better, between correlational and theoretic procedures within science.

Investigators bent on basic explanation are never satisfied with a statement of correlation coefficients. Their reaction to the discovery that, in our last example, the coefficient differs from 1 by less than 0.00001 is one of curious consternation; they feel the urge to probe more deeply, to *derive* this strange uniformity of experience from principles not immediately given. A complete understanding of what in fact the workers in all exact sciences do is the central problem of today's philosophy of science.

Even at the risk of belaboring this point we wish to illustrate it in other ways. It is well known that the Egyptians, long before the time of Pythagoras, knew and used the three-four-five rule in surveying the land of the Nile valley. This rule expresses the knowledge that the hypotenuse of a right triangle whose legs are three and four units in length has a length of five units; it is in its further implications practically equivalent to the Pythagorean theorem. Yet we pay homage to Pythagoras' mathematical demonstration. (We ignore for the present discussion the historical evidence, which indicates that the theorem in question was known to the Hindus and to the Babylonians before Pythagoras proved it.) Why should it be so important to devise a proof which adds nothing to the empirical knowledge already available? What distinguishes the Greek philosopher from the careful observers in Egypt? The answer is: Through his act a *theory* was born; the surface of mere correlation was broken, subsurface explanation had begun. To put it another way: The contingency of correlation had given way to logical necessity. This statement remains significant even if it is recognized that the necessity is not absolute but hypothetical, dependent on the validity of the Euclidean premises.

Scientists always hail the discovery of subsurface connections with an acclaim that would be uncalled for if the discovery served no purpose beyond augmenting and organizing empirical knowledge. Nor do such connections merely provide a useful mnemonic device for remembering facts. This was done perfectly well, at the beginning of our century, by *Balmer's formula*, which allowed an accurate prediction of the frequencies of the lines in the hydrogen spectrum. By inserting successive integers into this mathematical expression the frequencies of all known lines could be calculated with ease. Yet it was Bohr's proof of the Balmer formula, in 1913, which impressed the physicist as peculiarly brilliant and as settling the problem. Again, in this proof, a theory of the atom was born. An internal luminosity suddenly shone through the empirical formula.

The social sciences and economics are as yet not illuminated by numerous instances of creative proof. Psychology exhibits a few, such as the Weber-Fechner law, and is becoming increasingly aware of their importance; biology, particularly genetics, is rapidly turning theoretical, and physical science has availed itself almost throughout its history of opportunities for rational proof. It is true that Aristotle's science of mechanics, with its distinction between natural and violent motions and its consequent definition of force, was largely correlational. Not until the times of Galileo and Newton, who seized upon the concepts of mass and acceleration, did mechanics become a truly theoretic discipline. Historical evidence indicates that all sciences start upon the correlational level and evolve progressively toward the theoretic stage. At any given stage, no science is entirely correlational, and none is entirely theoretic. Nevertheless, if there is a significant methodological distinction, it is this, that a science is either predominantly correlational or predominantly theoretic. In this sense one may call parts of biology, psychology, and economics correlational, while mathematics, physics, and chemistry are theoretical sciences.

We now owe the reader a more accurate account of what constitutes the latter type of discipline, for within its structure lies the key to physical reality.

A theoretic, or deductive, science does not move wholly in the thin air of reason. It receives its validity by constant reference to what is empirically given. The kind of knowledge to which the term *empirically given* may be applied will therefore be subjected to scrutiny in the following chapter.

Summary

The vague and conflicting indications encountered in the first chapter seem to attain precision within science. But what is science? To what extent is it more than a collection of specific pieces of knowledge? In answer to these questions we have searched first of all for the distinguishing marks which separate the sciences from the humanities, and we have found them largely wanting. The widely advertised distinction is seen to be little more than an historical accident.

Attempts are then made to classify the various bodies of knowledge commonly known as the sciences. In this process attention is directed to the view represented by the Unified-Science movement, the Hegelians and the Marxists, and finally the Comtian positivists. The conclusion is drawn that each of these views casts illuminating side lights on the meaning of science, but all miss the heart of the problem.

In Sec. 2.6 we endeavor to show now an analysis of scientific method, in contradistinction to a survey of subject matter, confers upon certain sciences a measure of uniqueness. Some proceed by establishing *correlations*, that is, by comparing the incidence of data of different kinds. Correlational procedure is analyzed with reference to several suitable examples. At a certain stage of development, some sciences have abandoned the correlational method and have embraced the use of theoretical hypotheses, from which facts can in a certain sense be *deduced*. As a result, these sciences have become *theoretical*, exact, or deductive. No science is wholly correlational or wholly deductive, but the character of a given discipline may partake predominantly of one method or the other. The significance of the transition from the correlational to the theoretical stage is illustrated by examples.

Inductive logic has an important place in the former, deductive logic in the latter stage.

There is discernible in the history of all sciences a trend toward the theoretical method which seems to give substance to the assertion that it is a more perfect and ultimately the more desirable methodology. Accepting this clue, our discourse now turns to an examination of the epistemology peculiar to the theoretical sciences.

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CHAPTER 3

What Is Immediately Given?

3.1. The Scientist as Spectator of the Given

To most scientists and to many philosophers, what is immediately given is not a matter for dispute. The desk on which I write, the automobile which is moving past my window, the sun in the sky are objects whose palpable presence must simply be reckoned with. As matters of immediate sensation, they are sources of knowledge, complexes in memory, origins for trains of thought. For most purposes it is quite proper, indeed it is useful, to restrain reflection when it seeks to pass beyond this commonplace; not only does our normal attitude toward our daily tasks involve its unquestioning acceptance; much of science is based on it and proves successful—notably the science called correlational, which confines itself largely to what is given.

But when we begin to analyze the terms with which the immediate is contrasted, terms like *knowledge*, *memory*, or *thought*, certain difficulties appear. Where does sensation end and memory begin? Would mere thoughtless apperception result in our seeing the sun as an external object, or would it yield nothing but a vaguely shaped yellow patch, unobjective as a toothache? It is our intention now to raise these questions in a tentative and general way, only to transform them later into more specific problems to which science in fact gives some answers. Modern physics is *based* upon a specific answer; it cannot even be understood coherently unless the answer is always remembered. The questions just asked make it desirable that the nature of the immediately given be carefully inspected on philosophic grounds; more importantly, certain phases of modern science such as atomic physics force this task upon us as a necessary condition for all pursuits.

To state in advance what we shall find: it is that traditional science has been much too liberal in investing the immediate deliverances of sense with many significant qualities, qualities which according to the modern picture they do not in themselves possess and which quantum physics had to take back. We shall have to recognize that sensory fact is born a beggar and does not carry a silver spoon in its mouth.

The simplest and also the most common way of describing scientific activity is to picture the scientist as an observer, standing in a universe and surrounded on all sides by perceptible and perceived matters of fact. He is part of the universe, and the universe is not part of him. He is indeed but a minor part of the universe, and the universe can exist without him. His removal would affect the universe but slightly, would certainly not obliterate it completely. There will, presumably, be other observers who can attest to its permanence. And as to the existence of the universe when no spectator is immersed in it, most scientists would affirm it, though some would regard the question as meaningless. A slight embarrassment arises when the proponent of the view that asserts independent existence is asked to specify the boundary between the spectator and the remainder of the universe. He will then have to admit that most of his body belongs to the remainder and, upon being pressed, he will surrender his entire body to it. Thus he is driven back to the citadel of mind as the only truly spectatorial part of the universe. Mind, or ego, then appears as a singularity in an otherwise regular and continuous structure.

The strength of this position comes from its complete sanction by common sense. Common sense is the popularly accepted residue of scientific method, and hence the evidence for the spectatorial doctrine lies in the circumstance that we have been able to build physical science upon it. Its validity is indeed contingent on the success of science thus formulated; if science begins to fail because of it, the doctrine in question must then be abandoned.[1]

[1] Its abandonment may perhaps entail a further noteworthy change: it may eliminate the singularity, mind, and thus relieve the philosopher of the mind-body problem.

Let us see in what sense the view has produced a successful epistemology for science and in what sense it has failed in this respect.

3.2. The Mechanistic View of Nature

The development of the spectatorial doctrine has gone hand in hand with the rise of one special branch of physics, mechanics, and is indeed its logical correlate. Early Greek inquiries into the properties of *substance*, condensing themselves during the Middle Ages into the more special concern over the meaning of *matter*, led to the epoch-making discovery of Newton's laws of motion. The very generality of these laws, the accuracy of their predictions were taken as vouching for the universality and ultimacy of the entities which they relate, namely, mass particles. Matter was conceived as that which possesses mass and the whole universe as made of mass-bearing matter. The spectacle envisioned by Newtonian physics is one in which masses move in absolute space and time and are capable of being beheld by mind.

The history of recent physics has detracted considerably from its simple splendor. The majestic repose of absolute space, making the motion of particles within it so impressive by contrast, became difficult to maintain; and what first appeared to be a stabilizing background for motion transformed itself into an uncertain and flimsy attachment to the moving masses: absolute space became relational space and now owes its existence to bodies. Even so, the universe is still full of moving masses, but the stationary background is gone and with it somehow the great frame. To see whether one mass moves the spectator must now look at another mass as well, for only relative motion is significant. These curious encumbrances of mechanistic nature, though not recognized fully until the relativity theory cast them into bold relief, nevertheless harassed the Newtonians in the earlier days. The writings of Euler and Kant give clearest evidence of the quandaries into which they were thrown by the relativity of the space of mechanics.

Then the concept of mass, too, began to lose its basic significance. Electrical charges were found to be frequently associated with mass particles, and at the turn of the century all mass was thought to be electromagnetic in character. Nowadays, with the discovery of neutrons and photons, which have mass but no charge, the possibility of this reduction must be denied, and mass (energy) takes its place as an equal beside electric charge. Whatever will be the verdict of modern research on the mass-charge problem, it is already certain that the moving stuff on the stage of mind is much thinner than Newton's masses, which have been resolved into whirling essences with a liberal admixture of empty space. None of this, of course, seriously indicts the spectator-spectacle drama as the basic representation of science; it merely suggests that all is not well on the stage. Recent physics, however, presents arguments which are detrimental in a more serious way.

3.3. Mechanism in Need of Repair

The picture of a spectator (perhaps a mind) embedded in an objective universe bears a strange analogy to one theory of modern physics, namely, electrodynamics. The physicist has for a long time tried to understand the interaction of two charges by letting the first charge produce a force which causes the second charge to move. His analysis thus involves an ideal separation of a *field-producing* charge from a *moving* charge. We now know this distinction to be a spurious one and to be at best only an approximation. It will be found instructive to examine to what extent it was successful and why it ultimately failed.

We do not suggest that the spectator-universe relation is more than a formal analogue of the relation between the two charges. All we assert is that we have here two situations in each of which an arbitrary distinction is drawn between two parts. In the philosophic situation we run into difficulties but are uncertain as to their origin. In the physical situation we encounter difficulties which are formally quite similar, and we see their source. Here the difficulties can be traced directly to the imposition of the original distinction. And this leads us to wonder whether, perchance, the spectator-universe relation is likewise to be impugned before philosophy can move ahead.

We now consider the details of this analogy.

In electrodynamics, a charged particle is said to interact with other charged particles through a *field.* More specifically, the force of one on the other equals c/r^2, where c is a constant for given charges and r is the distance between the particles. This function, which assigns a value to every point of space exclusive of the point for which $r = 0$, represents the Coulomb field. The point for which $r = 0$, and hence the two particles are coincident, is a singularity because here the force becomes infinite. Now all of classical electrodynamics was developed by assuming the validity of the Coulomb field. It was highly successful provided two conditions were respected: (*a*) The singular point must be avoided. (*b*) One of the particles must remain fixed. Condition *a* means that the moving charge must remain a finite distance from the fixed, or field-producing, charge; the second forbids all questions with regard to the fate of the field-producing charge. The mathematical singularity seals the mystery of the entity which determines the motion of the other entity, much in the manner in which mind conceals within itself the source of its own determination. Coulombian electrodynamics impressed upon interacting charges an artificial subject-object distinction. It thereby succeeded in explaining those phenomena in which the subject charge was fixed and the object charge was moving. But it surrounded the subject charge with an impenetrable barrier to understanding.

In physics, the persistence of this enigma has become intolerable. Recent investigations in quantum electrodynamics make it imperative that the singularity of the Coulomb field be removed, and some of the ablest workers have attempted to do so, though not as yet with complete success. One result is clear: we shall never understand the electrical interaction of several charges so long as they are not all treated on the same footing. Holding one fixed will not do; ignorance with respect to one introduces uncertainties into the behavior of all others. The analogy with the spectator-spectacle distinction will now be clear: The spectator may be likened to the field-producing charge, the spectacle to the moving charge.

It is interesting to note that the asymmetry inherent in the spectator-spectacle relation is giving rise to similar difficulties in the philosophy of modern physics. Later we shall study the *un-*

certainty principle in detail. Here we assume a general acquaintance with this principle on the part of the reader and merely advert to its function in linking the observer with the observation. According to this principle, the act of observation has an important effect upon the observed; indeed the act of knowing has an important effect upon the known.

The philosopher must not shrug this off as self-evident, as a mere example of what he has long suspected, namely, that measurements always involve an uncertainty because of uncertainties in the condition of the measuring devices employed. The physicist would be the first to accept this consolation and resort forthwith to his accustomed methods of correcting for such uncertainties. He has long used the theory of errors. This, however, will not do. The reciprocity between observer and observation is a more basic one and, as will be shown, requires a reformulation of the whole of physical description; it requires, in fact, an abandonment of some very important features of the spectator-spectacle relation. That relation retains validity only as an approximation to a truer description, an approximation which happens to be adequate for the phenomena of our daily lives.

What makes many of us reluctant to yield our roles as spectators is the solid evidence of what is presumed to be "immediately given." Moving masses are seen to move; they are obviously before us in time and space; they occupy definite positions at definite instants of time. In other words, the mechanistic view, which is so closely adjoined to the representation of the universe here under examination, appears to be supported by every disclosure of our senses. Let us, therefore, not be hasty but examine the meaning of mechanism in detail and see whether it will stand the test of scientific scrutiny. But first an apology.

The use of the word *mechanism* in the foregoing account may have seemed deplorably vague. We hope to redeem ourselves for this fault in the next section. An observer watching objects about him need not be a mechanist, since he may invest his world with laws other than mechanical. The example of interacting charges was not strictly a mechanical one. But these differences are hardly important. The mechanistic view in a wider sense is held by everyone who believes himself to be an observer in an independent universe and who locates all events and objects uniquely in time and space.

We shall return to the problem of what is immediately given later in this chapter.

3.4. The Breakdown of Mechanism

The quantum theory indicates that precise location of small objects, such as electrons, is no longer possible, at least not by methods now at hand. In telling the story of what has taken place in physics we start again with Newton's way of describing motion. He assumes that a particle [1] may, at every instant t, be said to occupy a point of space and that the points it occupies hang together in a continuous curve. If it moves along the x-axis, the position of the particle, x, is a continuous function of t; $x = f(t)$. To write this equation is to assert that a precise association of instants t with positions x is meaningful.

We are not going to quarrel about the fact that any relation of the form $x = f(t)$ is an idealization upon experience which is true only in a limiting sense. No finite number of observations can specify exactly the function f; there is an infinitude of functions f, all of which can accommodate a finite set of observations. This knowledge is as old as the science of mechanics and is not embarrassing. For if you wish to reject description in terms of continuous functions in mechanics for the reason that experience does not dictate this description uniquely, then the whole of mathematical physics must be thrown out and recourse must be had to the correlational procedures of the preceding chapter. All description in the deductive natural sciences is based upon this idealization, and the idealization would be objectionable only if science did no more than describe immediate experience, which we deny.

It looks, then, as though Newton and all of classical physics [2] were entirely safe from every possible criticism. This is an erro-

[1] A small, pointlike bit of matter; smallness is necessary in order that questions as to rotation of the body about an axis will not arise.

[2] It has become customary among physicists to regard as "classical" the whole field of physics which antedates quantum mechanics (about 1920). Even relativity is now part of classical physics.

neous inference. For even if the relation $x = f(t)$ can never be completely *verified* by empirical means, it can fail for two very good reasons: (*a*) Observation may directly *contradict* it. (*b*) The assumption that the relation exists may contain inconsistencies when confronted with other facts. The second contingency is particularly damaging if it arises: Should (*a*) occur, one could presumably modify the function f until agreement with experience is attained. But if the relation presents basic difficulties affecting its meaning as in case (*b*), then there is no hope for it. In quantum mechanics we are facing situation (*b*).

An example will serve to illustrate this. If you tie a stone to a string and whirl it, you can see it moving in a circle, and a relation like $x = f(t)$ unquestionably represents our visual experience provided that x now stands for the distance measured along the periphery of the circle. Even if the stone is made to whirl as fast as the hand permits, the eye can follow it and thus verify the relation.

If a body is attached to a rapidly spinning wheel, its motion can no longer be discerned by the unaided eye. Association of exact positions with exact instants of time—which we shall henceforth call *classical description* [1]—now becomes a slight extrapolation upon immediate experience, but one which need cause us no concern. For there are numerous ways in which the body, though it appears as a circular streak to the eye, can be made visible for the purpose of checking its position; stroboscopic illumination, for instance, will do. As the speed of rotation is increased, this method will ultimately fail, but some devices, like high-speed photography, are still available. Even if the body makes a million revolutions per second, photography will "spot" its (more or less) instantaneous position.

What if it goes around 10^8 times per second? In this range of speed there happen to be no means for spotting the body in its motion, at least no means as direct as those considered so far. Also, objects of our ordinary experience, like stones, have not been made to revolve quite so fast, but charged particles in a cyclotron do. If physicists were thoroughgoing operationalists they

[1] This is a well-nigh universal terminology among physicists, who apply the word *classical* to everything at variance with latest theories. The word is already collapsing under its ever-increasing burden.

would have given up classical description at this point because of the absence of experimental devices for corroborating the association of x and t; but they did not do so. They clung to classical description long after this experimental infelicity was known.

This is because the unavailability of measuring techniques was deemed incidental. Being inveterate optimists, physicists hoped that someday means would be discovered whereby x could be measured as a function of t. And in this optimism they fell prey to the complacent assumption that classical description must of course always work. They now know that it violates the laws of nature.

To see this let us leave the uncertain region of speeds which lie just beyond the limit of direct detectability and go down to the atom, where, according to Bohr's "classical" theory, the electron revolves about 10^{16} times per second. Measurements as such cannot confirm this figure. Yet our minds can visualize such motions as easily as they picture the whirling stone. In spite of this freedom of our imagination we could never see the electron. If it were to be seen, it would have to reflect or emit light, and it takes time for light to be emitted or reflected. The "birth period" of an ordinary beam of light [1] is approximately 10^{-10} second, that is to say, it takes a light beam about 10^{-10} second to emerge from its source. In this time, the electron in the Bohr atom would have performed a million revolutions, and the best which light could tell us is that the electron's position is a circular smear.

Here one begins to wonder whether we have no obstetric means for speeding a light ray's birth. In trying to do so one runs straight into a fundamental difficulty, long known in optics but not applied to the point here in question. Reducing the birth period of a light ray means shortening the length of its wave train. We know from Fourier analysis that shortening the wave train involves broadening the spectrum toward higher and lower frequencies. Our electron, however, does not possess enough energy to supply the high frequencies. In other words, seeing the electron in its motion would contradict the laws of classical physics!

Light is not unique in its refusal to tell us what the electron is

[1] This birth period is essentially the "lifetime" of an excited atomic state. See for instance H. E. White, "Introduction to Atomic Spectra," McGraw-Hill Book Company, Inc., New York, 1934.

doing at x and t. Every other conceivable messenger (conceivable within the framework of the known laws of nature) is caught in the same contradiction, and indeed in several others. Among them is the well-known interaction between the messenger and the electron, mentioned in the preceding section; the electron's reaction to sufficiently energetic signals is strong enough to alter its motion; and the message itself would be confused between what was and what is now the state of motion. To summarize: No *conceivable* observation could possibly substantiate the relation $x = f(t)$ without violating the laws of nature. We are facing, not an empirical difficulty, but a serious inconsistency in the very heart of science.

Thus the physicist was confronted with a simple alternative: either to give up the contravening laws or to alter his classical mode of description. Closest inspection of the contravening laws, which include the principle of conservation of energy, left no doubt of their validity; modified laws [1] designed to remove the conflict met with failure. Hence physics was forced to renounce classical description. It is no longer proper to speak of an electron as having a definite position at a definite time. Intuition is deluding us when it insists on the significance of this association. After all, intuition has often deluded us before. Did it not also convince the Greeks that atoms have taste? Does it not somehow suggest that electrons have color? We have accustomed ourselves to dismissing common sense in these instances. We shall also have to accustom ourselves to the thought that particles do not have unique positions in space at specific instants.

Our present purpose being served, we need not continue here with a detailed account of quantum mechanical description, the type now current in atomic physics. This account will follow in due course. But to keep the reader's imagination in reasonable bounds we do wish to guard against certain conclusions.

a. Classical description cannot be saved by merely saying: The electron is an extended body which occupies many different points

[1] See the attempt of N. Bohr, H. A. Kramers, and J. C. Slater [*Phil. Mag.*, **47**:785 (1924)] to modify the principle of conservation of energy and its refutation by W. Bothe and H. Geiger [*Zeits. f. Physik*, **26**:44 (1924) and **32**:639 (1925)].

at a given time. Several models of an extended classical electron have been proposed;[1] none is satisfactory.

b. It is not correct to say that the electron has properties which science cannot comprehend. The evidence lends no support whatever to this sort of mysticism; quantum mechanics continues to describe the electron, and its description is adequate, though different from what is customary, as we shall see.

c. It is often said that all is well, provided that the electron is pictured as a particle for most purposes but as a wave for the sake of discussing its motion. This procedure transfers the difficulties elsewhere and solves nothing.

The lesson to be learned is simple: In basic matters we must discipline our intuition and rely more heavily on abstract thought. In essence, the new analysis ceases to specify x as a function of t; *but it does state the probability* w *that the electron be at* x. Thus it describes the *smear;* if, for example, w is constant, the electron is equally likely to be encountered anywhere on the circle; if w is large in a certain region, the electron is more likely to be encountered there in measurements. The new description implies nothing about the place of an electron at every instant, for it talks about happenings when observations are made.

Suppose w, when integrated over the upper semicircle, has a value $\frac{2}{3}$, its value over the lower being $\frac{1}{3}$. Then we know the following: Let an apparatus be arranged to determine whether the electron is in the upper or the lower half of its path—this can be done without violating laws of nature so long as no pretense is made as to determination of the exact time at which it was there. The apparatus will catch the electron sometimes in its upper, sometimes in its lower arc. Our w function asserts that it will be found twice as often in the upper as in the lower part of the circle. $w(x)$ does, in fact, not even say it *was* there, for it refers only to what is observed. Extrapolations, while not forbidden, are always undertaken at some risk.

The new description is less extravagant than the old; it is weaker in a logical sense. $w(x)$ is implied by $x = f(t)$ but does not imply it. Hence the quantum mechanical probability could be

[1] Abraham developed a theory picturing the electron as a solid sphere; Lorentz assumed it to be a spherical shell. Schrödinger at one time pictured it as a cloud.

computed if classical description were valid; but an exact association of x with t cannot be inferred from quantum mechanical (probabilistic) description. Classical physics, though abundantly fed by our fond imagination, leads an indoor life and is bound to succumb to that vitamin deficiency which befalls theories when they withdraw themselves from observation. This is the accurate epistemological diagnosis.

Fortunately, the ailment leaves the patient at present unhindered in all practical affairs. For when quantum mechanics is applied to the motion of ordinary objects, it can be shown to yield the same results as classical physics. Practically, therefore, the latter discipline retains its lease on life because it is a good approximation to the correct form of description. This accounts for its successes. But it should not be forgotten that in a basic sense a stone obeys the same laws, and is subject to the same kind of uncertainty regarding its instantaneous position, as an electron. But the amount of uncertainty, being inversely proportional to the moving mass, is very much smaller. While we could not localize the electron in its entire orbit, our inability to localize the stone is confined to a region so small as to be quite insignificant.[1]

Modern physics is an indictment of the universal adequacy of common sense. It cautions against too glib an acceptance of the so-called "deliverances" of our senses. And for that reason the foregoing considerations have an enormous bearing on the problem of the present chapter: the character of what is immediately given in experience.[2]

3.5. The Theory of Auxiliary Concepts

The physicist has made his choice in rejecting as basically incorrect the classical description of motion. The philosopher has not

[1] The mass of a two-pound stone is about 10^{30} times as great as that of an electron. While the uncertainty in the position of the electron within an atom extends over 10^{-8} centimeter, the corresponding uncertainty in the position of the stone would be confined to a region of 10^{-38} centimeter and could never be directly observed. See Chap. 18.

[2] Experience, in the terminology of this book, is *all* experience and includes thought, conjecture, and feeling as well as sensation. But the word *empirical* is used in its customary sense as opposed to rational.

always been willing to follow him. The philosopher's reluctance, strengthened by the cajoleries of common sense, is aided and abetted by the physicist, who, when talking as a philosopher, sounds like a classical physicist. The confusion of the actual situation is best presented by a bit of dialogue. A theoretical physicist, having solved a quantum mechanical problem, tells his philosopher friend what he has found. The latter objects: "Good heavens, man, you don't mean what you are saying! We all know that phenomena take place continuously in space and time. Your results cannot be true." To which the physicist replies: "Oh, I see! I'm glad you pointed this out to me. Of course you must be right, for you should know." Then he begins to mince his words to please his friend. But he goes right on solving the Schrödinger equation.

Our philosopher, too, has a second thought. He cannot quite forget the physicist's embarrassment. Hence he invents the theory of *auxiliary concepts*. It holds that science, released from the bondage of sensory experience, no longer describes reality but makes "models" of reality which serve only the purpose of explanation and calculation. They are what the Germans have called *Rechengrössen*, quantities or entities useful in calculating and predicting. No one will deny their usefulness, for they allow us somehow to sneak up mentally behind reality and say peekaboo to it. In its extreme form, this is the view of Mach, who regarded all nonperceptible entities (*e.g.*, atoms) as artifacts, as auxiliary concepts not related to sense impression. Accepting it, we are assured that all is well; the electron together with all other invisible entities of modern physics are figments of the imagination.

This is the extreme form of the theory of auxiliary concepts. It is the most defensible and at present the least widely held. Its more moderate form, which counts numerous adherents both among philosophers and physicists, appeases science by granting that electrons, photons, protons, mesons, having perceptible qualities, are indeed more than *Rechengrössen* and belong to "nature." Then it faces the other way and proclaims that our description of them, deviating as it does from the straight and narrow classical path, is more or less subjective and presumably not final. According to this view the physicist, in abandoning the classical form of description, permits himself a methodological lapse into imaginative territory. He is forced to make it, so the argument goes, because he is ignorant or because his methods for observation are not sufficiently detailed. But the physicist restores his scientific status by at once admitting it and by regretting his lapse. This equivocating attitude deserves no comment but condemnation.

If there is anything characteristic and incontrovertible in the procedures of modern physics it is the fact that quantum mechanical description is *more true to the nature of electrons* than the classical assignment of x to t. Only because they wished to remain true to observation did Heisenberg, Dirac, Schrödinger, and Bohr abandon the latter practice. Anyone who believes the part of physics which accounts for the motion of stones and planets and yet takes it upon himself to criticize the theory of the atom because it is too abstract is guilty of deceit; for he endeavors to enforce upon scientific method the familiar features of his grade-school world, refusing to learn anew. If science is trusted in what it says about the moving stone, it must also be believed in what it says about the electron. Common sense is a most docile thing; it can be trained to regard this and even stranger things as obvious. It never leads, it always follows science.

3.6 The Given Is Internal, Not External to Experience

We started this chapter by conceiving man as a spectator of the universe and recognized how the polarity of the spectator-spectacle becomes an obstacle to ultimate understanding. We saw how mechanism, as the formal structure and the driving power of the spectacle, tended to lose its validity because of its failure to include the observer. And finally we witnessed at length how the character of what the spectator thought he was perceiving, namely, the hallowed space-time-matter complex, degenerated into probabilities for perception. The spectacle has begun to involve the spectator. It is with this background of knowledge that we now face the problem we were so long in approaching: What is the nature of the immediately given?

Let us acknowledge one simple fact: It must be sought *within experience*. It is wholly unwarranted to *start* a theory of knowledge with the ontological premise characterizing the spectator-spectacle distinction. If experience, on proper analysis, invests this distinction with meaning, we are ready to accept it, but even then only as a property of the contents of experience, actual or possible. I do not deny that the tree in front of my window is a real tree—real in a sense to be clarified—a tree which can be seen, touched, climbed, or felled; I refuse to perform the leap from this tree to another entity behind it, an entity which "causes" me to have these experiences. This restraint is proper throughout science, throughout epistemology. Physical science needs no ghostlike world beyond that to which it refers.

The reader may sense here an unfriendly attitude toward metaphysics, a kinship with positivism. Whether we shall live up to this prejudgment is uncertain, for there will be a good deal of metaphysics later in this book. It seems most unwise, however, to make a particularly violent metaphysical assumption, that of transcendental realism, at the very beginning before the investigation gets started, and to let it put an a priori limitation on all procedures. If it turned out that science becomes impossible or difficult, if experience could not provide a stable basis for an objective world from within itself by immanent procedures, then we should be forced to undertake the initial metaphysical plunge. But we hope to show this to be unnecessary.

Whoever wishes to undertake this plunge can still go along with us, but only under one condition. He must not impart uncritically to the transcendental elements of his universe those synthetic properties which our mind is only too prone to bestow upon them: existence in common-sense space and time, substantiality, and so forth. He may accept the Kantian *Ding an sich* if it gives him comfort. The author prefers to *start* his journey without metaphysical impediments and to acquire them en route as needed.

The given, we have urged, is to be sought within experience, not, of course, within mind. Whatever mind may be will emerge from a careful study of experience; it is a fatal error to lift it out of context at the beginning of all inquiry and thus to convert it into an intractable singularity in the field of experience, without use or meaning. The Lockian view with its material and mental substances is a typical attempt to avoid these difficulties; it fails because it creates a dualism, a system in which the difficulty has been made into law. Northrop[1] points out interestingly how Locke's philosophy is a natural sequel to Newton's physics. This gives perhaps the clearest perspective in which Locke's epistemology can be viewed and indicates at the same time its science-bound limitations.

Location is not one of the properties of bare experience, though elements within experience may or may not have location. It is therefore never necessary to say *where* experience is. Nor is there anything *external* to experience, for such a spatial attribute can at best be only a metaphor. However, in saying this we do not surrender what is commonly meant by an "external object" if that term is correctly understood. The adjective *external* as used is in fact gratuitous, added perhaps for the sake of emphasis but not with metaphysical deliberation, and implies a quality peculiar to certain things *of* our experience. We shall call this quality *objectivity*, and we shall indeed find room for it, the rules certifying what is objective in things being a major part of the epistemology here presented. The problem of externality thus becomes the problem of objectivity.

Ability to invest objectivity with meaning is what saves the present approach from landing us in Berkeleian idealism. Berkeley's error was to regard experience as not significant in itself, as requiring transcendental stabilization, which it attained by being the thought of God. For Kant, on the other hand, significance is an essential element of experience, an element with which experience is born and which is attached a priori in different measure to different parts of it. The point we shall endeavor to make is that experience does not come with predetermined significance nor without any significance whatever: significance has to be determined within it, has to be discovered by procedures of which we all are vaguely cognizant and which reach highest precision in the methods of the theoretical sciences. Only in this way can we avoid the difficulty which Husserl raised for Kant by asking: Why should not all clear contents of consciousness be allowed to compete with sense data in determining reality?

[1] F. S. C. Northrop, "The Meeting of East and West," The Macmillan Company, New York, 1946.

3.7. Sense Data

Having defined the field within which the immediately given is to be sought, let us then look for it. What is a sensory perception when it is not construed as a message sent by the unknowable? It is simply an element of experience distinguished from others by its *spontaneity*, by its relative *independence* from the other elements, by its *irreducibility*. Kant's apt phrase, "the rhapsody of perceptions," describes it well. The sensory part of my experience in seeing a tree is the residuum which remains when all rational aspects and all mnemonic associations are deleted from that experience. This residuum cannot be conjured up at will; it can be thought or represented in memory and yet declares itself to be unmistakably different from thought and representation. It can be the source of thought and the terminus of an expectation. Being the irreducible residuum of experience it withdraws itself from rational manipulation, and this is the reason why pure elements of sense perception, such as the blue of the sky as cursorily apprehended, or the fragrance of the flower, or the seen shape of this desk, can never figure by themselves in physical theories. They must be translated into wavelengths, chemical compounds, and geometrical figures: they must be "rationalized" before being scientifically treated. In fact, they must be rationalized before they can be discussed at all. In this sense they are truly immediate, residing on that level of experience which defies analysis, but they are not part of a stratum of significance that transcends experience.

There is another way of putting the matter: Uninterpreted sensory perception is the completely *passive* component of experience. One can of course be passive and yet highly alert. An attitude of expectancy combined with the passive phase may surround sense data with a texture of rational relations, but it does not make them rational in themselves. It is in recognition of this feeling of passivity that language tends to describe sense data in the passive voice: we are being assailed, we are being given—the very word *datum* reflects this tendency. Unfortunately, the hazard of insinuating the presence of an ulterior "giver" is not always easy to overcome; traditional "substance" realism finds an ally in linguistic habits. To avoid the bias to epistemology it might be well to replace the word *data* by *habita*, but we shall bow to custom (while urging the reader to make his own clear decision on this point) and continue to use "data."

Equally seductive is the word *presentation*, which seems to suggest the introduction either of some unknown newcomer politely given to us by nature or perhaps of a poster with a message suddenly held up before us at the end of a long pole, the other end of which is not visible. The only virtue of the term presentation lies in the ease with which it can be related verbally to *representation*, the active repetition of data in memory.

Neither the property of being spontaneous nor that of being passively encountered sets a bona fide sense impression apart from sudden pain, dreams, hallucinations, and optical illusions; the latter are or can be as irreducible, as clear, as vivid as the image of the sun in the sky, and their perception need not be confined to a single person. They are therefore not to be excluded from the class of sense data. Whether or not they are significant is another matter. Significance, however, is not a characteristic indelibly stamped upon the deliverances of sense and therefore cannot be determined by reference to sensory data alone. Strangely, the realm of the immediately given is divided into what is valid and what is not by an appeal to principles which are not themselves furnished by sense. But more of this later.

If the immediately given is defined by the attributes of spontaneity and a certain passivity characterizing its incidence, if it is nothing more than that which, within experience, declares itself to have a character of its own, then we have no right to limit its range to what is called sensory perception in the narrower sense. We must indeed include all its manifestations, the sensations ordinarily regarded as objective (seeing the tree) as well as the affective presentation of awe in a religious experience. The reason why the former are usually singled out as sense impressions par excellence is that science has evolved a universally accepted formalism for rationalizing data, by means of which their objectivity can be certified. For the latter type of immediate experience this has not been achieved on nearly so wide a scale.

Finally we would note that the use of the singular, datum, is not a happy one. What we encounter in purely sensory experience is ineffably complex and multiple; every attempt at dividing it into individual parts is artificial and arbitrary. When we divide the tree into trunk, branches, and leaves, we are not dissecting a group of sense data (tree) into "datums"—we are dividing an object into parts. The denotative immediacy of the sensory tree can be vaguely decomposed into "solid brown round upright," "green moving patches," and so forth, but individuality and singleness cannot be assigned to any part of it.

The foregoing study is far from complete as an account of experience. For one thing it ignores emotional and volitional factors and says little about judgment and action. Occasion may later arise for dealing with them, and our omission thus far is not meant to be detractive to the importance of such factors in experience. In physical science, it is true, they play a lesser role; but they are of greatest aid in linking science with the whole of human activity. Second, our study has been evasive with respect to a rather critical point: *Whose* experience does it describe, yours or mine? It might seem to leave the author sinking in the morass of solipsism, where he calls to himself for help. At this stage the temptation to draw such a conclusion may be great, and I ask the reader to postpone his decision, for later on we shall find unique criteria for discerning the objective from the nonobjective, and there is room in every experience for objects having experience. Meanwhile, I can only hope that I have described correctly a certain limited part of both—your experience and mine.

Summary

Most of the contents of Chap. 3 are matters on which philosophical controversy has nourished itself for centuries. They are elementary and are deemed by many to be below the level of present-day philosophic interest; they raise questions that are not wholly scientific, certainly questions that do not yield to the techniques of modern empiricism. In the center of our discussion stands the query as to what is immediately given in experience.

There is an important need for returning to such questions despite their unpopularity at the present time. Twenty years ago the physicist was disposed to consider them academic and useless, as inviting idle speculations among philosophers. Meanwhile, however, he himself has disinterred the bones of old disputes; his quantum theories have raised again the very issues he thought academic. Quantum theory is meaningless without a clear understanding of what, precisely, is immediately given. For if the physical investigator were undeniably *given* such facts as the position and velocity of particles—to cite a famous example—how can the uncertainty principle deny their observability under any circumstances? If time is given immediately in sensation, how can the physicist make theories that fashion time after abstract mathematical patterns? Modern natural science presents many such challenges to unsettle the complacency of those who thought they had been emancipated from the debates of "school philosophy."

In the vein of these convictions we have examined the mechanistic view of nature, in which the observer or possibly his mind is exposed to the spectacle of external events. It is found that the spectator-spectacle relation is difficult to maintain in the face of the newer knowledge of science, primarily because the knowing subject intrudes itself unpreventably into the objective scheme of things. The theory of auxiliary concepts, which is briefly sketched, will not exonerate the spurious spectator-spectacle distinction, and it becomes apparent that a new start must be made. An analysis of *all* experience is suggested as the correct point of departure. The simplest type of experience, *i.e.*, immediate experience, or sense data, is then superficially examined, and it is shown very briefly how it functions as a terminus for cognition.

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CHAPTER 4

Departure from the Immediate; Constructs

4.1 The Continuity between the Spontaneous and the Reflective [1]

One often thinks of data as kinds of experience which point beyond themselves. By virtue of this transcendent linkage, it is supposed, data enjoy a unique position in the scheme of reality, with a stability and a significance all of their own. This view has very naturally created the illusion of a marked cleavage within experience, between sense data as parts of the immediately given on the one hand, and representation, abstraction, thought on the other. Accordingly, data are easily distinguished from concepts.

On our view, which encourages experience to walk on its own feet and denies it the use of ontological crutches, and which therefore forces us to seek meaning and significance within our own cognitive procedures, the difference between sensory fact and thought is not so apparent. We shall argue that sensation as part of the process of knowledge is not wholly *sui generis* and that a passage from the qualities that signify an act of clear perception to those characterizing pure thought may well be gradual.

Beforehand, however, one possible misunderstanding should be forestalled: In asserting such continuous gradation we are not

[1] The arguments set forth in this section are rudimentary and incomplete. For a careful and competent treatment of all matters within the purview of the present task, but from different philosophical standpoints, see B. Blanshard's two volumes, "The Nature of Thought," and C. L. Lewis, "An Analysis of Knowledge and Valuation," The Open Court Publishing Company, La Salle, Ill., 1947.

referring to psychological continuity only. Psychological evidence is indeed in our favor; but we mean to say that, in addition, an analysis of the immediately given and of rational concepts as methodological parts of the cognitive process, or of logic in its wider sense, will reveal elements common to both realms.

The coerciveness of the given, when not interpreted as the mystic hint of the real, resolves itself into spontaneity and passivity of experience, as has been shown, and these qualities are not wholly peculiar to sensory perception. One may talk eloquently, and in general meaningfully, about percepts vs. concepts; yet the speaker finds himself embarrassed when asked, for example, whether the equality in size of two objects, fleetingly perceived in a casual glance without intent at judgment, is the one or the other. On the one hand, many *concepts* have sensory-empirical aspects because of their reference to the immediately given (indeed this empirical circumscription of thought has been made a basic recognition by British empiricism and by some of its modern descendants), and it is quite clear on the other hand that *sensory data* require concepts for their interpretation. Torn out of its context in experience, the immediately given becomes as grotesque as its counterpart, the rational, has often been when nourished in seclusion. Unless one is careful not to disturb the natural setting of data and thought, one's philosophy is artificial and certainly unrepresentative of science.

Let us then acknowledge what is evident: An act of perception may be heavily weighted on the side of immediacy; I may dreamily or joyfully dwell among the ineffable, loosely integrated aspects of a clouded, sunlit sky. Or it may be pregnant with rational relations, as when I watch the swing of a galvanometer on a scale, expecting confirmation of a causal prediction. Again, a concept may be so abstract as to invite no response from the world of data, as for example the idea of a unitary matrix or a differential operator. Or it may be accented on the intuitive side, as the concept of a man or a tree. We believe that experience can move continuously between all four of these, that there are *typically* rational and *typically* sensory parts of cognition, and that it is wrong to ascribe to mind, as the receptacle of experience, one special faculty of perception [1] and another of reasoning. Certainly, concepts and percepts can in general be distinguished, and we shall continue to regard them as discernible; but they merely form extreme types of activity, or results of activities integral to the process of knowledge.

Most of this activity is in the field of concepts; what is immediately given in sensation lies, figuratively, in a thin limiting layer, or on a limiting plane of experience. We are endowed with the ability to pass from there to any point among concepts, arbitrarily far from the limiting plane. How this is done will be further discussed in Secs. 4.3 and 4.4.

4.2. The Haziness of the Immediately Given

The fact of continuity between the spontaneous and the reflective elements of experience has an important bearing on logic. To the degree in which a concept approaches the spontaneous, does it incur difficulties of precise explicit definition. A pure abstraction is always easy to define: as an example, the mathematical concept of a group may be considered. Its definition is complete and adequate, for there is never any doubt as to whether a set of quantities forms a group. Denotatively given things, on the other hand, always elude precise definition. When they are to be incorporated in a logical system, either they function as undefined elements or else generous allowance for uncertainties must be made. If a dog be defined as a four-legged mammal with numerous other characteristics, all of which are specified, then a difficulty arises every time a three-legged dog is encountered.

There is this inherent logical diffuseness in all parts of the immediately given, *and it is important that this be clearly recognized.* Because of it one may deem it questionable whether formal logic can ever ingress far enough into the spontaneous elements of experience to give a satisfactory account of them. At any rate it

[1] The term *perception*, when used without qualification, is intended to be synonymous with sensation, sensory awareness, and the like. It may designate the content of the perceptive act as well as the act itself, both being aspects of the same experience in the view we advocate. For finer distinctions, see Blanchard or Lewis, *op. cit.*

is obvious that an epistemological investigation like the present one need not pause, in fact cannot undertake, to define exactly what it means by such terms as *percept* and *concept*, the *spontaneous* and the *rational*. We shall encounter this penumbra of meaning again in another context (cf. Chap. 6).

If the view here stated is correct, it must have important effects upon the meaning of reality, and it allows an important lesson to be drawn. Spontaneous experience is richer than logic, to be sure, but it is also richer than language, which is a primitive form of logic. The rational can be adequately symbolized, either by ordinary language or in some other way, but the immediately sensed loses its fullness upon expression. Again the metaphor of a penumbra comes to mind. The process of translating experience into language may be likened to the projection of the shadows of objects upon a screen. A point source of light casts sharp geometrical shadows, a broad source surrounds each shadow with a region of haziness. It is as though the source of illumination increased in size as we proceed from reflective to spontaneous or sensory experience.

We may now properly judge the transition from meaning to language to logic. Something vital is sacrificed in every one of the steps involved, and the loss is greatest in the field near perception. That is why logical positivism, in so far as it restricts itself to an analysis of scientific language, can never do complete justice to science; it must forever talk about propositions, where the scientist concerns himself with meanings that are prior to propositions.

4.3. The Passage from Data to Orderly Knowledge

At this point the reader may perhaps reach a conclusion most unfavorable to the objectives of this book, namely, that the author's position, in so far as it differs from positivism, differs by his embracing mysticism. And it may not seem worth while to go on reading an argument if its writer openly admits being hazy about the definition of some of the terms he uses. But here we ask the reader's indulgence and beg him to consider two things: first, that the frank acknowledgment of the existence of logical uncertainties, grounded in the nature of experience and not injected by carelessness, dispels rather than creates mysticism; second, that the reader, to the extent to which his experience is richer than our language, is able to reconstruct from hazy outlines the sharper contours of meanings. Thus it is quite proper for us to assume that we know what a dog is even if we may not be able to define him, that we understand the difference between an ass and a horse although the mule may baffle us. Similarly, we are justified in taking for granted the difference between the rational and the immediately sensed in our own experience despite the existence of regions where they merge. And, above all, it is still significant to ask how we get from one to the other. It is this passage which has recently worried Eddington and many others, who found it difficult to make sense out of the physicist's assertion that a physical object, as revealed by *sense*, has constituents (atoms, electrons, latent photons, etc.) whose properties are described by *mathematical abstractions*. Here lurks a problem of which traditional philosophy has not always been wholly aware.

The passage in question, while not abrupt, nevertheless has its starting point amid sensory awareness and ends among items of orderly knowledge, thus correlating fragments of experience [1] of different sorts. Its simplest form is the act of reification, which associates with the various ephemeral aspects of the visual tree the public object "tree." This process is often erroneously described as a synthesis or an integration of immediately perceived qualities or relations. In fact it is much more than this, for the intentional object "tree" implies an infinitude of aspects not given in sensation. Werkmeister,[2] though yielding to convention in his use of the terms *synthesis* and *integration*, speaks in his most careful passage of an "imaginative supplementation" of the perceptually given. Clear cognizance of this distinction has been apparent in the Kantian and Neo-Kantian school of thought, and much of the emphasis conferred upon this point by that school is now indispensable as a condition for comprehending modern science. It is not our intention, however, to follow the Kantians

[1] As to our meaning of experience, see footnote 2 on p. 44.

[2] W. H. Werkmeister, "A Philosophy of Science," p. 103, Harper & Brothers, New York, 1940.

beyond their clear recognition of an important distinction, toward what appears to us an untenable position of transcendentalism, with its attendant dichotomy of mind and nature.

Let us make sure that we understand the difference between the seen tree and the physical object, tree. The visual, tactile, kinesthetic impressions composing the former are supplemented by two kinds of qualities when knowledge of the objective tree results. The first kind has reference to sensations, though not actually present sensations. The immediately given involves nothing but a few spatial aspects of the tree at any moment: one side of the surface is seen, parts of the surface are touched, the trunk is found to be solid, and so forth. Various aspects can be combined in integrative fashion; we may look at different places and combine in one synthetic impression the different bits of awareness. Whether we say that we have at this stage already passed beyond the evidence of what is immediate is of no importance; to affirm it would indeed be an artificial limitation of the realm of sense data. The transition, as has been noted, is a gradual one. But we do not stop here. We draw on memory and attribute to the tree an interior which is not now seen, an interior which is brown and hard when seen and felt though not now exposed to view and touch. We assume that it has roots which could be made visible by digging, cambium that would bleed if its bark were injured. All these properties might be called integrative since they result from an addition of a multitude of remembered perceptions. They do not, however, constitute the objective tree.

For this is after all the whole of all properties, including those not remembered and those not known. Natorp and Cassirer have often signalized this fact by a linguistic artifact in German which, unfortunately, loses its point in English. An object, they said, is not *gegeben* but *aufgegeben*; it is not "given" in the usual sense of the word but "posed" as a problem. We leave aside for the present all the mysterious implications of this thought-provoking phraseology and focus attention on what it clearly means, on the *conceptual* character of the objective tree. This has brought us face to face with the second kind of quality, previously mentioned, the kind which is more than an abstraction from or integration of sensory perception. This class includes the noteworthy property of permanence, or continuity of existence, which could never be abstracted from data. For a finite sequence of perceptive impressions can supply continuity no better than a finite number of points can generate a line.

The act of reification of data involves more than integrations: it involves *construction*, construction in accordance with rules. Objectivity emerges as a result of this procedure; to assert objectivity is our way of acknowledging the success of the transition from data to the rational wholeness of constructed objects. But this does not mean that we have solved the problem of objectivity, for it is necessary to illuminate the obscurity surrounding the word *success*. We have indicated only that objectivity, if it attaches to experience, is brought into the scene during our passage from the immediately given to what may here loosely be called concepts or ideas.

What these concepts are must be further investigated. They are not, as is sometimes claimed, the *invariant* aspects of sense data. This view mistakes the invariant for the rational, and the rational can hardly be called an aspect of sense. Furthermore, objective things change in a manner somehow conformable to data and therefore can lay no claim to temporal invariance. The other view which should be mentioned as unsatisfactory is one which identifies the object with the sum total of all *possible* sense data, as was done by Mill, Hamilton, and Pearson. For it leaves the matter trustingly in the hands of the unknown, as does every reference to the possible. Aside from being evasive, this view is also false, since the known character of an object determines what future sense data are possible quite as much as these determine the object. We postpone for a while the study of these conceptual objects and return now to the "rules" which led to them.

4.4. Rules of Correspondence

Are these rules unique, and is their verdict inescapable? Do the impressions which normally evoke the idea, tree, always compel this response? Brief reflection here is likely to lead to affirmative answers—longer reflection may, however, negate them again. We wish in fact to show that the rules are not unique. So long as attention is confined to reification, which constitutes the most obvious and therefore the most difficult example of a passage from data to reflective knowledge, the Kantian view of *necessary* correlation (through schemata) is the most natural. But if we look upon this act of reification as an elementary form of a device used under more complex circumstances throughout science, if we view it as the simplest instance of a universal class of relations which join, in a less intimate way, the smell of gas to an open valve, the elasticity of rubber to the shape of its molecules, the blue of the sky to a certain wavelength of light, then perhaps the illusions of predetermination and of inescapability in these relations disappear. That all these relations are of the same type epistemologically cannot be doubted, I believe, for I am struck with the fact that I am doing the same kind of thing when I note the complex of sensa before me and pronounce them a desk as when I look again and associate with its color a certain wavelength. If there is any difference it is the trivial one that I learned to do the one as a child, the other within my memory.

Now certainly there was a time when people had the impression "brown" but did not pass (by virtue of one of our so-called "rules") to the concept wavelength. Nor is life particularly unpleasant for those who do not make this passage now; indeed I sometimes wonder if the physicist does not spoil his own aesthetic enjoyment by indulging too freely—by habit if not by desire—in complicated associations of this kind. However that may be, they are not unique, and, perhaps worse, they change as scientific theories change. If reification is one such rule of correspondence, applied automatically, it must owe its semblance of uniqueness to the *practical* invariability with which it is performed.

Of course we *can* refrain from seeing a light and constructing it into an object, as we do in the dim consciousness of half waking. It is quite possible that in other cultures, among the orientals for instance, the transition is performed in a different way or is not performed at all. Could it be that the Hindu's nirvana, reputedly so unattainable to Western man, is its willful suppression?

Notable is also the fact that we do not apply any such rule to a large part of our immediate experience. We have seen that, as far as immediacy and spontaneity are concerned, many affective sensations are indistinguishable from perceptions. Yet rarely, and never uniquely, do we proceed to construct objective essences as analogues of affective sensations. From the point of view of epistemology the theologian who seeks the "rule" for passing from an immediate experience of religious awe to a divine presence is doing precisely the same as the neurologist who correlates this feeling with a certain condition of the nervous system; and both are attempting what the physicist does when he associates a wavelength with a color sensation. The questions as to which rule works and is finally adopted and whether a given set of data may be translated into concepts by several different rules cannot be answered until we investigate the part played by the concepts.

Against the Kantians we should thus argue that the specific character of the passage from raw data to organized knowledge, so clearly discerned by them, is not grounded eternally in the nature of experience. It has simply evolved. The act of reification is, in fact, the first step taken by our race toward more sophisticated procedures now prevalent in the exact sciences. To see in it and in similar rules the "transcendental conditions for the possibility of all experience," to smuggle in a few synthetic a prioris in the guise of forms of pure intuition along with them, is more hazardous now than in Kant's day, when a sweeping phraseology, liberally interspersed with the mystic and untranslatable *schlechthin* and *überhaupt*, was sufficient to ensure exalted standing for theories of knowledge. Science has made continued inroads into the a priori; it has taught us the danger of forming unqualifiable beliefs about experience. And so, to be safe, we shall regard the rules not only as having evolved but even as alterable.

Throughout the writing of the preceding pages a mild shudder has run down the author's spine every time he used the term *rule* for the manner of passage from data to concepts, since there is no assurance that this usage may not give an altogether unfortunate bias to the meaning intended. A rule is usually something which can be stated, has a range of application, and has content. The relations here in question cannot always be stated except by reference to the terms they relate; they are often simply passages. In a sense, therefore, they have no content of their own. Their range of application is also difficult to specify; we know when the tree rule applies, when the stone rule, or the sunset rule, or the person rule, or the wavelength rule, or the electron rule applies; but this knowledge is often habitual and is clear only in its application; indeed it could hardly be otherwise in view of the haziness of the given. In many respects, therefore, the word *rule* is a misnomer; it was chosen because of its lack of color; we wanted to avoid mystifying the reader even at the risk of temporarily misleading him.

F. S. C. Northrop,[1] in discussing the connection between the "empirical component of any complete object of knowledge to its theoretic component," uses the very appropriate term "epistemic correlations" for these rules. When adopting this phrase occasionally hereafter we shall remember that the correlations do not have positive epistemic content, that is, do not confer validity upon knowledge in and by themselves. They have to be considered within a larger context of method before they become significantly epistemic, and their acceptance is determined by the functioning of the conceptual apparatus which they generate. In earlier writings, the present author[2] has called them "rules of correspondence." Later we shall see that Bridgman's[3] operational definition is a special type of these rules. It seems best at this time to illustrate their use by reference to further examples.[4]

[1] F. S. C. Northrop, "The Meeting of East and West," p. 443, The Macmillan Company, New York, 1946; see also his earlier writings.

[2] *Phil. Sci.*, **2**:48,164 (1935).

[3] P. W. Bridgman, "The Logic of Modern Physics," The Macmillan Company, New York, 1927.

[4] The rules of correspondence might be regarded as a modern, though very specific, version of Kant's "transcendental schemata of the understanding." Similar concepts in the more recent literature are Norman Campbell's "dictionary" of a theory ("The Elements") and Bridgman's "text" of equations ("The Nature of Physical Theory"). The distinction now current between the "syntax" and the "semantics" of a theory (see Charles Morris, "Signs, Language and Behavior," Prentice-Hall, Inc., New York, 1946) also makes manifest the need for our rules of correspondence, which fulfill a semantic purpose. Semantics is in fact the study of those rules of correspondence which join immediate experiences ostensively with the words of a language.

It is thus seen that the function of these rules extends very far. In this book we limit our discussion of them to certain aspects which are of importance in science, leaving aside for example the vast field of linguistics.

Examples of Passages from Data to Orderly Knowledge. When one studies the linkage between the immediately given [1] and its conceptual counterpart in isolation from the totality of scientific method, as we are now about to do, he is bound to be troubled by a sense of artificiality pervading the whole analysis. This is because relations that are merely exhibited necessarily appear *ad hoc.* Hence we beg the reader to defer his final judgment on the appropriateness of our account until he has seen a little more of it. Meanwhile, let us consider five different examples illustrating the points at issue.

1. The simplest application of the rules has been discussed; it is the act of postulating a *thing* in the face of certain *sensory evidence.* This was called reification. That this thing be an external object cannot be certified by the "rules" alone but requires documentation of another sort, documentation which refers to the coherence of our entire experience. We must thus distinguish between a rule that reifies and the larger part of experience that objectifies. The inverted palm tree seen in the sky over the desert is a thing but not an external object in our sense. The reifying rule of correspondence only has been applied to it. We acknowledge that the present use of terms, *thing* and *external object,* is arbitrary, but the distinction intended is not. In this chapter we pay no attention to the validating procedures which supplement the application of the rule.

2. Another passage from Nature [2] to concepts, which is a little

[1] It will be economical and less pretentious if we now introduce a simpler term in place of the "immediately given." We have already used "data." In doing so no attempt has been made to separate those immediate experiences from which transition always is made to orderly, conceptual knowledge (these are usually called sense data) from those which at present do not permit such transitions in a manner acceptable to all (sensation of guilt, sudden pain, etc.). We now collect the former class of experience and call this part of the immediately given, "Nature" with a capital N. Although aware of the fact that this usage is not in agreement with custom—for nature includes far more than Nature—we shall nevertheless select this artifact for its brevity and leave until later all questions concerning the relation between the two. Similar semantic decrees will be encountered as the book proceeds. Our discourse will thus become more convenient even if somewhat strange.

[2] For the meaning of "Nature" as distinct from "nature," see the preceding footnote.

less direct than the foregoing, occurs whenever the postulated thing is endowed with specific qualities of its own, qualities not "read from data." As a case in point we mention the assignment of *mass* to bodies, the act which sets the science of mechanics going. Newton's apocryphal experience in the orchard at Woolsthorpe led him easily to the object, apple; assigning *mass* to the apple, however, was another step. By taking it Newton was enabled to formulate laws of motion, the law of universal gravitation, and so forth. Now it is to be observed that mass is something very specific, not seen or even felt, something that requires careful definition in terms of data.[1] What interests us here is the circumstance that adoption of the idea "mass" again lands us in rational territory where theoretic procedures are possible, which are not possible among the bare elements of Nature. And the way by which we got there is *different* from that which led us to reify the apple. Mass, though not part of Nature, has some intuitable aspects; but it lies somewhat farther from Nature than does the apple.

The intuitable qualities, vague as they are, of concepts such as mass tend to delude us into thinking that they are data. This tendency is strongly augmented by the bluntness of language, which fails to provide different words for separate meanings. There is a way of feeling the mass of an object, as Dr. Johnson did when kicking the stone in order to convince Berkeley of its reality. This so-called "mass" is immediately given; but a glance at a physics text will convince anyone of the difference between it and the mass which the physicist employs in the laws of motion. The same confusion embraces almost all quantities encountered in natural science; force, energy, light, sound are examples. To avoid its annoyance the physicist has found it necessary to use the words *subjective* and *objective*, which illustrate the difference but leave it with a metaphysical prejudice. Let us then be careful in ferreting out the multiple meanings of words.

3. The rule of correspondence between color and wavelength, already referred to, deserves brief examination. On the side of Nature there is the visual red. Certain operations, not unlike the

[1] See, for example, the elaborate definition of Mach, of which a résumé is given in Lindsay and Margenau, "Foundations of Physics," pp. 92–94.

inspective, the tactile and kinesthetic investigations which resulted in the idea "tree," must be performed before the concept, wavelength, arises; a spectroscope with prism or grating must be at hand, the light must be properly collimated, the slit adjusted, the telescope focused until a line appears on a scale. Finally we read the wavelength, let us say 6,948 angstrom units, from the scale. The rule in this case is highly instrumental and leads to a concept which is not very obviously related to its perceptible counterpart. The wavelength itself, of course, is not perceptible even if a line is seen on the scale. But it might be called pseudo-sensible because it admits of sensual representation in the form of the length of other types of waves which can be seen.

4. There are instances in our immediate experience where one body is repelled by another. Under certain circumstances the physicist considers it proper to say that these bodies are electrically charged, and he proceeds forthwith to ascribe an electric field to the surroundings of the repelling bodies. He goes further and associates with every point of space in the neighborhood of the bodies a vector called *field strength* which he denotes by **E**. Its numerical magnitude is the force which would act on a certain charged body if it were placed at the point in question. The field strength is not a force, but a latent force which can be made evident by suitable procedures. Clearly the rule by which **E** is constructed is of a highly complex sort, involving both instrumental manipulations and mathematical processes. Moreover, **E** is not found in Nature and is difficult to visualize; we shall call it abstract and understand this term as the opposite of concrete in the customary sense, not literally as "drawn from" sensory experience.

Abstractness of explanatory concepts does not trouble the modern physicist; in this respect his mood differs significantly from his predecessors' of the last century. Faraday, for instance, whose great contributions to science include the formulation of **E**, found himself unable to accept it as an abstractum and invented the space-filling ether to give it respectability. To him, electric fields were stresses in an all-pervading ether. But we now know from the theory of relativity and from many experiments that respectability, if it is to be construed as the quality of being imaginable through sensory representation, is purchased at too high a price. So the ether was abandoned, and electric fields became physical quantities in their own right, quite apart from matter. The fashion concerning respectability has changed to more formal proprieties, which are studied later in this book (Chap. 5).

5. As a final example of the operation of the rules of correspondence we choose an even more difficult one, and one which emphasizes strongly the point just made about abstractness. Conditions occur in which the physicist asserts the presence of electrons. We shall assume here for simplicity a rather idealized though not impossible situation where he can allow single electrons to be emitted by an electron gun. He wishes to find the position of an electron a certain time after its emission.

Now we have already seen in Sec 3.4. that this position cannot be determinate; hence the use of the word *position* must be acknowledged to be loose. In other respects, too, we have been forced and shall continue to be forced to use common words ambiguously. For instance, the whole description of the last paragraph was indiscriminate in sometimes referring to Nature but more often to concepts already separated from Nature by the application of our rule in various forms. Thus we talked about **E** as related to Nature and summarized this relation by a linkage between **E** and the force on bodies. In fact, however, neither force nor bodies are parts of immediate sensation. Yet we have used these terms in that context as though they were, tacitly expecting the reader to supply the further simple rules of correspondence which make them such. This incompleteness of statement will be apparent in much of the following discussion; it can hardly be avoided, except by adoption of a highly formalized and artificial language. With this understanding we return to our example of an electron's whereabouts.

To satisfy his desire the physicist sets up an electron counter which clicks whenever an electron enters its opening. Assume the opening to be very small and the gun pointed exactly toward it. Then it will be found that an emitted electron will sometimes enter it and sometimes miss it. A person acquainted with modern physics will not think this strange, for it stands to reason that various influences conspire to deflect the electron from its straight-line path. But numerous facts, firmly embedded in the theory of quanta, require us to reject this explanation of the electron's manifest departures, force us to say *it has no exact position.*[1] Hence the physicist repeats the experiment many times, allowing electrons to be emitted from a gun fixed in position but with the counter in slightly different places. He thus determines the probability (or relative frequency, if the reader pleases) with which an electron, whose state is prepared in a definite way, is found at the various points of space. The function φ, defined in such a way that the squares of its absolute values shall be identical with the observed probabilities, is then the *state function* of the electrons thus prepared.

The φ function is a property of an electron just as truly as the blue color is a property of the sky. It is a physical quantity in the same sense as a wavelength or an electric field. And it corresponds to aspects of Nature. But the correspondence, while perfectly unambiguous, is highly instrumental, selective, and refined. The rule in this case takes us from data by a long path into the field of the abstract, but it plays nonetheless the same methodological role as did the rules encountered in the preceding four examples. They take knowledge from bare sensory fact to a domain in which logical processes are possible; for in terms of φ functions we can reason and predict, where no amount of listening to counter clicks would have made this possible.

Notice, too, how another unusual feature has made its appearance. A φ function is related by a rule of correspondence, not to a single event in Nature, but to many. *All* the counter responses are synthesized via the rule into *one* state function, and the synthesis involves the idea of probability. This may seem strange or even objectionable from the standpoint of classical physics. If it does there is but one remedy: The methodology of classical science must be enlarged to accommodate such features.

[1] Dirac's book, "Quantum Mechanics," Oxford, New York, 1930, contains most of the arguments which make this conclusion inescapable. See also J. von Neumann, *Mathematische Grundlagen der Quantenmechanik*, Springer-Verlag, Berlin, 1932. The present discussion is an anticipation and a brief summary of the material developed more cogently in Chaps. 16 to 18.

4.5. Constructs

The examples of the foregoing section were selected with a purpose in mind. They illustrate in their chosen order a progressive widening of the gap which is spanned by the rules of correspondence. In reification we take but a small step toward concepts, in assigning mass we move a greater distance, until finally, in defining a state function, we make a flight of considerable magnitude into the very abstract. We have spoken of *one* rule in each of these instances, but this should not predispose us against admitting the resolvability of any rule into an arbitrary number of simpler rules. In theoretical formalisms, including logic, it is dangerous to count entities, for there is rarely a way of determining what entities are basic. Ideas, concepts, relations do not satisfy the axioms of arithmetic.

If such reduction is performed, it will be found that among the new relations there are some which do not have one terminus in Nature but join two or more different concepts. For instance the relation, counter clicks $\rightarrow \varphi$ function, could be broken up into two: (1) counter clicks $\rightarrow$ electron; (2) electron $\rightarrow \varphi$ function. Of these the first has one leg in Nature, the second none. Whether or not we regard the second as a rule of correspondence is a matter of nomenclature only; we prefer not to do so at present and consider it a relation between entities already made determinate by rules. But it should be clear that any given rule of correspondence, or epistemic correlation, can be arbitrarily extended on the conceptual side by adding relations of type 2.

A rule of correspondence links what has here been called Nature to entities which we have vaguely termed concepts, ideas, reflective elements, and so forth. For these, too, we should now introduce a special name. A particular tree or a particular electron is hardly a concept because of the generic implications of that word. And yet our rules lead to particular trees and particular electrons. Nor can these entities be termed ideas unless one wishes to open the floodgates to misunderstanding. On the other hand they do partake of the character of concepts and ideas by being rational, by submitting themselves to logical procedures in a much fuller measure than do the data of Nature.

To indicate that they are not mere gleanings from the field of sensory perception, that they come into their own through what are felt to be creative processes in our experience rather than through passive contemplation; to emphasize their rational pliability and yet to distinguish them from shadowy concepts, the writer has previously called them *constructs*. Although this usage has justly been criticized as vague and has at times been misunderstood, I am still at a loss for a more appropriate name. Every familiar term is preempted, and I should incur the wrath of my scientific colleagues were I to waste a Greek word on anything less than an atom-smashing device. Hence, with apologies to the reader, I shall continue to use the term.

A construct is not found ready-made. It has many of the qualities of an invention. Here perhaps even the sympathetic reader will shake his head, for it is obvious that tree, mass, electron are more than inventions and hence more than constructs. But we do not deny that they are more than mere constructs. They are *useful*, or, to be much more precise, they are *valid* constructs, valid in a sense to be more precisely defined later. Ghosts, mirages, and the luminiferous ether are also constructs. It is the principal business of a methodology of science, and indeed of all epistemology, to investigate what requirements a construct must satisfy to be admitted as valid, or, as we shall later say, to become a *verifact*. To this task we devote the next two chapters. Let us understand, then, that our designation, construct, is intended to assign to trees, electrons, ghosts, and devils merely their correct genetic status in experience; the character of being a construct does not alone provide an entity of thought with scientific importance.

At the risk of repetition it is necessary once more to consider the already labored point about the difference between concepts and constructs. Clearly, the tree *is* the construct; the unitary experience of the tree is summed up in that way. The tree is permanent exactly to the extent to which permanence has been invested as a rational element in the construct. There is not a tree *and* my construct of it, nor a wavelength *and* my construct of it.

Nevertheless there exists a large area of discourse in which the word *construct*, as we have employed it, is wholly synonymous with concept. Thus, to consider another example, the construct, electron, when used without any qualifying reference to time and space, is also what is commonly called the concept, electron. But when we speak of the concept of a particular electron, that which has just impinged upon a fluorescent screen, we are prone to imply a distinction between that concept and the actual object on the screen. The attitude we have endeavored to sketch wipes out this distinction. The electron, as an external object, is the construct. Of all the words readily available, concept is the closest to what we wish to designate; but a desire to avoid commitment to realism on the one hand and to idealism on the other, and to avoid reopening the problems which such a commitment would involve, has determined the author to continue the use of the less customary term.

Among more modern writers it was Karl Pearson [1] who gave currency to the word construct. However, he employed it in quite a different way and ascribed its use to the biologist Morgan. On page 41 of "The Grammar of Science" we read:

> Owing to the large amount we ourselves contribute to most external objects, Professor Lloyd Morgan, in the able discussion of this matter in his *Animal Life and Intelligence* (p. 312) proposes to use the term construct for the external object. For our present purpose, it is very needful to bear in mind that an external object is in general a construct—that is, a combination of immediate with past or stored sense impressions. The reality of a thing depends upon the possibility of its occurring in whole or part as a group of immediate sense impressions.

The spirit of the foregoing discussion precludes our acceptance of the meaning of the term with Pearson's excess of positivistic seasoning.[2] Trees, molecules, electrons, genes are not compounded from sense impressions, past and present, alone. They also contain rational elements which point beyond all the aspects of immediacy that go into their making.

We hold that *abstraction* is an elementary form of construction,

[1] Karl Pearson, "The Grammar of Science," 3d ed., Part I.

[2] Pearson goes on to say (*ibid.*, p. 66): "The mind is entirely limited to the one source, sense impression, for its contents; it can classify and analyze, associate and construct, but always with this same material. . . ."

and only an elementary form. An abstraction is the union of particulars into universals and occurs at all levels of a science. Construction, in addition to performing this union, endows the product with suitable properties of its own: it is a creative as well as a synthetic act.

The difference can be made clear by considering the ideas of space. The space *abstracted* from experience has very little uniformity; it is a kind of private space, nonhomogeneous, nonisotropic, with a bewildering variety of idiosyncrasies. Indeed it is hardly proper to speak of *one* abstracted space—one should speak of many, a visual, tactile, auditory space, and so forth. *Constructed* space, however, like Euclidean or Riemannian space, towers above these others by virtue of a postulated uniformity which, in a way to be discussed hereafter, both welcomes and in large measure defies the onslaughts of immediate experience. The word *construction* is meant more nearly in Russell's sense.[1] To him, it signifies the replacement of complexes of sensation by *mathematical* relations because of formal similarities between them. In the following it will appear, however, that his emphasis on mathematical similarity is stronger and his conception of the replacement more phenomenalistic than is advocated in this book.

Summary

Experience, in becoming complete and integrated, moves from the sensory and spontaneous to the rational and reflective. By this transition, the elements of the given take on orderly traits and allow reason to take hold of them. Among the peculiarities of bare sense data is a certain logical haze, a tangled connectedness, which defies classification of mere data as individuals. For this reason it is not possible to define datal experiences in a manner other than denotative or extensive.

The passage to orderly knowledge involves the positing of constructs, which are the rational elements to which datal experience is made to correspond. An external object is the simplest

[1] B. Russell, "Our Knowledge of the External World as a Field for Scientific Method in Philosophy," The Open Court Publishing Company, La Salle, Ill., 1914, and George Allen & Unwin, Ltd., London, 1926.

construct which we habitually set over against most kinds of sensory awareness. Others are geometric forms, numbers, and most of the refined entities of modern physics. Invention of a construct does not carry with it the assurance that the construct is scientifically acceptable or that it is part of reality. The conditions under which these qualities may be asserted are subject to examination in later chapters.

Experience moves from data to constructs via guiding relations which are called "rules of correspondence." These are studied in connection with several examples, and the following facts emerge: The simplest rule of correspondence is the reifying relation, the transition from the unanalyzed visual aspects "tree" to the external object, tree. Others are less direct, as for instance the rule which leads the physicist to postulate a wavelength when a certain blue light is seen. The rules of correspondence, it is held, are not eternally grounded in the nature of things, nor are they immediately suggested by sensory experience; they are important parts of every theory of nature and receive their validity from the consistency, the internal neatness and success of the entire explanatory scheme.

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CHAPTER 5

Metaphysical Requirements on Constructs

5.1. Constructs Are Not Wholly Determined by Perception

From the narrow field of the immediately sensed, experience flows along channels marked by "rules of correspondence" into that freer domain of concepts in which logical operations become possible. If the rules of correspondence were uniquely set by sensory experience and if their correct discernment, effected by contemplation of data without recourse to metasensory principles, satisfied our yearning for understanding, then the term *construct* as a designation for the counterparts of data would be ill chosen. If, however, the rules are not in themselves unique and if, furthermore, the rules do not settle whether what they define is physically acceptable, the conative flavor of the word recommends its use as proper. In that case other validating principles, not yet considered, must be at work in the cognitive process. The counterparts of sense are then merely *suggested* by data; they are tentatively offered for examination or provisionally constructed under the guidance of *assumed* rules of correspondence. We shall endeavor to show that the latter is the case.

The history of science is full of instances which corroborate our view. Selecting a particularly obvious one, we recall that the sensory experience "water" was correlated with a geometrical figure (icosahedron) by Pythagoras, with smooth atoms by Democritus, and is at present correlated with the formula H_2O and with all that it implies. This is a paradox if Nature conveys to us predetermined and valid rules of correspondence. Similarly, one cannot follow the development of optics, the changes in our conceptions of heat, sound, electricity, chemical affinity, combustion, the firmament, or our notions about the human soul without being impressed by the multitude of ways in which man has endeavored to link Nature with reason. Nor can he maintain, in view of such evidence, that the method now in vogue has any likelihood of being ultimate.

The situation is badly confused by those who insist that our choice of rules is but apparent, arising from the limitations in our knowledge of Nature, and that there would be no uncertainty and no choice if all possible sensory experience were now at hand. If this were true, one would have to regard the historical process as self-correcting and as converging upon the ultimate rules of correspondence, and furthermore this convergence would have to be regarded as enforced by data alone. Now there may be such convergence, and the historical process may be self-correcting rather than periodic or cyclic or random—this point cannot be established by scientific evidence alone. But the corrective factor, if present, is *not* supplied entirely or even predominantly by external observation; internal consistency, simplicity of conception, and elegance of formulation have at least an equal share in controlling scientific progress. Data alone do not furnish sufficiently stringent conditions to render our understanding unique.

This fact has been recognized and at times exaggerated by modern thinkers. Poincaré attributed so much weight to formal principles, which he regarded as subject to fairly free manipulation, that he termed the selection of constructs in large fields of science *conventional.* Mach and his followers clearly perceived the failure of constructs to be uniquely given by Nature; and therefore they took them to be fictitious concepts, invented for the purpose of economy of thought. Such attitudes do reflect a significant aspect of all constituents of physical theory. But they are in error because, upon recognizing the insufficiency of observations for defining theory, they stop all inquiry and look no further. In the present chapter we shall investigate certain ideal principles which supplement the rules of correspondence in their function of producing acceptable theory. But to show more clearly the need for such principles, let us illustrate further the insufficiency of these rules for generating constructs.

The simplest, and certainly the safest, type of epistemic correlation is one between sense data and the workings of a divine Providence. Experience can never controvert it, and if Nature alone determined theory, this would be the best possible interpretation of the sensory universe. However, scientists reject it; they reject it, not because it fails, but because it fails to satisfy them, and it fails to satisfy them because it is too blunt; it lacks, strange to say, the appropriate degree of metaphysical refinement. The very fact that it can never be shown in error is against it, makes it violate what might be called the spirit of science, the rules of the game. Soon we shall have to answer carefully the question: Why is such a theory unscientific despite the circumstance that its rules of correspondence are simple—simpler perhaps than those which cause us to reify an object—and despite the fact that they cannot miscarry?

A similar though less trivial situation arose in physics at the beginning of the twentieth century. The habit had been formed among investigators to correlate data with mechanical models. Here men were on surest ground, for Poincaré had shown, or was believed to have shown, that *every* phenomenon could be reduced to some form of model. No serious doubt on this point arose until Heisenberg and Dirac appeared; yet models had been abandoned in electrodynamics and optics long before the decisive discoveries of quantum mechanics were made. Clearly, such abandonment was not required by new *experimental* facts; it took place for subtler reasons. And let it be noted that the attitude of the late mechanist, his insistence on models and his preference for intuitable schemes, was itself rooted in a texture of convictions about the world, and not dictated by Nature.

Our knowledge of the force of gravity has gone through a number of interesting phases. With Aristotle there was no force at all; falling bodies were in natural motion and required no force. This position, together with the strange definition of force which it implies, is quite unassailable on empirical grounds but is unsatisfactory on others. It yielded to Newton's conception of universal gravitation, one of the grandest scientific theories ever known. General relativity now tends to replace it, and it is instructive to see why that theory claims to be superior. In the realm of the immediately given one finds only two exceedingly small effects which are out of accord with Newton's law: the advance of the perihelion of the planet Mercury and the deflection of light rays passing near the sun. Both of them could be easily taken care of by a slight correction of Newton's law, a correction which leaves its essential constructs and its rules of correspondence intact. But Einstein wishes to scrap its entire conceptual structure, do away with masses and forces, assume that all bodies move along straightest paths, and put the blame for apparent deviations upon space. His arguments are powerful indeed; his case, however, does not rest on the little effects mentioned above but attains significance and persuasive power by an appeal to principles which unify our understanding.

Finally, we cast a hurried glance at one of the problems of today. The constituents of atomic nuclei are held together by forces which are known to be vastly more intense than the familiar forces of electric or gravitational attraction. They also show an unexpected kind of variation with the distance between attracting particles. Why should the physicist regard these facts, which he could well accommodate within the old scheme of things by acknowledging the discovery of a new and strange set of forces, as a challenge to his intelligence? The only answer is that he felt that there could be nothing so blunt and matter of fact as a new set of forces entering the scene unannounced and coming from nowhere; his faith in the internal fitness and the simplicity of all explanatory schemes reasserted itself and is now rewarding him with greater knowledge about mesons, of all things! For it turned out that the meson, the newly discovered nuclear particle, is the origin of these strange forces.

What, then, are the *principles*, aside from rules of correspondence, which guide us in forming constructs, and whence do they come?

5.2. Science and Metaphysics

We have endeavored to show that the directing principles do not come to us out of the sense world. It is perfectly proper to say that they are imposed on experience by reason, provided that we mean by imposing a manner of fitting rather than of constraining. The conformity between theory and experiment is not that between a die and its cast but that between a liquid and an object submerged in it. The metaphor becomes even more appropriate if we think of the object as elastic and yielding slightly to pressure of the circumambient fluid and of the fluid as shearing off or dissolving minor irregularities from the surface of the object.

To speak less vaguely and to show the rational principles at work, we select the example of nuclear forces for further brief consideration.[1] First there was the empirical evidence of new forces between the elementary particles composing matter, a state of affairs created by new observations (Chadwick's discovery of the neutron, recognition of protons and neutrons as building blocks of nuclei, binding energies of nuclei, the scattering of neutrons and protons by protons) coupled with old rules of correspondence. The baffling features in this situation were the enormous strength of the forces between nucleons[2] (about 10^{36} times the strength of the gravitational force) and the extremely short distance over which they act (this distance, called the range, is only 1/10,000 of the diameter of an atom). Why should these features be baffling?

In the eyes of the nonscientist the greatness of a discovery is often commensurate with its strangeness, with the amount of public surprise it occasions. To the scientist, however, this is a clear misapprehension, for he distrusts great departures from expectation and suspects error or incompleteness in a strange discovery. He is more easily baffled than the newspaper scientist and has greater faith in the reasonableness of his experience. In the case of nuclear forces he simply concluded that the departure from reasonable expectation was too great, that it represented a violation of what has sometimes been called the "consistency of

[1] A simple account of this subject may be found in B. Hoffmann, "The Strange Story of the Quantum," Harper & Brothers, New York, 1947. A somewhat more technical account is in a paper by H. Margenau and R. B. Setlow, *Am. J. Phys.*, 13:73 (1945).

[2] The name *nucleon* for any of the particles composing an atomic nucleus is coming into general use. Nucleons known at present are the proton, the neutron, and at least two kinds of meson.

nature" (meaning experience); he preferred to believe he knew only part of the story.

The Japanese physicist Yukawa then made a brilliant suggestion: If particles of mass intermediate between that of an electron and that of a proton existed in the neighborhood of two interacting neutrons or protons, the attraction between them could be explained in the same way as that between ordinary charged particles. The suggestion was brilliant because it lessened the strangeness of the original discovery, because it restored consistency to experience. And the remarkable fact is its success; the conjectured particles were found in cosmic-ray researches.

Belief in something like "consistency of nature" is certainly a powerful factor in scientific research. An account of the philosophy of science, indeed of the meaning of reality, cannot afford to neglect it as a methodological factor. Under the name of consistency of nature the principle in question is certainly vague, and we intend to formulate it much more clearly. But despite its present obscurity one sees immediately that the principle is not implied by any specific set of data and, in so far as it regulates our interpretation of the immediately given, its character cannot be determined completely even by the finite totality of all data. Of course it is substantiated by sensory experience and psychologically ingrained in all rational procedures. In a logical sense the belief in consistency—and other beliefs like it—are independent of sensory data, though it is not very meaningful to say that they are prior to perception. Whether or not they represent synthetic judgments will be left for later discussion, for that is a matter of some importance and also cause for much misunderstanding. Clearly these "principles" contain a hortatory element which has some aspects of the analytic. They have something in common with Kant's categories. To view them as a priori conditions of all experience, however, is to epitomize them to extinction; for while they are independent of spontaneous experience, they are nevertheless part of, and are stabilized by, experience as a whole. Yet we believe that there *can* be experience without them.

Metaphysics is an odious word in some scientific quarters. Its meaning has fluctuated widely throughout the history of philosophy. But since Kant it has tended to designate two large branches of thought, ontology and epistemology. We hold with Kant that epistemology must precede ontology and that epistemology denotes the methodology of the cognitive process. The methodology of science involves deliverances of sense as well as rules of correspondence, constructs, and principles regulating constructs. Having learned that the latter are not conveyed by sensory data and yet function in guiding experience, we should call them metaphysical principles in the modern sense of the word. Metaphysical principles, thus understood, are an important part of all procedures which ultimately define reality.

They are not predetermined for all time by our organs of knowledge; their immutability is a myth, openly exposed in the study of the history of every science. Saint Thomas Aquinas' postulate of compatibility of revelation and scientific method, Descartes' insistence on the identity of what is formally established with what is materially perceived, are metaphysical principles which have both been abandoned by modern science. And the same may be said about Kant's a priori conditions of knowledge and about Maxwell's belief in the ultimacy of mechanical models as symbols of explanation. It is true, however, that metaphysical principles change relatively slowly and that a slight change in them occasions profound modifications in the detailed structure of science.

What, then, is their origin? We hold that they first emerge in the stream of experience as tentative expedients, grow into implicit beliefs with increasing application, and finally, strengthened by repeated success, pervade the entire texture of our theories about the world. Their ubiquity makes them difficult to discern and indeed leads many positivists to deny their presence altogether.

Having recognized their presence and the role they play, let us now attempt to get a clearer understanding of their specific nature as it regulates the theories of present-day science.

5.3. The Requirement of Logical Fertility (A)

This book does not pretend to give a complete analysis of the metaphysical principles used in the deductive sciences; it merely attempts to enumerate them and to show them in action. We are not sure that our enumeration is exhaustive; we may say with certainty that the individual items, that is, the requirements on constructs, which are separately labeled and treated in Secs. A to F, are not cellular; they will be seen to overlap. As is often the case with ideal issues, they refuse to divide themselves neatly into countable parts, and there is no reason why they should; as already mentioned, ideas do not obey the laws of arithmetic. Thus, for example, when the present section is devoted to the requirement of logical fertility and the next to that of multiple connections, the distinction drawn is somewhat arbitrary. A separation of the two connected topics is advantageous merely because the first requirement speaks only of constructs, the second refers to constructs and rules of correspondence.

The demand that constructs shall possess *logical fertility* (or manageability) is so simple as to be nearly trivial. It requires that constructs shall be so formulated as to permit logical manipulations. They may be subjects or predicates, particulars or universals; they enter as terms into propositions which may include, contradict, or imply one another. All this, which is here crudely stated but forms the subject of many treatises on logic, will be expressed by saying that the constructs shall *obey logical laws*. It asserts little more than that they have relational meaning. But in no sense does the present requirement make it necessary for the proposition involving constructs to be materially true, to have an existential counterpart. The detailed connection between constructs and immediate experience is far from obvious and will be examined more closely in Chap. 6.

A theory which wholly violates the demand of logical fertility is difficult to imagine; a system like Berkeley's, which views man's perception as an occurrence in the mind of God, is perhaps a close approximation to this negative ideal. For it would be devoid of logical fertility were it not for the rational structure of God's mind. Appeal to blind fate is another instance of relative sterility, though not a perfect instance.

The absence of good examples which violate the requirement completely does not detract from its importance. Theories can obviously differ in *degree* of logical fertility. And the present axiom inclines us to look with favor upon the one possessing most, provided that all other requirements are equally met.

By virtue of this requirement physical laws may be stated as universal propositions from which passage to particular instances is possible. Such terms as *mass*, *molecule*, *chromosome* are capable of denoting concepts as well as individual entities; the general concept of number becomes relevant in science, and, on the highest plane, mathematics becomes applicable to constructs, all by virtue of this demand for logical fertility. The factual success of mathematics in science is not, of course, ensured by the present axiom alone.

Nor do we discern anywhere within the methodology of science convincing cause for the preference enjoyed by two-valued logics. Aristotelian logic was the first type available and has been used almost exclusively by scientists thus far.[1] There is no reason why it may not be abandoned at some future time. Experience cannot prove or disprove directly the law of the excluded middle (*tertium non datur*); hence it is certainly subject to tentative denial. But one thing, though it is sometimes advocated by scientists as well as philosophers, cannot be done. One may not assume that *tertium datur* in certain domains of science, such as the theory of probability and the quantum theory, and then proceed to use the conventional form of mathematics elsewhere without major scruples, as though nothing had happened. For many of the accepted results of mathematics have so far been obtained only with the use of two-valued logic and may be inconsistent with the basic tenets of many-valued calculi: compatibility must first be proved. This is particularly true for all results established by the method of *reductio ad absurdum*, which fails when there are more than two truth values.

Except in a very general and cursory way, natural science is joined with logic through mathematics. A change in logical axioms, to be effective in science, must first be traced through the entire structure of mathematics, and the necessary modifications in that field will have to be made. Only then can the success of the

[1] Unless we agree with Russell, who points out that this logic does not justify relational inferences of the kind: If horses are animals, then heads of horses are heads of animals.

change be judged by an application of the modified mathematics to empirical science. Unfortunately, however, this is more easily said than done. The mathematics now used in physics (*e.g.*, the differential calculus, operator theory) are not on the friendliest of terms with logic, even with two-valued logic; complete reduction of one to the other has not been achieved, and so long as the compatibility of useful mathematics with two-valued logic remains problematic, it is difficult to say what the ultimate fate of many-valued logics is going to be.

5.4. The Requirement of Multiple Connections (B)

Through their definitions, constructs enter into relations with one another. The process of definition deserves closer attention than it receives at this point, and we shall return to it in Chap. 12. At the moment our interest leaves aside the origin and settles on the character of the connections which constructs may enter. These may be of two types, *formal* and *epistemic*.

A formal connection is one which sets a construct in a purely logical relation with another construct; an epistemic connection is equivalent to and arises from a rule of correspondence which links the construct with data. Examples of formal connections are: all relations between geometric quantities which are provable on the basis of a suitable set of axioms, such as the relations between angles and sides of a triangle, the sine law, the cosine law, and so forth, the relation between a number and its square, a circle and its radius. In physics and chemistry also, every connection between entities that is derivable from postulates is a formal one: Examples are the relation between force and acceleration of a given mass (postulate: Newton's laws), the relation between a point charge and its electromagnetic field (postulate: Maxwell's equations), between the curvature of space and the quantity of matter in the universe (postulate: Einstein's law of general relativity), between the temperature and the mean kinetic energy of a gas (Gibbs' statistical mechanics), between the structure of a molecule and its molecular weight. It is true that all formal connections are stable only so long as certain postulates are maintained, that they are in this sense hypothetical judgments. It is also true that their formal character becomes less obvious when they are empirically verified, as many of them are.[1] This, however, need not concern us now.

Epistemic connections will doubtless occur to the reader in considerable abundance: They exist between the objective tree and what is called the vision of it, between a force and an awareness of muscular exertion, between the weight of an object and a reading on a scale, between a wavelength and the discernment of a line on a photographic plate. All these experiences are linked by epistemic connections. One of the terms is a construct, the other is in Nature. There are instances of connections which are difficult to classify, connections which are formal according to one and epistemic according to another theory. In view of the gradual transition already noted between sensory experience and constructs, this fact need give us no concern. Here again we encounter the possibility of different detailed interpretations; what matters is that each interpretation produces its own unambiguous set of formal and epistemic connections.

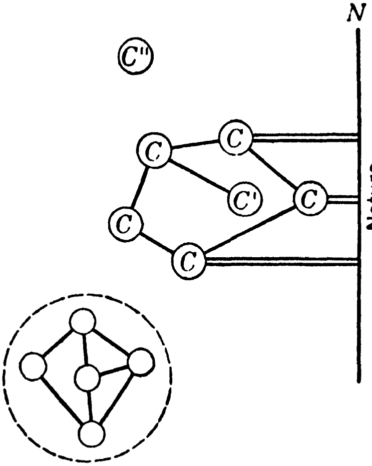

Figure 5.1

In Fig. 5.1 we have represented Nature, that is, the aggregate of all experience which is immediately given, by a vertical line marked N. Formal connections are indicated by single lines, epistemic connections by double ones. All constructs are denoted by C, with or without primes.

Those without primes stretch two or more arms, either toward

[1] Many physicists insist that Newton's second law, for instance, is an empirical relation. What is meant by this assertion and why it can be correct without invalidating the above analysis will be shown in Chap. 12. Suffice it to say here that the formal relation—it must be formal because of its universal character—would have been abandoned were it not also empirically verified in particular instances.

other constructs or toward Nature; they may be described as *multiply connected.* Every construct used in natural science will be seen to be of this type, permitting passage elsewhere in at least two ways. For example, from the idea of an electron one can pass to its mass, its charge, its field. From its electric field one can go in one direction to the idea of potential, in other directions to certain observable aspects of experience (along double lines). It is instructive to trace this nexus in any theory of physics.

But we also note in the figure a certain construct, labeled C', which is peninsular, possessing only one connection with a normal construct. It hangs loosely within the system and obtains what meaning it has only from a coherent set of others. An example of a peninsular construct is the color of an electron. No harm is done if color is assigned to it, but there is no way of substantiating this attribute, for it leads to no other significant knowledge by any formal route, nor does it allow verification by any possible rule of correspondence. The only egress from the idea is along the line which constituted it originally, that is, by repeating the categorical statement: The electron has color.

The construct C'' has no connection whatever with others, nor with Nature; it is insular. Its insertion into a theoretical system makes no difference whatever. An example of an insular construct is the God of deism, which has no place in science. There can also be a group of isolated constructs, mutually connected but without epistemic connections, such as those surrounded by the dotted circle. They may be said to form an island universe, consistent in itself though unverifiable. Science sometimes generates such tantalizing curiosities, then looks for possible rules of correspondence which might give them significance. But they are dropped again unless such rules are found. An instance has arisen in the quantum theory of the last two decades. The exclusion principle (see Chap. 20) demands that the states of a group of certain elementary particles, electrons for instance, shall be represented by mathematical functions which change their signs when the coordinates of any two particles are exchanged. This demand gives rise to many relations between constructs, and the constructs themselves have counterparts in Nature. Now the urge is strong to see what happens when the opposite demand is made, namely, that the state functions shall retain their signs upon an exchange of coordinates of elementary particles. This conjecture produces an equally interesting set of relations, but the entities involved fail to correspond to immediate experience. They form an island universe and are not at present considered as valid constructs of physics.

One might add the interesting speculation, often entertained by physicists, that epistemic connections which give substance to these conjectures may someday be discovered. Perhaps there are particles in the far reaches of the astronomical universe, not yet explored, which conform to the rules just mentioned. Science has at times made progress by thus uniting an island with its main body of knowledge.[1]

The metaphysical requirement here under examination may now be stated briefly: Constructs admissible in science must be multiply connected; they may not be insular or peninsular; sets forming an island universe must be excluded.

This axiom is not meant to settle all possible future contingencies. It should be admitted that situations can arise which render the established methodology of science powerless, where new and more precise directives are required. Science then has to feel its way and modify its metaphysics while proceeding. Indeed this question may have occurred to the reader: What if a *single line* could be drawn between one of the normal C's in the figure and one of the constructs of the island? Frankly, the state of affairs would then be embarrassing, not only to the philosopher of science who attempts to formulate its metaphysics, but also to science itself. We happen to be confronted with just that situation in physics today.

According to Dirac's theory, an electron can be in states of positive and in states of negative kinetic energy. The latter states have never been observed. At first they were thought to form an island universe and were forthwith dropped from consideration. But alas, the same theory showed that an electron, residing inoffensively in a state of positive energy, can pass without notice

[1] For an example of insular constructs in relativity theory see H. Weyl, "Philosophy of Mathematics and Natural Science," p. 118, Princeton University Press, Princeton, N. J., 1949.

into one of the objectionable states! The line in question had been drawn. Much confusion in scientific quarters was the result; even now physicists are holding final judgment regarding the "reality" of negative-energy electrons in abeyance, hoping for further clues. Very few regard the state of the theory as satisfactory.

One final reminder before we go on to the next requirement. A glance at Fig. 5.1 shows that normal constructs C do not invariably stretch one arm to Nature. It is sometimes asserted, though erroneously, that every element of a physical theory must have a *direct* empirical counterpart. Atomic theory is one among many which violate this supposition, for as we have already seen, there is no way of relating the minute constituents of matter *directly* to experience. But it must always be possible, and it is possible in atomic physics, for us to pass from an acceptable C *via other C's* to Nature. In this milder form, the proposition is implied by our requirement of multiple connections.

5.5 The Requirements of Permanence and Stability (C)

It goes without question that the elements of a theory may not be arbitrarily altered to fit experience. What is once accepted must be retained with respect to all its implications for sensory experience. It is to be judged true or false as an integral logical complex; there must be permanence in its structure and in the rules of correspondence which give it epistemic importance. Thus, similar sets of external impressions do not connote a tree in one instance and a rock in another, nor does a tree ever become a rock in theoretical manipulations. The same stability is observed in the less familiar procedures of natural science: a luminous sensation cannot correspond to an electromagnetic wave in one case and to a particle in another, nor does a particle transform itself into a wave when it is treated in accordance with the quantum theory. Even in that branch of science, which has suffered so severely from misrepresentation, a construct remains what it is so long as the premises of the theory are accepted.

The permanence to which we are drawing attention is not, to be sure, of an absolute sort. Theories are often modified or rejected, rules of correlation are changed in the presence of new evidence. But these modifications are then universally applied to all instances; the liberty of using the old rules here and the new ones there at the investigator's convenience is basically denied. What is here called permanence, and what indeed exhibits itself as permanent structure in a temporal sequence of applications, turns into an aspect of uniformity when viewed as the uniform principle in many simultaneous applications. But in either sense this permanence, we now note, is not unlimited: it extends over the lifetime of a given theory.

The process of cognition involves another noteworthy quality closely connected with permanence, a quality which for that reason we wish to include under this heading. When I see a tree, I am sure it is a tree and not a rock; that is to say, I am not in doubt as to the rule of correspondence which relates my immediate experience to objects. Uncertainty of identification can arise only as a result of ambiguities in the immediate experience, not through an available choice of rules of correspondence. These rules are clear and unequivocal in their action once they have become maxims of experience. It is therefore improper to assert that a given complex of sense data relates to objects with probability only, that one rule has a greater chance of being correct than others. The *theoretical* components of experience are taken to be sharply defined and uniquely determinable, whereas the *immediate* ones are subject to defects of clarity. We shall return to this situation in Chap. 6.

In stating our position, we acknowledge the possibility of an alternative view, one frequently held by philosophers of science who received their training in correlational discipline. It is that data are unique in their givenness and that uncertainties or probabilities are introduced by theoretical processes. We see no reason why a complete understanding of scientific experience *could* not be built upon this premise. For while it is necessary to relax the stringency of the correspondence between constructs and data somewhere in order to provide for verification in finite terms, this may be done with equal a priori justice on the side of theory and on the side of data.

But we are not describing here what could be done; we are giving an account of what is being done in deductive science.

Hence our choice of position. A physical theory defines its concepts sharply—albeit often erroneously—transforms them by means of unambiguous mathematical equations, and predicts with exactness. The rigor of such predictions is finally softened by an admission of experimental errors.[1]

It seems to us that the requirement of stable constructs, which is quite manifestly made in science as was noted, also conforms more closely to the ways in which we ordinarily comprehend the world. Everyone is willing to admit the limitation of his sensory perceptions; these are regarded as unavoidable imperfections of knowledge, whereas similar imperfections of reasoning are considered as mistakes which can be and are to be avoided.

To summarize our somewhat discursive treatment of requirement C: the constructs generated in explanation of a set of immediate experiences must, so long as the theory of which they form a part is accepted, be used with utmost respect for their integrity of meaning in all applications. They are to be treated as logically clear and sharp entities which may or may not correspond to clear and certain empirical situations.

5.6. The Requirement of Extensibility of Constructs (D)

None of the principles reviewed thus far accounts for the expansive drive of physical methodology, for the ever-widening encompassment of new domains by science. On the one hand, scientists judge the quality, and ultimately the correctness, of a given theory by its range of application, taking the generality of a system as a measure not only of its usefulness but of its credibility. On the other hand, the same tendency, the same judgment expresses itself in the common belief that ultimately a single theory will render an adequate account of all experience. Our endeavor to reduce the findings of biology, psychology, and even the social sciences to a basis of physical laws is a case in point.

[1] The foregoing description is incomplete in so far as theories rarely predict data from nonempirical premises; they usually "transform" data, *i.e.*, they start with one set of data and produce another. In doing this, theories never introduce uncertainties of their own except through voluntary approximations made by the computer; they merely modify the uncertainties inherent in the initial set of data. This will be further discussed in the next chapter.

To incorporate this trait into the methodology of science we record, as the fourth metaphysical requirement, *extensibility* of constructs. Its specific meaning is easily illustrated by a host of important instances.

a. Galileo's greatest achievement was the formulation of a theory which explained the falling of terrestrial objects, a theory wholly satisfactory with respect to requirements A to C. In a limited sense, it was also extensible since the ideas of mass and acceleration could be applied to a great variety of bodies, namely, all those located near the surface of the earth. However, Newton's discovery of the law of universal gravitation won far greater acclaim because it was more *extensible;* it included within its range the celestial bodies. By seizing upon the idea of a gravitational force acting between *all* particles and varying inversely with the second power of the distance, Newton provided a concept of impressive width and thereby significantly advanced the science of mechanics.

b. Similar progress was made when *special* relativity was superseded by the *general* theory of relativity. The former found it necessary to adopt the special notion of Newtonian forces, although it treated them in a distinctive and a more successful way. Its virtues were most evident where it confined itself to inertial systems.[1] This led to the distinction between two kinds of forces: those caused by physical agencies (*e.g.*, gravitation, which is due to the attracting earth) and those caused by mere motion of the body under consideration (*e.g.*, centripetal force). Aside from this, the success of the special theory as an explanatory scheme was satisfactory. Then Einstein showed, by generalizing the axioms of geometry, how all forces could be regarded as being of the latter type. Gravitation, which previously enjoyed a status of its own, now appeared as an instance of a more extensible construct: the curvature of space-time.

c. In chemistry, similar transformations have often taken place. The concept of a valence bond seemed for a long time to be irreducible. Despite its success it left chemists embarrassed because of its failure to be sufficiently extensible; it explained how

[1] An inertial system is one which moves without acceleration relative to the mean position of the fixed stars.

atoms unite to form molecules but very little else. Relief was felt when Heitler and London, in 1927, proved for the first time that valence forces themselves are mere instances of a vastly more extensible idea: the quantum interaction between electrical charges.

d. Expansive tendencies are equally manifest in the life sciences. Vitalism, for example, has fallen into disrepute not because it provides an inadequate understanding of the facts of biology—which is hardly true—but because it affirms autonomy and non-extensibility of biological explanatory constructs. It thus sins against the requirement of extensibility. In a similar way, the opponents of behaviorism, often unable to find factual fault with the procedures and the results of that movement, rest their case primarily, it seems, upon the special and inextensible nature of the behaviorist's assumptions, upon his unwillingness to avail himself in full measure of the theoretical aspects of neurophysiology and chemistry. The stimulus-response relation which is so fundamental in this theory is admittedly limited to living things.

Conflicts like these are frequently confused and made difficult of solution by being represented as grounded in observational fact. It should here be emphasized that the point at issue is whether vitalism, behaviorism, or many another interpretative doctrine contradicts or satisfies a *metaphysical* requirement. This can be finally settled only by an analysis of the whole methodology of science. In other words, it can hardly be settled at all unless we arrive at more general agreement with regard to those matters which are here being examined. The dispute about vitalism, like many other general controversies, high-lights the importance of our recognizing the presence and the action of metaphysical principles in science.

e. Finally, we undertake to discuss an example in which the need of the methodological approach becomes critically important. Science has grown and is growing without conscious philosophic control. It is this author's belief that it could grow faster if attention were given to the principles which have guided it unwittingly in the past. Physics, having undergone a number of erratic changes in its recent history, is at present alive with a slogan that we must look for *different laws of nature on different scales of magnitude.* The nucleus of an atom, it is said, is to be described by rules appropriate to domains of the order 10^{-12} centimeter in linear dimension, rules which have not yet been discovered. Atoms, themselves, and other physical entities of the order of 10^{-8} centimeter in size, are subject to the laws of quantum mechanics; bodies of ordinary size are governed by Newtonian mechanics; stars and galaxies behave in a distinctive manner of their own. A glance at history and at the principles operative within the development of science certainly exposes such clichés as irresponsible, and one may hope that a respect for these principles, particularly for the requirement of extensibility here under discussion, will curb unduly multifarious endeavors along the line of "special laws for special physical domains."

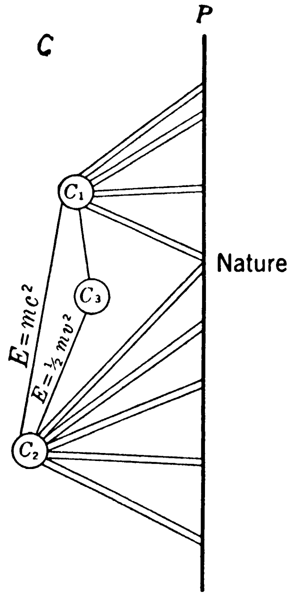

Figure 5.2

We have seen the principle of extensibility in action from afar; it is well for us now to observe it more closely. Constructs, we recall, enter into two types of relation: with Nature and with other constructs. Hence they should be extensible in these two ways. Figure 5.2 illustrates this: it shows two central constructs of mechanics, mass (C_1) and energy (C_2). Mass has been extended via rules of correspondence (double lines), not merely to the legendary stones which Galileo dropped from the Leaning Tower of Pisa or Newton's apple at Woolsthorpe, but to all other material bodies, to the moon, the planets, the stars and finally to electricity and light. Energy can be identified with equally many empirical data; all moving bodies, electricity, light are known to possess it. Hence the number of double bonds stretching from C_1 and from C_2 to Nature is exceedingly numerous. Constructs must be extensible, first of all, in this sense.

Furthermore, there exist mediate relations between C_1 and C_2

via other constructs such as C_3. If C_3 stands for velocity (the figure omits its rules of correspondence), then the equation Kinetic energy = $\frac{1}{2}mv^2$ exemplifies such a relation. One of Einstein's great achievements was to provide a more direct link between C_1 and C_2. This occurs in the famous mass-energy relation, which asserts that every mass is equivalent to a proportional amount of energy. The idea of mass has thus finally extended itself to include energy, or, what amounts to the same thing, that of energy to include mass.

At this point, the requirement of extensibility shows a close affinity to B, the requirement of multiple connections. Both are satisfied in the same creative act, for while Einstein merely introduced a new relation, that relation happened to be one of equivalence which allowed the merger of two constructs and hence the extension of either. The greatness of this discovery comes from the unusual way in which it satisfies a metaphysical drive and is felt instinctively by every scientist.

Figure 5.2 illustrates a small part of mechanics. Almost any other theory of physics could have served as well in exemplifying the meaning of requirement D here under examination.

5.7. The Requirement of Causality (E)

There are places in every book where systematic development demands one sequence of presentation while pedagogy demands another. We have reached such a place. Causality is indeed one of the metaphysical requirements of physical theory and should therefore be listed here, but its detailed study involves technical matters which would arrest us unduly at this point and had better be left for later discussion (Chap. 19). For that reason, we present here a bare preliminary outline of this requirement.

The word *causality* will here be used in a very specific sense, not with the quadruple meaning of Aristotle (formal, material, efficient, final cause) or the fortyfold proliferation of causes that occurred in the seventeenth century.[1] It represents a relation patterned after the good old "If *A*, then *B*." Certain obscurities in this relation, however, need to be clarified. Above all it must

[1] See A. Wolf, "Spinoza's Short Treatise," A. & C. Black, Ltd., London, 1910.

be stated whether A and B represent immediate experiences, *i.e.*, data, or constructs; if the latter, then whether they are *objects* or *states* of objects. Also, the ambiguity inherent in "if" and "then" is to be eliminated, for it is perhaps not clear without comment whether these are to be taken in a conditional or in a temporal sense.

The answers here given are far from obvious at first sight; they definitely require justification (Chap. 19). We wish to regard causality as a relation between constructs, in particular as a relation between *states*,[1] or conditions, of physical systems. The principle of causality asserts that a given state is invariably followed, in time, by another specifiable state. Even without closer analysis this formulation will be seen to possess two virtues: It is precise and definite, and it reflects the best practices in the exact sciences. Later we hope to show that more customary views of causality, in so far as they are meaningful, can always be reduced to this.

Lightning is said to be the cause of thunder, or thunder the effect of lightning. To take this relation as one between immediately perceived phenomena has numerous disadvantages: For one thing it contradicts usage, as lightning is meant to be the cause of thunder even when the lightning is not seen. Furthermore, it is difficult on this interpretation to get around the powerful arguments of empiricism, which sees in causality merely an overwhelmingly frequent succession of experiences. This is precisely what the causal relation would be if cause and effect were not taken as constructs.

In the sense we advocate, lightning is a condition of the atmosphere describable in terms of physical quantities, such as electric field strengths, ion densities, excitation of atoms and molecules, luminosity. In fact, all these are measurable quantities. There exist laws by means of which another condition of the same medium, described as thunder and characterized by other measurable quantities, *e.g.*, rhythmic variation in density, can be inferred as the consequent of the first condition. If this condition is verified sufficiently often, the laws are said to be valid and are said to be *causal laws*. Truly, then, causality is a property of physical laws and not of observations.

[1] States are special constructs, to be considered more carefully in Chap. 8.

The word *condition* is here used loosely. We shall see later (Chap. 8) that every theory contains or generates constructs which can be employed to designate the states of physical systems uniquely and completely. Causality holds with respect to states defined in terms of such significant variables only, and this will here always be understood. Thus the mechanical condition of a mass particle which can enter properly into causal connection with other states is one defined in terms of position and velocity; it is irrelevant to the causal description of mechanics whether the particle is blue or malodorous. On the other hand, its blueness may be a proper causal condition with respect to a theory of optics. We see, therefore, that the word condition requires further limitations and careful alignment with the other components of a given discipline. What matters at this stage of our discussion, however, is that there can be laws which give rise to conditions showing an invariable linkage in time and other laws which do not.

We hold that causality is a metaphysical requirement. It demands that constructs shall be so chosen as to *generate causal laws*.

There is no need for revising this appraisal of the causal postulate in the face of modern physics, which is often claimed to have done away with causality. As a strict relation between immediate perceptibles, quantum theory has taught us to deny it. The impropriety of that view should have been, and was, plainly evident to the thoughtful students of classical physics; it was destroyed by Hume long before the day of Heisenberg and Born. But the contribution of these latter men is of greatest significance also, for it shows what strange and unexpected properties the *states* of physical systems must possess in order to be causally related.

5.8. Simplicity and Elegance (F)

Most embarrassing among the metaphysical requirements is one which is often called the postulate of simplicity, for it is more elusive and more difficult to state than all the others. When two theories present themselves as competent explanations of a given complex of sensory experience, science decides in favor of the "simpler" one. The victory of the Copernican over the geocentric system of astronomy is often cited as a case in point. Ptolemy, whose Almagest contained the most successful account of sidereal and planteary motions, ascribed to each heavenly body a circular path (epicycle) the center of which revolved on another circle (deferent) about the earth. A large number of unrelated epicycles was needed to explain the observations, but otherwise the system served well and with quantitative precision. Copernicus, by placing the sun at the center of the planetary universe, was able to reduce the number of epicycles from eighty-three to seventeen. Their complete elimination was impossible because he assumed the orbit of a planet to be a circle, not an ellipse (Kepler). Historical records indicate that Copernicus was unaware of the fundamental aspects of his so-called "revolution," unaware perhaps of its historical importance, he rested content with having produced a *simpler* scheme for prediction. As an illustration of the principle of simplicity the heliocentric discovery has a peculiar appeal because it allows simplicity to be arithmetized; it involves a reduction in the number of epicycles from eighty-three to seventeen.

Unfortunately, however, there are few examples where the situation is so unambiguous. The electromagnetic theory of light superseded the hypothesis of a luminiferous ether because it was simpler; but what is there to be counted in this situation? Quite probably, what is at issue is already cared for by the preceding postulates in our list, chiefly those of extensibility and multiple connection. It seems doubtful, therefore, that another postulate is needed on logical grounds, although no harm is done by adding it. Our inability to count ideas, which makes the simple so elusive, also prevents a discrete, nonoverlapping classification of our methodological principles.

Historically, simplicity was early recognized as a guiding motive in research. Occam's razor is perhaps the most celebrated device for effecting it: "Non sunt entia multiplicanda pracfer necessitatem." The professed nominalism of the *doctor invincibilis* has impregnated the dictum with a certain philosophic partiality in its origin, and it is still bearing this color in its more modern version of economy of thought. In truth it has no color; it is a plain confession of faith on the part of those who seek scientific knowledge. It somehow expresses the totality of all metaphysical requirements in concert. Kepler [1] voiced it often: "Natura simplicitatem amat"; "Amat illa unitatem"; "Nunquam in ipsa quicquam otiosum aut superfluum exstitit"; "Natura semper quod potest per faciliora, non agit per ambages difficiles." Planck, Einstein, and Cassirer, among many, have avowed it fervently. Thus we bow to history and include simplicity in our list.

There is also an aesthetic element, closely allied with simplicity, to be discerned in the metaphysics of science. Some discoveries are pretty, some are beautiful and awesome; the scientist often employs these words to express his aesthetic satisfaction. The ecstasy of creation, which proverbially is the artist's reward, is proper to scientific achievement in equal measure, as the writings and oral utterances of scientific genius amply testify. It comes as a glorious fulfillment to the expectations which sensitivity to the metaphysical requirements of science has instilled in a researcher.—But it is questionable whether we are now discussing things relevant to methodology; perhaps we have gradually drifted into the precincts of psychology.

We thus end our account of the metaphysics of science. What has it availed us? The realistic reader, who suspected from the beginning that we were telling a rather subjective story of the affairs of science, may now find his suspicions confirmed. He will perhaps take issue with the basic premise which marks the concepts of science as something close to inventions. In fact the position outlined in the book thus far can, without much shift in emphasis, be interpreted as idealistic. If the certainties of scientific experience are mere *constructs*, the critic will ask, where does science get the stability which it obviously possesses? Why does it lay claim to possessing *facts* in a more solid sense than other disciplines? Without an anchor to "reality," he will conclude, our epistemology, itself amorphous, floats within experience like jelly in an ocean.

To answer him, we would first object to the seemingly innocent bias introduced into the argument by the simple word "mere":

[1] See I. Hart, "Makers of Science; Mathematics, Physics, Astronomy," Oxford University Press, New York, 1924.

generically, the elements of scientific theories are undeniably constructs. But they are not *mere* constructs, as idle inventions would be. They do not owe their existence to accident or caprice but stand in uniform correlation with immediate experience; and after their birth they are subjected to a most rigorous regime of methodological principles. These constraints alone eliminate any mereness from the nature of scientific constructs. Their scientific validity, their complete trustworthiness, however, is conferred upon them by further, even more limiting and more exacting procedures: by a continual test against immediate experience, called *confirmation*. This we propose to analyze in the following chapter, where the constructs now introduced will be seen to transform themselves into what we shall call *verifacts*. After that we hope to make it clear how these scientific constructs, instead of drifting anchorless within experience, congeal into the firmest kind of reality we know.[1]

Summary

To be acceptable to science, as to common sense, constructs must satisfy two kinds of demands. The first is of a formal sort: it requires that every explanatory system possess a consistency and a logical fertility which sense data alone do not confer. The second demand is one of empirical verifiability, a problem examined in the following chapter.

[1] The view here expressed is found documented in the writings of outstanding men in modern physics. To quote de Broglie: "Si tranchée que paraisse à première vue la distinction entre la découverte expérimentale et l'invention théorique, une étude plus attentive ne tarde pas à l'attenuer considérablement: car elle montre que la découverte des faits expérimentaux, du moins dans la science actuelle, est à bien des égards une invention tandis que l'invention théorique est en quelque mesure une découverte."

Max Born, in "Experiment and Theory in Physics," Cambridge University Press, London, 1944, says: "I believe that there is no philosophical highroad in science, with epistemological signposts. No, we are in a jungle and find our way by trial and error, building our road *behind* us as we proceed. We do not *find* signposts at crossroads, but our own scouts *erect* them, to help the rest. . . . My advice to those who wish to learn the art of scientific prophecy is not to rely on abstract reason, but to decipher the secret language of Nature from Nature's documents, the facts of experience."

The formal requirements are here called metaphysical, for their function approximates closely the role which was played by metaphysics in the older systems of philosophy. Here these requirements are analyzed into a spectrum of six more or less specific axioms, or postulates (A to F), which are not wholly distinct but blend together into a system which might be called the logic of theoretical science.

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CHAPTER 6

Empirical Confirmation

6.1. The Circuit of Empirical Confirmation

Rules of correspondence work two ways. They allow us to pass from Nature to the field of constructs, and when used in reverse they provide us with *expectations*. In this latter mode they furnish what Lewis [1] has aptly called "terminating judgments." The reversals are, however, in need of some analysis, for they can be accomplished in trivial and in nontrivial fashion.

A trivial reversal of a rule of correspondence occurs when the path which led originally to the formation of a construct is retraced. Seeing a tree is an act which, though integral in a psychological sense, nevertheless involves the heterogeneous elements of immediate awareness and construction; it serves to set up the external object, tree. If, having seen a tree and then gazed elsewhere, I turn back in the former direction and expect to see the tree, that expectation is a trivial reversal of a rule of correspondence. Another is the judgment that the sun will rise tomorrow, or that sweet coffee contains sugar, or that an electrical discharge in mercury vapor is green.

But suppose we hear the sound from a bell and, remembering simple physics, assume it to be a vibratory disturbance in the surrounding air. Very little reflection then tells us that, if there were no air, the vibrations could not exist and the bell could not be heard. Hence, having become curious, we place the bell under a jar, evacuate it while the bell is ringing, and find the sound slowly dying away.

In "predicting" this we went through the following little exercise: From the sound sensed in Nature we proceeded by a

[1] C. I. Lewis, "An Analysis of Knowledge and Valuation," The Open Court Publishing Company, La Salle, Ill., 1947.

rule to the constructs, air, vibration—in fact to the proposition: the air is vibrating. From a hypothetical denial of this proposition one can pass back to Nature via the same rule of correspondence, which then implies the absence of sound. In our example, the denial was forced by taking away the subject of the proposition, *i.e.*, by removing the air. This was a nontrivial reversal, or rather a nontrivial return to Nature.[1]

While it is interesting to dissect the logical structure of this and all other instances of prediction, the detail encountered is usually so great as to forbid it, and we shall therefore follow custom and avoid it. The methodological movement is clear: It passes from perception to the constructional realm, where it undergoes a logical transformation, and then it returns to perception. The transformation, that is, the transfer in the constructional sphere from one element to another, characterizes the return as a nontrivial one.

To simplify the terminology, we may speak of the class of all perceptions that may enter experience as the P field, of all constructs as the C field, thus permitting ourselves a use of the term *field* which is descriptive, but not accurate in the technical mathematical sense. The P field is identical with Nature and might be pictured as a two-dimensional plane, to conform with the lack of analytic depth peculiar to perceptions. The C field is to be thought of as a sort of three-dimensional continuum of rational structure. The movement sketched above then represents a circuit which starts in P, travels to a point in C, say C_1, goes from there to another point in C, for example, C_2, and thence to a point on P. The greater the distance between C_1 and C_2, the greater the departure from triviality in the circuit.

A few examples will clarify the meaning of these circuits. The examples are arranged in the order of increasing distances between C_1 and C_2.

[1] The latter phrase will be preferable to the more meticulous reader who realizes that the rule employed on return differs from the first because it says non-B implies non-A, while the first says A implies B. To establish the justification for this logical transformation in symbolic terms is interesting and important. The scientist rarely pauses to do so, trusting his mathematical intuition to keep him afloat.

a. We see a stone. The idea, stone, involves that of matter with all its logical attributes, such as hardness. This leads to the expectation or prediction that the sensed stone will be hard to the touch of the hand.

b. The sun is seen. The sun is an astronomical body subject to certain theoretical laws. Hence the conclusion that the sun will be found to be highest at noon.

(*c*) A falling object elicited in Newton the conjecture of masses subject to the law of universal gravitation. Analytic procedures, made possible by the theory of "fluxions" [1] and involving additional constructs such as the distance between the earth and the moon, led him to "predict" the period of revolution of the moon.

d. The physicist studies the *spectral lines* emitted by a substance named sodium. Having collected a large body of empirical evidence, he is able to construct, using rules of correlation which have recommended themselves by their fruitfulness in other instances, an ordered arrangement of electrical charges called the sodium atom. On further theoretical investigation of considerable laboriousness and mathematical complexity, but without essential guidance by other empirical data, he comes forth with an apparently unrelated prediction concerning the *index of refraction* of sodium vapor.

e. The nuclear physicist, studying the atomic weights of all elements, observing the constituents of nuclei, noting the scattering of one kind of nuclear particle by another, and so forth, evolved the idea of a strange force between nucleons. There the matter stood for a while. The new force, though arising from respectable and compelling epistemic correlations, was fundamentally not tied to other constructs; it was felt to be *unexplained.* The physicist found himself at a loss to make any but fairly trivial returns to Nature. All this changed with one brilliant conception. Yukawa said: There must be "mesons," to be discovered by noting certain sensory facts in Nature. And there were.

The methodological form of these examples is (somewhat naïvely) illustrated in Fig. 6.1, which represents what scientists call prediction. The start is made at some point in the field of perception P_1; our quest then sweeps over rules of correspondence R

[1] Newton's name for the differential calculus.

into the field of constructs; here it is embedded in logical relations, attains meaning and scope; finally it sweeps back over other rules of correspondence toward P_2, a second point in P, which is said to be predicted.[1] The combination of constructs (enclosed within the circular contour of the diagram) which were utilized in this movement forms a theory.

The conditions enabling a prediction are thus seen to be of two kinds: empirical knowledge symbolized by P_1, and the rational knowledge symbolized by constructs. No prediction is possible when one of the two is missing. The claim of the rationalist, who wishes to omit the first part of the journey and to start within C, is as futile as that of the empiricist, who thinks he is going directly from P_1 to P_2. And yet it is remarkable how nearly both are right. It is not wholly wrong to say with H. Cohen: "Nur das Denken kann erzeugen, was als Sein gelten kann." Nor does it falsify our picture to insist, with Locke, that "Nihil est in intellectu quod non prius fuerit in sensu."

The circuit drawn in Fig. 6.1 may be interpreted in another way. Only if the theory through which it passes is already accepted does it function as a prediction. The other interpretation conceives the circuit as a device for *establishing a theory;* it involves to begin with merely a tentative acceptance of that theory and views the passage to P_2 as a challenge to Nature. If the challenge is met, the theory is said to be confirmed or verified in this instance. And the theory is *valid* if it is confirmed in a sufficient number of instances. Furthermore, the constructs which form organic parts of a valid theory will themselves be called valid constructs, or verifacts. *Processes of validation, when conjoined with the metaphysical requirements discussed in the previous chapter, create scientific knowledge. It is this purgatory of validation which removes from constructs the figmentary stigma which their epistemological genesis first attached to them.*

[1] The word *prediction*, as used in science, does not mean "forecast" in a temporal sense. *Pre-* implies "prior to completed knowledge"; it does not contrast with *post-*, as does *ante-*. The counterpart to *pre*fix is not postfix but *suf*fix. It is therefore unnecessary to coin a new word, *postdiction*, to denote what we should call prediction of the past. The use of this word, though it has been suggested, would seem a bit "*prepost*erous."

But this leaves us with two important questions: (1) When is the number of instances of confirmation sufficient? (2) What constitutes agreement between theory and observation? The second question is particularly troublesome because, as has been pointed out, theoretical prediction is always definite, whereas the immediately given is of necessity surrounded by a haze of uncertainty. To answer it requires the exposition of slightly technical considera-

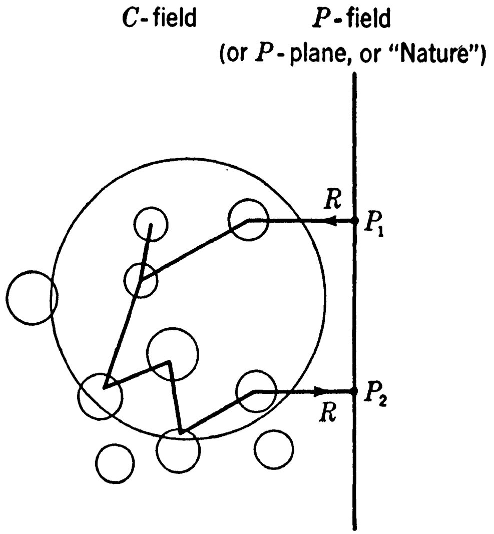

Figure 6.1

tions; these will be presented in the following sections. Question 1 can be treated at once.

Though it may seem strange to the logician, scientists are none too meticulous in their demands for sufficiency in the number of validating instances. They have never evolved a pretentious formalism for deciding when they may stop testing a theory. A few crucial confirmations often satisfy their quest for certainty, perhaps to the consternation of staunch empiricists. But the reason for their modesty should now be apparent: It is to be found in the orderly relations which exist among constructs before they are put to test. Reliance upon the logical coherence of his conceptions exempts the scientist from the need of exhaustive verification. There is an important sense in which a theory is more than the class of sentences it can generate, and the awareness of this transcendency inclines scientists to unhesitating acceptance of a theory after it has been subjected to a number of tests which are wholly inadequate statistically. This pervading rational nexus also allows them to decide what experiments are crucial.

6.2. The Meaning of Agreement between Theory and Observation

In claiming that all immediate experience is uncertain one seems guilty of exaggeration, for it is true that some aspects of perception attain the highest possible measure of certainty. While an infinitesimal doubt may affect my experience of seeing a *tree*, the act of *seeing* nevertheless is indubitable. But as we progress to higher degrees of qualification, elements of uncertainty creep into our declarations: "I see a red rose" is questionable if I am color-blind; "This table is three feet high" is sure to be suspected as lacking exactness—every physicist knows that he may not state the value of a physical constant without estimating the error of measurement which the value admits. Thus, as we proceed to greater refinement of perception, the uncertainty, while often becoming smaller, also becomes more manifest. We are confronted with intrinsic uncertainties everywhere on the level of particularized immediacies. Of course it must be admitted that, on the level of language, these uncertainties can often be artfully undone: If I say, "This table is *about* three feet high," I cannot be accused of inexactness—but the underlying equivocality of experience is not changed by this form of statement.

Now it happens that science in its more advanced stages is interested primarily in experiences of a highly specific type, called measurements. All measurements involve *numbers*. But this generalization should not be understood as barring from scientific interest many observations which do not yield numbers, examples of which are easy to cite. Suppose, for instance, that according to some theory a certain substance should emit a spectral line in a given spectral region and that according to another the line is forbidden. Whether or not it occurs is a matter of much importance, and it is settled wholly without an appeal to number. Again, it may be of great value to know whether two straight lines drawn on paper do or do not intersect. Observations of this sort again are not significantly represented as numbers; in our sense they are not measurements, but they are nevertheless important.

Turning now to measurements proper, we note a variety of ways in which they lead to numbers. Eddington believed that all measurements result from readings of the position of pointers on a scale, but in this he strained the facts for the sake of uniformity. To wit, there is at least one important kind of measurement that cannot be reduced to pointer readings, namely, counting. Much useful physical information was obtained by the early workers in the field of radioactivity through the tedious process of counting scintillations on a screen or by listening to the clicks of a relay activated by a Geiger counter. Observations on the growth of an embryo and on cell division yield numbers, though not via pointer readings. All these activities should be classified as measurements in the wider sense.

There has been much discussion concerning the kinds or classes of numbers encountered in immediate perception. It is usually argued that they are *real* numbers, the supposition being that $\sqrt{-1}$ does not occur in immediate experience. But neither does any other number. An appeal to number already involves a passage to the C field, a passage so obvious that it easily remains unnoticed. Thus even the customary statement of the results of an observation is already a paraphrase, though a harmless one, of an immediate experience. With this realization it becomes clearer that complex numbers need not be excluded from the records of observation, and their use does indeed conform with modern practice. Engineers employ theories which attribute complex conductivities to all substances. The real part of the conductivity is a measure of the electrical power transmitted by the substance, the imaginary part a measure of the phase change which the power undergoes. Both are measurable, customarily on scales that are calibrated in real numbers. But for the sake of use in formulas the numbers on one scale must be multiplied by $\sqrt{-1}$.

It is therefore quite proper to maintain that the entire field of complex numbers is available for expressing the results of observations.

There is the further distinction between rational and irrational numbers, the former being ratios of integers, the latter such quantities as $\sqrt{2}$, which cannot be written as a ratio of integers. Presumably every measuring instrument has a "least count," a smallest interval of reliable detection. The smallest division on a carpenter's rule is $\frac{1}{8}$ in., and it is doubtful that a person using the rule can discern a fraction of it. Whatever the least count for an operator using an instrument may be, his measurement must result in a multiple of least counts. If, then, the least count is a rational fraction of the unit of measurement, the result is a rational number of units. This is the argument which has convinced some writers of the impropriety of irrationals in science.

The argument is not very conclusive, however, for suppose we were to choose as our unit, not 1 in., but π in. Since π is irrational (and transcendental), the value obtained with a carpenter's rule is now irrational (and transcendental). Or suppose we wanted to measure the side of a square and, lacking a ruler, we employed an integraph, a device which measures the area. If the result were 200 cm^2, the measured length would be irrational. We see how troublesome it is to decide the issue between these two classes of numbers.

Fortunately, the decision is unnecessary, and the issue with regard to measurements is quite illusory. The intrinsic uncertainty of perception relieves us of all the difficulties. Being finite, this uncertainty surrounds every measured value by a range of other possible values, and since rational numbers intersperse the irrational ones in every small interval, the measurement allows no discrimination. Perhaps this relief is no sheer satisfaction, for the mathematician cannot help feeling slightly aggrieved at an empirical science which permits him no application for so beautiful a concept as the distinction between the rational and the irrational numbers. The moral to the philosopher, however, is a simple one: the distinction has certainly not come from sensory perception; reflection upon this point furnishes an indictment of pure empiricism, and perhaps a recommendation for the kind of constructional epistemology here presented.

We should now take a closer look at the uncertainties under discussion. Suppose we measure the length of a table. If we are satisfied with a rough result, we may use a carpenter's rule and state the length as 61 3/8 in., realizing, however, that we may be in error by about 1/8 in. A more careful procedure, and one adopted in scientific practice, is to perform the measurement with a more precise instrument, perhaps a rule equipped with a vernier scale, and also to repeat the measurement a number of times. This yields a set of results, such as 61.36 in., 61.40 in., 61.39 in., 61.37 in., and so forth. None of them is more trustworthy than any other, and there is no way of finding which is "true." Greater refinement would produce another set of numbers, more closely spaced but again without preference as to individual values. It is this equivocation of immediate perception, wholly unalterable in principle by effort or design, which concerns us here.

In particular, it forces us to return for more serious study to the question asked in the preceding section: How can deductive theory cope with equivocal observations?

6.3. Measures of Precision

First, let us not forget that a circuit of empirical confirmation (Fig. 6.1) has *two* termini, not one. Hence there is one haze of uncertainty surrounding the point P_1, another surrounding P_2. Neither the rules of correlations nor the deductive procedures in the C field introduce uncertainties of their own. If a theory is to be confirmed, then, it must propagate and transform the haze around P_1 into a predicted haze about P_2, and the actual haze of P_2 must somehow overlap, or in some other sense be in agreement with, the predicted one. The idea of overlapping would seem to recommend itself as attractive and practical if only the *range* of uncertainty could be numerically defined. To say, for instance, that the range of P_2 extends from the smallest to the largest value measured will not do, since if more measurements had been made, still smaller and still larger numbers might have appeared. Our first task will therefore be to indicate how a finite error, or a measure of precision, can be assigned to a scattered set of numerical data. This will take us a little distance into the theory of errors.

The word *error* carries an overtone of "falsification" or "departure from truth" that is misleading in so far as it suggests a true value which experience could reveal to us. Observation delivers no true value; what is taken as the true value on the basis of a given set of measurements is fixed by methodological agreement. We see how even here, at the very boundary of the immediately given, regulatory devices from the *C* field must be invoked to stabilize the epistemological situation. What is actually done is this: One chooses as the true value of a set of observations the *most probable value*—most probable under conditions to be outlined forthwith—and this happens to be the arithmetic mean. Having fixed the "true value," one assigns to it a range of error, that is, an interval about the mean, the size of which is a measure of the untrustworthiness of the chosen value. To be sure, the experimenter often talks as though there "existed" an absolutely true value to which the mean is the best approximation; and he is prone to say that the assigned error marks the range within which he judges the true value to be confined. This, however, had best be regarded as a convenient metaphor, for except in cases where the true value is fixed by definition (the atomic weight of oxygen is 16.000 without error!) we have no way of determining true values exactly.

Why is the arithmetic mean of a series of observations the best choice for the true value, and how is the range of error determined? In outline, the reasoning is this. There are two kinds of departures from constancy in a measured value, those called determinate, or *systematic*, and those called random *errors*. The former presumably result from mistakes on the part of the observer, in reading a scale or in recording his data, from faulty or incorrectly adjusted instruments. They are usually large, detectable, and avoidable and are therefore assumed to be eliminated in a good series of observations. Random errors, on the other hand, are indeterminate, *i.e.*, they have numerous unknown causes and are unavoidable. It is assumed that they produce a unique distribution of measured values about their mean, the so-called "normal," or *Gaussian*, distribution, which may be described as follows.

If x is the result of a measurement, then the number dN of measurements yielding values between x and $x + dx$ is, according to the normal distribution,

$$dN = \text{const}\ e^{-k^2(x-x_0)^2}\, dx \tag{6.1}$$

where k and x_0 are unknown and therefore adjustable constants.[1] The reasons for assuming the validity of this law will be presented in the next section. Once it is accepted, the following procedure for the treatment of data suggests itself:

First the measured results are plotted, the number of measurements yielding a certain x (called dN in Eq. (6.1)) as ordinates vs. x as abscissas. The plot will in general be bell-shaped and conform vaguely with the plot of Eq. (6.1). We then draw the best Gaussian curve through the plotted points and determine both x_0 and k from this curve. Now there is always some uncertainty in the choice of x_0, which is the "most popular" measured value in the sense that the greatest number of measurements lie near it; but this uncertainty can be eliminated by a theorem provable with Eq. (6.1) as the premise. The theorem states: If a set of measured values has a normal distribution, then the most probable x_0 is their arithmetic mean. We avail ourselves of this theorem in determining x_0 and then proceed to find k by curve fitting. The first of these quantities, x_0, is arbitrarily identified as the "true value," while k is called a *measure of precision*.

The greater the value of k, the narrower the graph of the normal distribution, and the smaller the number of observations departing considerably from x_0. Hence its name. The reciprocal of k has the same physical dimension as x and represents a departure from x_0, called an error. It would seem natural to take $1/k$ as a measure of the error in a given set of observations. Certain considerations from the theory of probability, however, indicate that

[1] The numerical value of "const" in this equation is not at our disposal because the sum (or integral) of all dN's must be made equal to the total number of measurements.

this would be an unduly pessimistic estimate and that a more flattering measure should be chosen. Hence scientists have accustomed themselves to using one of the following three types of error, or measures of uncertainty, each having a specific statistical meaning:

Probable error: $r = \dfrac{0.477}{k}$

Root-mean-square error: $m = \dfrac{0.701}{k}$

Average error: $a = \dfrac{0.564}{k}$

The probable error r is a departure from the mean in a normal distribution, so chosen that one half of the errors of all observations made are greater, and one half are less than r. The average error is the arithmetic mean of all errors without regard to sign, while the root-mean-square error, also called the standard deviation, is defined by its name.

There is no agreement among scientists as to which of the three is the most adequate measure of the uncertainty of observations, as indeed there cannot be in view of the arbitrary meaning of the measures. Practically, the differences are unimportant—so unimportant that scientists have hardly felt the need of getting together for a common declaration of faith in one choice or the other. But the philosopher of science is obliged to take note of this remarkable fact: both "truth" and "tolerance" must be fished out of the uncertainties of the immediately given by more or less arbitrary rules not immediately presented in Nature.

Such tolerance, or error, *makes possible* a comparison between theoretical prediction and observation which would be worse than problematic if "point" predictions had to be matched with point-like data. Hence a decision of whether a given theory is verified is practically attainable. The point P_1 in Fig. 6.1, at which the predictive circuit starts, is surrounded by a range of error. Two methods can be used in carrying through the confirmation. One neglects the error at P_1 and inserts into the theory the "true" value of the measurement; it comes out with an exact P_2. Confirmation is achieved if the exact predicted value is included within the tolerance of observations on P_2. Cautious scientists, wishing to be safe, choose the probable error as their tolerance. The second method is to take account of the tolerance at P_1, and to let the theory predict both a true value and a tolerance at P_2. If the predicted and the observed tolerance have a range in common, the theory is considered to be verified.

6.4. The Normal Error Curve

As was seen, acceptance of the normal curve as given by Eq. (6.1) is crucial in the outlined treatment of observations, and a clear exposition of our reasons for endorsing it is indispensable for critical understanding. Not uncommonly one finds a scientist justifying his use of the normal law by an appeal to direct experience, saying that a sufficient multitude of observations have yielded to the law and hence establish its truth; that the law is substantiated empirically. The weakness of this reasoning is apparent, not merely in its circularity, but more notably in the falseness of its premises. For experience presents the scientist with innumerable *skew* distributions, differing perceptibly from the normal law. These he often dismisses or corrects, because for some hitherto unstated reason he objects to them. He uses the normal distribution both as an inductive generalization from experience and as a criterion for the trustworthiness of that experience. Thus he is lifting himself by his bootstraps unless an independent argument can be given for the normalcy of that distribution.

This can be done by means of probability theory, a calculus whose applicability to observations we shall here take for granted. It is necessary to do this in order to break the vicious circle which always results when the treatment of observations is justified by exclusive reference to observations. What is left obscure will be further and more carefully examined in Chap. 13; for present purposes it is sufficient to assume that probability, a *construct* like many others and defined by rigorous mathematical methods, is related to empirical data via rules of correspondence. Failure to realize this simple fact can easily transform the probability calculus into a mass of inconsistencies.

The normal distribution can then be proved on the basis of three hypotheses:

1. There is a *very large* number of agencies, or "causes," producing random deviations.

2. Each agency produces a probability distribution of deviations which has both a finite mean and a finite dispersion.[1]

3. The total deviation resulting from the simultaneous action of all agencies is the algebraic sum of all individual deviations.

Under these conditions the probability of a total deviation x turns out to be given by Eq. (6.1). If we assume further that the number of observations yielding the value x equals the total number of observations times the probability, we have established the normal error distribution. The details of this derivation are perhaps not interesting to the philosopher, but the fact of its possibility and its premises are of paramount importance. Through them it becomes apparent that the scientist makes sense when he uses measures of precision, when he assigns errors to his observations, and when he discriminates between reliable and unreliable data. Here lie the rational roots which provide a stable anchorage for his methods and a justification for his use of norms.

The treatment given in the present and the previous section is, of course, far from complete. There are cases where good reasons lead an investigator not to apply Eq. (6.1) but some other distribution, normal under altered conditions. Questions also arise concerning the probability that a given true value or a given measure of precision be adequate, questions of the sort: What is the chance that 100 observations will yield an arithmetic mean differing by less than a given amount from the mean of 10 observations? Finally, there are numerous practical tests by which the significance of data can be determined, and there are timesaving short cuts for tedious calculations. Observational statistics form a wide and highly complex field. We have here presented a mere outline of its basic philosophic structure.

[1] For an exact definition of these terms and a proof of the theorem, see Lindsay and Margenau, "Foundations of Physics," John Wiley & Sons, Inc., New York, 1936.

6.5. Can Errors of Observation Be Reduced Indefinitely?

It is sometimes said that theoretic description is always *approximate*, never completely true. According to this view it is up to theory constantly to improve itself until it meets the verdicts of experience. Many are captured by the ingratiating frankness of this metaphysical position, which recognizes so clearly the difference between the predicted results and the measured facts of science, and which tries so adroitly to do something about it. But let us examine it again. The difference which it acknowledges is certainly there, and that difference amounts, as we have seen, on the factual level to continual disagreement. Of this disagreement, the interpretation in question speaks euphemistically as an *approximation* by theories.

But what do theoretical predictions approximate?

The view just cited involves a misunderstanding of the process of observation. It takes for granted the sanctity of a single measurement, calls it reality, and then challenges theory to name it. It assumes that theory then proceeds to writhe in agony, making many attempts to guess the truth and coming ever closer but never quite reaching it. Unfortunately this naïve interpretation fails to resolve, indeed it ignores, the ever-present uncertainties of immediate perception; it overlooks the all-important office of statistics in ordering and regularizing experience.

Theory does, of course, correct itself when confronted with any empirical fact that falls outside the range of error admissible in view of the circumstances. But it rarely does so by successive approximation. It looks for new ideas, tries to remember effects it had not included, and predicts again, never sure that the new prediction will be near the old, although it often is. Whether there is a *convergence* of theoretical prediction is in most cases difficult to say. In general, therefore, theories do not approximate data, nor do they converge upon them.[1]

[1] One often meets theories which claim to be no more than approximations to valid theories and therefore also to observational facts. Such situations arise when an exact theory, though ideally available, is too complicated for consideration or too refined for practical use. Our concern here is not with them, for approximate theories approximate exact theories, and can be recognized as

It is another thing to inquire into the convergence of observations. To illustrate what happens here we return to our former example, the length of the table. We assumed that four successive measurements, made with an instrument of considerable precision, yielded the values 61.36 in., 61.40 in., 61.39 in., 61.37 in. and suggested as a "true" value their mean, 61.38 in. There are two ways in which this result can be improved:

(1) by taking more observations with the same instrument;

(2) by using more refined measuring devices.

The first method provides us with a larger set of data, all of which, however, must fall on the same normal curve. They enable us to draw this curve with greater exactness, but they do not change that curve to one with a greater measure of precision. The advantage gained lies in our ability to establish the maximum of the normal distribution with greater accuracy. As a result, we may place greater reliance in the judgment that the mean of the set of data is the best ascertainable measure of the required length, but the probable error is not affected.

The state of affairs can be presented more formally in the following way: If the individual measurements are labeled $l_1, l_2, l_3 \ldots$, the mean of n measurements, which is $1/n \sum_1^n l_i \equiv \bar{l}_n$, will change with n as n increases. One might hope that the limit of $\bar{l}_n$, as n approaches ∞, exists. Unfortunately, this is not the case in the simple sense in which the term *limit* is used in the ordinary theory of functions. Indeed the value of $\bar{l}_n$ may fluctuate with finite amplitude even in an infinite series of observations. For this reason statisticians have invented a more elaborate type of limit, called a *stochastic limit*, the precise meaning of which is not wholly free of troublesome mathematical implications. This limit is a value toward which $\bar{l}_n$ tends to converge in such a way that the *probability* of a large deviation from that value becomes vanishingly small for infinite n. If we accept this definition, method 1 provides us with an estimate for the length of the table which may be different from 61.38 in. and has a better chance of being satisfactory; but the intrinsic probable error of our measurements

approximate only when rules by which an exact theory *could* be constructed are at hand.

has not been reduced. The convergence to a more and more accurate mean value just described will be called *internal* convergence of a set of observations.

Method 2 allows a reduction of *intrinsic* errors. It involves the use of more precise measuring instruments such as a traveling microscope or an interferometer. We do not wish to suggest here that the table is a worth-while object for the practice of such refined techniques, but as an example it will do. The first of these instruments will yield a set of values more closely spaced, like 61.367 in., 61.371 in., and so on, and the normal curve to which they are fitted will be narrower and possess a greater measure of precision. A sufficient number of measurements defines a mean which will lie within or without the limits of the probable error defined by the cruder set. If it falls without, one of the sets must be discarded on the assumption that it was affected by systematic errors. Otherwise both sets are deemed compatible, and the latter is adopted as being more significant. The same procedure is finally carried through with the use of an interferometer, and we shall suppose that a third mean has been established. Now denote the successive mean values by $\bar{l}$, $\bar{l}'$, and $\bar{l}''$, their associated probable errors by r, r', and r''. *External* convergence will be said to exist if the limits of error of the finer set of measurements fall within the range of error of the coarser set; that is, if

$$l_1 + r_1 \geqslant l_2 - r_2, \qquad l_1 - r_1 \leqslant l_2 + r_2$$

$$l_2 + r_2 \geqslant l_3 - r_3, \qquad l_2 - r_2 \leqslant l_3 + r_3, \text{ etc.}$$

A mathematical treatment of external convergence has not been developed and is unlikely to be successful because of the incidental availability of accurate measuring instruments. Therefore, the question of ultimate external convergence cannot be answered simply and must wait for the progress of scientific techniques. In practice, however, the scientist is content with a limited sequence of externally convergent observations, and his theories are pronounced correct or incorrect by reference to them. Until recently, his belief in the ultimate external convergence of all scientific observations was unchallenged. It was thought that the margin of error in physical measurements could be indefinitely reduced by choosing more and more delicate measuring devices.

A physicist writing thirty years ago would therefore have ended his account of the vagaries of observation at this point, for he had no reason to suspect that the account is incomplete. Our example was chosen to conform to the expectations of classical physics. We now know of situations in which our story must be modified.

To measure the length of a table is to determine the position of one of its ends relative to the other. Imagine a task of the same sort but on a very much finer scale: the determination of the position of one atom in the table. In suggesting this we are by no means talking nonsense, for the atom is a construct like the table, related to immediate perception by rules of correspondence which, while more complex, are no less certain. There are, in fact, well established procedures for measuring the position of an atom. Our discussion will become simpler if we consider an object of more regular structure than a wooden table; hence we transfer our attention to a crystal, like rock salt or any metal. X rays and electron beams can tell us the position of one atom relative to another in ways that are familiar to physicists and need not be recounted here. Finally, to focus upon the simplest aspect of our task, it is well to conduct our measurements at very low temperatures so that the motion [1] of the atoms will not complicate the problem. When the measurements are interpreted and recorded, they are seen to involve the usual spread about a mean and their distribution is represented by a normal curve. The measure of precision will depend on the wavelength of the X rays or electrons employed and also on the quality of the recording devices. The results will show *internal* convergence.

On repeating the operations with signals of smaller wavelengths in the hope of increasing the precision, our expectations are disappointed, for it turns out that no available arrangement will reduce the precision below a minimum which is apparently fixed in the nature of things. Nor is this an accidental occurrence

[1] The physicist may object that we cannot eliminate the zero-point "motion." This reflects a misunderstanding resulting from a careless usage of words. An atom does possess zero-point *energy* and consequently an uncertainty in position. To attribute this uncertainty to motion is unwarranted and contradicts the established quantum mechanical fact that the atom possesses no mean velocity. The apparent paradox, if it be a paradox, will be resolved in Chap. 18.

attending the imperfections of the devices used; to assume that it could be otherwise would contradict known laws of nature. We see that *external* convergence fails. Furthermore, the irreducible error may be quite large.

In the field of physics this recognition came for the first time when Heisenberg discovered the uncertainty principle, which is a succinct and formalized, but special, expression of it. In other, less mathematical sciences, a similar situation had been vaguely known and interpreted by saying that precise measurements were impossible. The psychologist has long realized the interdependence of an individual's response to a given stimulus and the individual's advance knowledge of the experimental procedure. Indeed in all biological sciences reactions are strongly conditioned by the fact of measurement. The well-known paradox of economic prediction, exemplified in the circumstance that if it were possible to forecast the rise of stock-market prices with accuracy the rush of buying would certainly make them fall, is another case in point. All these phenomena, together with the kind of physical measurement last described, reduce in their final analysis to a simple confirmation of one basic fact: Individual observations may exhibit an unavoidable scatter, or spread, about their mean; their external convergence cannot be guaranteed.

We now return briefly to our atom with its normal distribution of position measurements. Had we determined its position at a higher temperature, the observed results would not have fallen on so simple a curve. In cases of this sort the normal distribution loses its central significance and gives way to an infinite variety of other predictable distributions. That fact, however, while we thought it worth recording, does not alter the fundamental results obtained. The physicist, seeking for familiar explanations, often attributes the failure of external convergence in his science to an unavoidable interaction between the system whose properties he measures and the measuring apparatus. Such attempts to relieve the embarrassment occasioned by the discovery of "ambiguities" in Nature will be discussed and evaluated in Chap. 18.

For many purposes it is useful to distinguish quantities, like the length of an ordinary physical object, which show a high degree of external convergence on measurement, from others, like the position of an atom or an electron, which do not. It will later be shown that quantities of the former class, when used in defining the states of physical systems, lead to causal description. If states are defined in terms of quantities of the latter class, causal description cannot be achieved.

In the last few sections we have talked of measurements as though their locus were entirely in Nature and as though the passage to theory were a clear flight across no man's land. The artificiality of this simplification must have become apparent to the reader, and we owe him an apology and a correction. For it should be remembered that the term *measurement*, itself, describes not a process limited to experiences in the *P* field but a complete passage from *P* via rules to *C*. Thus length, position, quantity, and state lie in *C* but have their roots in *P*. All uncertainties, however, are confined to *P*.

And finally, perhaps to confuse the issue altogether, we recall a point already emphasized (Chap. 3). The boundary between the *P* field and the *C* field is not sharp: it can be drawn with arbitrary variations. For, after all, both *P* and *C* are parts of one continuous experience, and the philosopher must learn to view the distinction between the rational and the sensory much as the chemist sees the hazy though meaningful difference between the organic and the inorganic world.

Summary

Theories which possess the rationality and coherence imposed by the metaphysical requirements of Chap. 5 attain validity through empirical confirmation. This process represents a circuit, traceable in either sense, from perception (observation) via rules of correspondence to constructs and back along some other route to perception. In examining the details of such circuits of confirmation several important questions arise: In how many instances must a theory be verified in order to be acceptable as valid? What is meant by agreement between theoretical expectation and observation? The last question, which is troublesome because of the afore-noted intrinsic uncertainty of perception, calls for careful scrutiny of the statistical means employed in science for the purpose of smoothing data, and that inquiry is conducted in Secs. 6.2 to 6.4.

In attempting to reduce the uncertainty of data, two different points of view must be maintained. By making more and more observations, the "true" value of a physical quantity can be obtained with ever-increasing precision: the *mean* of the set of measured results approaches the *true* value. We speak of this phenomenon as *internal convergence* of a set of measurements. The other point is this: When several sets of measurements are at hand, each obtained with a different measuring device, then their mean values will not in general agree nor will their probable ranges of error necessarily overlap. Progressive agreement between such sets, as the refinement of the measuring instruments increases, is here called *external convergence*, and an attempt is made to define it.

Internal convergence is a rather general phenomenon. External convergence fails in certain parts of physics, *e.g.*, in quantum mechanics. Moreover, it may be shown that physical quantities (observables) can often be classified by seeing whether their measurements show external convergence or not. We shall return to this point in Chap. 8.

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CHAPTER 7

Space and Time

7.1. Space, Time, and Reality

Space and time are highly poetic subjects. As objects in the thought of man, they entered the history of ideas with godlike splendor and amid all the symbolic and beautiful trappings with which earlier cultures were wont to surround their idols. The awesome veneration of space and time has been continuous and has been continuously recorded, with the effect that even modern thought cannot entirely free itself from its mystic bondage to the past. The wings of Mercury have left with us a persistent reminder of the ubiquitous presence of something mysterious to be traversed, and the two bearded faces of Janus have impressed themselves indelibly upon our ideas of time. The task of the philosopher of science is, therefore, an unpleasant one, for he must ignore mythology, resist poetic impulses, and dissect with cool deliberation the factual and logical content of the terms *space* and *time*. It is upon this prosaic business that we here embark.

The philosopher must in fact do more than guard against tradition: he must renounce an easy tendency which carries ready thought so often beyond its proper destinations. When reflecting on space, the mind at once opens itself to the mysterious aspects of an unexplored astronomical universe, to a contemplation of the never ending, the wholly unoccupied; the known mingles with the unknown, and the inquiry is more likely to end in aesthetic or moral inspiration than in articulate knowledge. Space has this peculiar quality of putting greased runners under our imagination, because its familiar properties are easy to comprehend and to generalize, while so little is known about its qualities in the large.

It is the same with time as with space. Memory, on sweeping back over the past and losing itself in the cherished, shapeless events of our childhood, returns wistfully to the present and surveys the future as imagination. Here it meets no obstacles, no end. It may cling to a familiar vision such as a straight road, bare of trees and markers, running indefinitely ahead in a plane landscape devoid of identifying objects. Along this road it progresses, progresses until the tedium of motion blends with the glamour of eternity into an overpowering frustration. Time, the unfathomable, the unbounded, has been experienced! There were no objects on the road, indeed the road did not exist; hence the conclusion that time is independent of objects, that it goes on in its own right forever.

Thus has occurred the wedding of time with *Reality*. Unfortunately the union, though sanctioned by poets, must be pronounced illegitimate, and this for very good reasons. Least among them is the circumstance that the epistemologist did not pronounce the blessing; more important, perhaps, is the reason that Reality has altogether too many wives; most damning is the fact that Reality has no birth certificate. Untold confusion has arisen from man's willingness to admit Reality to the arena of philosophic controversy without examining his credentials, in this instance as in many others; for he behaves like an untutored ruffian who runs away with every argument, using fair tactics or foul. Now we do not wish to ostracize the fellow, but we do want to know his origin, his background, and his reliability. Thus we provisionally dissolve the marriage until, toward the end of the book, we have become better acquainted with the presumptive groom. We shall then find cause to reinstate the union under less romantic circumstances. For the present, we had better see what can be done with space and time in their virgin states.

The fictions surrounding them are best removed if we try to expose a cardinal fallacy that has persisted through the history of thought. It is this: "We can think things away, but not the space which they occupy and the time at which they are present. Therefore time and space are independent of things and demand a status of their own." Usually that status is then pronounced as one of independent existence or reality, and we are back at the wedding to which we already objected. But just as easily the argument can be given a more spectacular turn and lead, in its

Parmenidean version, to an impressive picture of the physical universe: an all-filling, eternal sphere reposing in absolute quiescence. Or it may give rise to controversies such as that recorded between Descartes and Henry More,[1] who debated the issue of space as divine extension. Descartes, unable to think of empty space, held that a vase whose interior is completely devoid of matter would collapse. More, however, thought that divine extension might rush in and keep its walls intact. We recall these arguments, not as examples of amusing aberrations to be remembered with a superior smile, but as honest and indeed unavoidable accompaniments of the acceptance of the reasoning above. What, then, do we mean by "thinking things away," and in what sense does space remain?

First, let us try to state the reasoning in its most beguiling forms. It can be conveyed by three rather different arguments to which one might give the names set down for them in the sequel.

7.2. The Uniqueness of Space Aside from Objects

*a*i. *The Kinesthetic Argument.* As a fact of common experience, bodies obstruct motion. They give rise to distinctive tactile sensations at their surfaces but exclude certain kinesthetic sensations to which we are normally accustomed. When the body is removed, these inhibited experiences become possible: one can move his hand in that region of space which was formerly occupied. Now the *body* owes its individuality to its character of being the focus of possible tactile experiences, numerous and diversified. When the body is gone there is still a focus of possible experiences, though these are uniformly of the kinesthetic sort. If the former gave us the right to speak of an individual body with numerous properties, the latter demand the introduction of a uniform agent, a carrier of possible kinesthetic experiences, which is called space.

ii. *The Property Argument.* Filling space is a property of all bodies, as are their color, their hardness, their odor, and their composition. It is a most remarkable thing that a body, when

[1] See More, "Oeuvres," Vol. 10, p. 184, or Burtt, "Metaphysical Foundations of Modern Physical Science, Harcourt, Brace and Company, Inc., New York, 1925.

moved away in fact or in imagination, takes with it all its properties but one: only the space it occupied is left behind. Thus it is evident that this space is not truly assignable to the body but rather is something in its own right of which the body merely partakes. If it were attached to bodies, it could not be left behind.

iii. *The Argument of Localizability.* The experience of bodies contains the quality "there." But the meaning of this local designation is very elusive, and a simple way to bring it within the compass of the familiar is the operational method, which argues as follows: On seeing the tree in front of my house, I can proceed toward it and determine its location by counting steps from my front door or by more elaborate operations. This defines the meaning of the position of the tree. But when the tree is felled, I can still proceed to its former place by the same series of steps: the tree is gone, but its position is still present. If all concepts are defined in terms of operations, one must reserve a special concept for the position, or location, of the tree, since that position can obviously be established when the tree is removed. Indeed, the original presence of an object is quite irrelevant in this procedure; one can go anywhere, stop at any point, and substantiate thus the thesis that there must be something. This something is space.

*b*i. We now turn to refute these arguments. It is of course against the precepts we regard as sound to define a body as the seat of possible experiences, and this objection would drain all meaning from the kinesthetic argument. But it is fairer to meet the argument on its own grounds. And there it collapses because it says nothing that is peculiar to space (or time). Of course, when a body is thought away, there is the possibility of other sensations; when one color is thought away, there is the possibility of, nay the necessity for, thinking another. Why, then, does not color occupy the same universal status as space? After every immediate sensation there lingers the possibility of another. The kinesthetic argument inflates one specific instance of this possibility to inappropriate dimensions and lets all others go. And it further displays a strong predilection for converting potentials into actuals in a manner often criticized in connection with medieval realism.

ii. The property argument fails if it can be shown that space is not a *property* of *anything*, and this is in fact the clear implication of modern physics. Even in its simpler branches, such as ordinary mechanics, where space enters as an element of description, position is now always viewed as a relation between at least two objects. The old conflict between absolute and relational space, while still alive in philosophic discussions, is settled and dead in science. More will be said about it at an appropriate place in this chapter; but here we note that every law of mechanics, even Newtonian mechanics, must specify a spatial *system of reference* in order to be applicable, and it thereby effectively denies the absolute character of space. But if space is a relation between objects, how can it be a property of one body, and how can it be left behind?

Another weakness of the property argument is its inaccuracy. To assign space to bodies was plausible on the old mechanical theory of nature, which viewed all bodies as homogeneous *plena* and as impenetrable. Atomic theory has left such notions pitifully weak, for it has taught us to regard only an infinitesimal portion of a body's space as being occupied by what is left of matter (elementary particles). Gases, liquids, even solids diffuse into one another, and the volume of an object has ceased to be an invariant measure. Photons interpenetrate without known interaction, and photons are truly matter by virtue of their mass. It is probably safe to say that, had all this been known in the days of Descartes, More, Newton, and Kant, the property argument might never have arisen.

Such considerations leave the assertion that bodies "own their space" a difficult one to defend; the fatal blow to it, however, comes from another direction. Granted that large-scale objects of our common experience may in a reasonable sense be said to *have* position, velocities, color, and so forth. The possessive relation is here supportable and definable. But in view of the considerations presented at the end of Chap. 6 this is no longer assertable of things having atomic magnitudes. The possessive relation, and with it the property assignment, becomes here fundamentally problematic, and the meaning of space and time takes on new features which will be explained more fully in the chapters on quantum mechanics (Chaps. 16 to 18).

iii. The argument of localizability can be attacked because of its operational tenor, for it is becoming increasingly clear that operations are not the only and often not the proper medium for defining scientific concepts (see Chap. 12). But let us face the question where it stands. We are told that the operation of walking toward something, of putting ourselves in or at the place of something else establishes the existence of this vague something. But why make the one operation of "displacing ourselves" so fundamental? It seems that one operation is as good as another. According to the argument under discussion, walking toward an object and finding it establishes its presence; walking toward an object and not finding it establishes the existence of space.

In the same way, throwing stones at an object and finding them reflected establishes the presence of the object. But are we justified in assuming a "ballistic" continuum because we can throw stones and find that they are not reflected? Or, to use still another operation, one can shout at the object and ask it questions. An intelligible response might be taken to establish the presence of an intelligent being. Would the absence of a response justify the declaration that there exists a kind of medium of intelligence? The argument thus defeats itself upon generalization.

The major consequence to be drawn from such observations is this: One must carefully guard against the temptation to hypostatize space into an entity, an entity given in and through immediate sensation, pervading all experience, and with its properties fixed externally by observation. Space is not wholly an *abstraction* in the literal sense from direct perception. We hold it to be a *construct* playing the same role as all the others. Indeed, nowhere does the constructional character of physical concepts become more manifest than in the analysis of time and space. And further, these concepts furnish a beautiful example of the multiplicity as well as changeability of rules of correspondence. For there is not one, there are many constructs called space, all of which correspond to different forms of immediate experience.

In this section we have limited our remarks mainly to space. Time can be dealt with in very similar fashion, but the positive case for an independently flowing sensory time is somewhat weaker. Only the property argument can be carried through without modification; argument *a*i is entirely inapplicable, and *a*iii is useless as it stands. The latter may, however, be given a semblance of force by doing in imagination what cannot actually be performed (we cannot go back in time and locate a past instant), that is, by replacing the steps in the former version by the ticks of a clock. Both these remaining arguments with respect to time can be refuted as was done for space.

7.3. The Spatial and Temporal Qualities of Perception

Unlike many physical quantities, space and time cannot be directly perceived. In this they differ from mass and force, to name but two examples. For there is an element in the P field called mass (the awareness of something impenetrable), there is one called force (sensation of muscular exertion), and there are in addition the constructs, mass and force, which figure importantly in scientific analysis. Time and space seem to lack that tangible quality in the P field and to present themselves under these names primarily as constructs. To be sure, they are not alone in having this distinction; electric fields, the soul, and the *élan vital* are other examples of entities that cannot be confused linguistically with elements of perception. But space and time are perhaps the simplest representatives of this class, and as such their elusiveness has been widely known and often misinterpreted.

What has been said can be put in another way: the words space and time rarely have reference exclusively to Nature. They usually denote constructs lying at some slight distance from the P plane.

There are factors in perception foreshadowing or suggesting the formation of the concepts of space and time. These factors do not stand out as unmistakably spatial or temporal but must be disentangled from other, equally vital factors of immediate experience. Crude space, for example, comes to us in at least *four* different forms which are not synthesized until they reach the level of reflection. First there is the obvious *thereness* of things, a quality which testifies to their actuality as distinct from memories and dreams, which are also immediate experiences. This quality of being "out in space" is probably the most primitive spatial abstraction from the sensory world. It becomes clearer in the second instance of spatial experience, the impression of *relative position* of an object with respect to others. Here the mere notion ofithereness somehow unfolds and reinforces itself, the awareness of space becomes more pronounced and extensive. Again we meet space in a third form, in the *size* of bodies, an aspect wholly different from the others on this elementary level of perception. For instance, it is this experience of size which introduces into the idea of space the feature of indefinite divisibility—relative position would not have suggested it. And finally there is blended into this mixture of qualities, thereness, position, and size, an element of more aesthetic appeal, a sense of *vastness* and indefinite extension that comes to us as we look up at the blue sky or out into the black night. The sensory roots of space are to be found chiefly in these four kinds of experience.

Time, too, is apprehended indirectly through certain mediate qualities of experience. These are rather similar to the instances through which space reveals itself: the mere presence of an event or an experience corresponds to spatial thereness; time relative to other events is the counterpart of relative position; interval takes the place of size, and the lapse toward eternity replaces the vastness of the sky.

All these fragmentary aspects of space and time, when united by abstraction, are sometimes called *private* or *psychological* space and time.[1] Here the unifying logical procedure already functions as a rule of correspondence and lifts time and space out of the realm of immediacies, but it is immaterial whether we regard these elementary forms of space and time as given directly in sensation or not. They lie near the P plane (Fig. 6.1) but somewhat to the left of it. At any rate, they do not possess much scientific interest. In fact the different spaces attributable to different forms of sensory experience may have contradictory properties. The eyes and feet of a weary hiker often disagree in their judg-

[1] For further discussion see A. C. Benjamin, "Philosophy of Science," The Macmillan Company, New York, 1937, or R. B. Lindsay and H. Margenau, "Foundations of Physics," John Wiley & Sons, Inc., New York, 1936.

ments of distance; lines seen in perspective as convergent are discerned as parallel by the tactile sense. On this level, there is no explanation of the numerous optical illusions. Nor is there any evidence for the various claims of geometry according to which space is homogeneous, isotropic, infinite, and so forth. To understand their meaning it is necessary to look on time and space as constructs and to investigate the rules of correspondence which fashion them into significant elements of scientific description.

7.4. Measurement of Space

Stable concepts of space and time appear when we learn to pass from the crude experiences thus far considered into what we have called the *C* field. Or, rather, these stable concepts gradually emerge as rational entities when the results of many such passages are consolidated into one unified whole. Space, as was pointed out, is not related to experience by a single rule of correspondence; but point, line, surface, angle, distance are constructs rather uniquely related to sensory experience, and out of these and certain postulates space is built. We are interested now in the procedures which link certain experiences with points, lines, and so forth, or, to put it the other way around, in the rules which allow us to "recognize" mathematical points, lines, etc., in Nature.

Some of these are so obvious, and we are so habituated to an unquestioning use of them, that we have no scruples about saying: I see a point. Strictly, of course, that is not true but is a suggestive way of saying: I see an object small enough so that it may function in my mathematical intuition as a point. I am really stating a rule of correspondence. Lines are similarly recognized as constructional counterparts of thin objects. Points and lines are often called idealizations; German mathematicians use for them the very appropriate term *Grenzbegriffe*, "limiting concepts," things not found in nature but suggested to us by them and arrived at by an ideal elaboration upon sensory concepts. There is not much more to be said about points, lines, surfaces, etc., and their relation to perception; but a great deal more about their relation to one another, and this additional information does not come from perception. Having located these elements within our comprehending experience by rules of correspondence, we have a choice among numerous logical possibilities for relating them; *e.g.*, a line is considered as made up of points, usually of an infinite number and sometimes of a finite number. The former assumption leads to familiar kinds of geometry, the latter to the more recent types of finite geometry. Immediate experience does not decide this issue directly.

A clear distinction between the rules of correspondence by means of which such things as points and lines are first established, and the rational properties with which they are then endowed, is needed also for another reason. The science of geometry proceeds deductively, starting with general propositions and going to particular ones. As is well known, it cannot define all its terms in its own discourse without becoming circular; it must assume an understanding of some terms to make its initial propositions meaningful. This situation is always recognized, and the logician acknowledges concepts like point, line, circle as indefinables or primitives in his system. The insular condition in which logic leaves such entities is removed by epistemic correlations.

Even in the insular condition constructs cannot be said to be wholly meaningless, for they are able to play certain formal roles as a result of their functioning within the context of postulates. Northrop has emphasized this point and has coined the phrase "concept of postulation" to ensure its recognition. We reserve a more careful examination of how scientific concepts attain their full meaning for Chap. 12.

Our analysis becomes more interesting when we turn to slightly more complex spatial concepts such as length and distance. Here the habitual passage from a rod to a line of definite length or from the visual impression of the distance between two objects to a number of feet is still possible but rarely relied upon. A *statable* rule of correspondence, a prescription for measurement takes its place. As we proceed to less familiar instances, the intuitive rules that were applicable for points and lines, cease to function in establishing distance: no one would think of guessing the distance between two stars or the wavelength of red light as he does the distance between terrestrial objects. Here our rules of correspondence turn into operational processes.

Now there have been and are many ways of measuring distances which need not all be discussed. The king's foot, being rarely available, was soon abandoned as a measuring rule. Equally apparent are the disadvantages of rubber tape. Physicists therefore seized upon what seemed to them the most satisfactory object for determining distances, the so-called "rigid body." The method is simple; a yardstick is laid off carefully between two points, and the number of times it has been laid is the number of yards composing the distance. Other methods are needed in other cases. Stars are located by light rays, guns by sound, fish and submarines by ultrasonics, but all these methods go back ultimately to the standard yard or meter, a supposedly rigid body in terms of which the lengths of other objects and the speeds of light and sound may be calibrated. And so the problem of measuring distances seems to be settled.

It was settled until the modern mathematician awoke from the pleasant dreams which Euclid's famous lullaby had engendered. He suddenly began to ask himself some perplexing questions, such as these: When I say the distance between points *A* and *B* is eighty yards, I mean of course the *shortest* distance between *A* and *B*. But how can I be sure I measured the shortest distance? Am I not confusing "shortest distance" with "distance along the line of sight"? Why should the two be equal? Thus he realizes that distance is not so simple a concept as he originally supposed. Very well, he says, I shall henceforth mean by distance the *shortest* distance between two points, and I shall not rely on lines of sight, for this would be tantamount to assuming that light travels along shortest paths. I will in fact lay off my yardstick along all possible paths between *A* and *B* and find which *is* the shortest. With this commendable resolution he goes to work, determines the shortest distance, and proves in doing so that light does travel along this shortest distance.

If he is a good empiricist, he will henceforth go on with this simple method and measure distances along lines of sight, for he has now satisfied himself of the correctness of that procedure.

But one day he reads Poincaré or Einstein. Doubts arise again, more sophisticated doubts. What justification have I for assuming that the rigid rod does not alter its properties when I use it in different ways? It is certainly conceivable that all physical objects change their sizes as we pass from one place to another. If our measuring rods, our steps, our eyes all change in equal proportion, these changes would not be reflected in immediate perception. The idea that all things thus conspire to delude us does seem grotesque at first, but it leaves a doubt that grows in strength with continued reflection. Poincaré's [1] example shows what is involved.

Suppose, he says, our universe were a large circular disk with a very peculiar distribution of temperatures. The center is very hot, and the boundary is at absolute zero. Imagine further that the linear size of all objects is proportional to the absolute temperature and that we, ourselves, have no sense of hot and cold. In that case, as we walk from the center toward the edge of our universe, our steps decrease in size without our knowledge, and we are forever approaching the boundary without reaching it. Indeed, we should be deceived not only about distances but even with respect to so fundamental a matter as the finiteness of our universe.

The improbability of such a universe is perhaps very much against it. The variability of hidden properties of this disklike world seems like an artificial conjecture. But wait! *Known* properties of geometrical objects do change from place to place in curious ways, and constancy is the exception rather than the rule. What, for instance, is the most probable surface? There are spheres, ellipsoids, irregular surfaces—and there are planes. Without prior knowledge we should hardly wager that a given surface, whose nature we are to guess, will be a plane; the probability that it will be an irregular surface is very much greater in view of the common occurrence of irregular objects. And yet the plane is the *only* two-dimensional surface which does not alter its properties from point to point. Furthermore, the change in properties of an irregular surface would go undetected by an observer who limited his observations to two dimensions, for curvature cannot reveal itself under circumstances in which the number of dimensions of

[1] H. Poincaré, "Foundations of Science," pp. 75*ff*., Science Press, 1946.

the manifold considered is equal to that which the observer admits.[1] Now our world is three-dimensional, and we confine our observations to three dimensions. Is it not possible or even likely, then, that our judgment regarding permanence of sizes may be in error?

We now return to our problem. The careful surveyor has measured the distance between *A* and *B* along the line of sight and found it to be shortest. But what if his yardstick were longest when laid off in that direction? It is conceivable that, when placed in some other direction, the yardstick, along with all other objects undergoing a turn in that direction, might shorten sufficiently so that it would have to be laid down a greater number of times in an even smaller distance. The original conclusion as to what is the shortest distance between *A* and *B* is then clearly in error.

Already we are coming close to the resolution of the dilemma. It is becoming clear that rules of correspondence, epistemic correlations alone do not suffice to fix scientific constructs. But it is well to pursue the details of our example another step. For the purpose of stabilizing the rules of correspondence which impart empirical meaning to the term *distance*, it has become necessary to introduce *standard directions* in which all measurements are to be made. Instead of laying the yardstick in what appears to be the line of sight, we agree to pursue the measurements along three preassigned directions, called coordinate axes. These may be Cartesian (rectangular) axes or any other set. Measurement thus yields three numbers, Δx_1, Δx_2, Δx_3, about which we have this certainty: Whatever the direction of the line *AB* may be, the coordinates Δx_i contain no ambiguity arising from the lack of isotropy [2] of the universe. Out of these coordinates, the length *AB* must be constructed.

This is as far as epistemic correlation will carry us. To go on from here, constructive postulates leading to different kinds of

[1] A being whose awareness is limited to two dimensions (left-right, fore-aft) would mistake a three-dimensional sphere, the surface of which it is free to explore, for a two-dimensional plane. See also the examples at the end of this chapter.

[2] A nonisotropic universe is one whose properties are different in different directions.

space have to be introduced. The axioms of Euclidean geometry, which were believed to be self-evident until the time of Gauss, Lobatchevski, Bolyai, and Riemann, were expressed in the equation

$$\Delta s^2 = \sum_i \Delta x_i^2$$

in which Δs is the distance AB and the Δx_i's are rectangular coordinates. We now know this to be a very special assumption, not dictated by immediate experience, rules of correspondence, or logical necessity. But we defer the discussion of the space generated by such additional postulates to the next section.

A very similar analysis is needed to determine the complete meaning of areas, volumes, and angles. Angles are relatively simple things and therefore perhaps most instructive. The epistemic elements which go into the definition of a plane angle are the length of one leg and the length of the circular arc passing through the angle about its vertex. In Euclidean geometry the measure of the angle (in radians) is the ratio of arc to leg. This, however, is not a definition demanded by sensory experience or by logic; it is again a constructive stipulation. A solid angle is the ratio of the area of a spherical surface spanning the angle to the square of the distance of that surface from the vertex. The arbitrary element in the definition is again apparent. Perhaps we have given enough detail to expose the complexities of geometry, and we may therefore be pardoned if we spare the reader the technical considerations which arise at this juncture. What has been said illuminates the epistemological status of geometry by showing how its elements come close to Nature at some points and yet owe the largest part of their meaning to purely constructional postulates.

7.5. Measurement of Time

"Time is the independent variable in the laws of mechanics." [1] This is the very best definition that can be given of time, and yet it does not define the idea completely. For whence do we get the laws of mechanics? Are they not formulated in conformity with an experience which already presumes time? These interrelations

[1] This statement is frequently found in books on mechanics.

of knowledge, the web of postulates and observations encountered here require for their understanding a separation of those qualities of time which are epistemically rooted in Nature from others which are constructively established. We first examine the former.

Our immediate awareness of time is linked to conceptual time by a choice of natural time units and by well-selected procedures enabling us to subdivide these units. Most prominent among natural time units are the year and the day. While their meaning rarely challenges the judgment of a practical person, these simple units nevertheless prove troublesome to the scientist. A brief exposition of these troubles will show in just what sense they are "given." It also indicates an interesting difference between the unit of distance and the unit of time: while experience presents us with numerous standards of distance, easily reproducible and reliably constant, natural standards of time are few in number and difficult to certify.

Roughly speaking, the year is the period of revolution of the earth about the sun. But its length depends upon the event that is chosen as reference for the period in question, and events are ill-defined in a universe in which all bodies move. Thus if one specifies the year to be the interval between one apparent passage of the sun through a point in the sky, fixed with respect to the stars, and another such passage, he obtains what is called the *sidereal year*. Its length is $365^d 6^h 9^m 9^s$, the days, hours, minutes, and seconds involved here being mean solar units (defined below). The *tropical year* is the interval between two successive vernal equinoxes; it equals $365^d 5^h 48^m 46^s$. It is the important unit in our daily lives because it remains in step with the seasons. The difference between the tropical and the sidereal year was discovered, to the everlasting glory of Greek science, by Hipparchus in 130 B.C. If the period of the earth's revolution is measured from the moment at which the earth is closest to the sun (perihelion) to the next such moment, the interval thus obtained is the *anomalistic year*, and its length is found to be $365^d 6^h 13^m 48^s$. Finally, the time elapsing between lunar nodes is called the *nodal year* and has a length of $346^d 7^h 53^m$. Though differing widely from the others, it has value as a basis for predicting solar and lunar eclipses.—It would be naïve indeed to ask: Which is the true year?

Next we examine the day, the period of rotation of the earth about its own axis. Again it is necessary to state what recurring event is to mark the end of this period. If we take it to be the time between two successive culminations of a fixed star, we obtain the *sidereal day;* if we measure it from solar noon to solar noon by noting the length of the stylus on a sundial, we get the *solar day*. Because the earth, while revolving about the sun, also revolves about its own axis, an observer on earth sees one less culmination of the sun than he sees culminations of a fixed star. The year, therefore, contains 1 solar day fewer than it does sidereal days, and if it has about 365 solar days, it must have about 366 sidereal days. More accurately, the sidereal day is shorter by 3^m56^s than a solar day.

Ease of observation makes solar time more useful than sidereal time. In view of this circumstance it is unfortunate that the length of the solar day fluctuates throughout the year. The cause of these fluctuations is to be seen chiefly in the eccentricity of the earth's orbit. To correct for them, the astonomer introduces a fictitious "mean" sun which passes through the vernal equinox at the same time as the actual sun but moves with uniform angular velocity along the celestial equator. All times referred to this fictitious sun are called *mean solar times*. The difference between mean and true solar time is known as the time equation; it is zero twice a year, in May and in September, but reaches a maximum of 15 min on other days.

Years and days are the chunks of duration which scientific ingenuity is to weave into the thread of time. There are no simple natural rhythms of convenient period in terms of which the day can be calibrated, the human heartbeat being too irregular. Early science therefore seized upon continuous motion to measure and to signify the flow of time. The water clock of the Egyptians, to which reference is preserved in the Latin metaphors *aquam dare*, *aquam perdere* (to give, to lose time), was a device similar to a sand hourglass. Its use depends on the assumption that equal time intervals correspond to equal volumes of water. Galileo is believed to have discovered the isochronism of pendulum swings while praying in the cathedral of Florence, by timing a swinging chandelier with his pulse. Reversing the situation, he afterwards constructed a pulse meter utilizing a swinging pendulum. Later, Huygens built a pendulum clock driven by means of a spring. There was now at hand a simple artificial rhythm to be checked carefully against a solar day, and the art of measuring time was greatly advanced. From then on better and better clocks were built, based on rhythms of remarkable constancy, until in our era we have learned to employ the subtle rhythm of light waves and the interval between radar pulses.

All clocks establish operational rules of correspondence between sensed durations and conceptual time. But do they solve the problem, or are we still left with an ambiguity similar to that involved in the relation between measured coordinates and length? That depends on the reasons we have for believing that clocks maintain a constant rhythm. To settle this matter we count the number of pendulum swings in a day—let us say a sidereal day because it is least uncertain. To our satisfaction, all days contain 43,200 swings, and our question seems answered, provided that we assume that all sidereal days are of the same length. But days are reckoned against the stars, and the stars have proper motions. To get around this difficulty the astronomer corrects for their motion on the supposition that their motion is uniform, for why should it not be uniform in view of the absence of forces? In the final stage, then, the mystery is pushed back into Newton's laws of motion: we *postulate* that bodies uninfluenced by forces move in such a way that equal distances spell equal times. We have, after all, not progressed very far beyond the Egyptians and their water clocks with respect to basic assumptions. There is no way in which purely constructional elements can be eliminated from conceptual time.

7.6. Conceptual Space and Time

Thus it becomes necessary for us to examine the postulational properties of space and time. As already noted, they are carried into the field of our inquiry by the need for answers to the following questions: How can *distance* be compounded from *measured coordinates?* And how can two time intervals be guaranteed to be equal? The reason we do not ask the second question about space is that space intervals can be compared by superposition, which is impossible for time; the reason we do not ask the first question about time is that it is one-dimensional, which is another way of saying that time and measured interval are one.

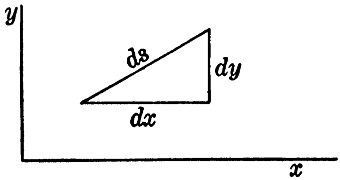

Figure 7.1

It has become customary to deal with the problem of distance by means of a mathematical device, called the *metric* of space. The reader may recall that, in ordinary geometry, the distance ds between two neighboring points on a plane, separated by dx and dy (see Fig. 7.1) is given by the Pythagorean theorem

$$ds^2 = dx^2 + dy^2$$

If these points lie on a sphere of radius r and their position is given by polar coordinates θ and φ, then the square of the length of the arc element between them is

$$ds^2 = r^2 \sin^2 \theta \, d\varphi^2 + r^2 \, d\theta^2$$

In a system of coordinates in which the axes are straight as in the Cartesian system but intersect at an angle different from 90°, we find

$$ds^2 = adu^2 + bdudv + cdv^2$$

provided that we call the coordinate intervals du and dv. These three results, and many others, can be combined by writing

$$ds^2 = g_{11}dx_1{}^2 + g_{12}dx_1dx_2 + g_{21}dx_2dx_1 + g_{22}dx_2{}^2 \qquad (7.1)$$

which is Gauss' expression for the arc element on a surface. In it, x_1 and x_2 stand for any coordinates in terms of which position can be expressed, and the quantities g_{11}, g_{12}, g_{21}, and g_{22} are either functions of x_1 and x_2 or constants which take on different forms or values for different surfaces and for different coordinate systems. By looking at the g's, the mathematician can tell whether the surface on which ds lies is a plane, a sphere, an ellipsoid, etc. They are characteristics of the surface.

Equation (7.1) can be generalized to three dimensions, in which case ds becomes a *line element*, the element of length between two neighboring points in three-dimensional or three-space. There are now altogether nine functions g, called *potentials* of the space—though only six of them are independent—and their totality is sometimes called the metric tensor, or briefly the *metric* of space. It is customary to abbreviate it with the use of the summation notation

$$ds^2 = \sum_{ik} g_{ik}x_ix_k$$

where the indices i,k go from 1 to 3. But the important question is this: What is the meaning of different choices of the g_{ik}'s?

In two dimensions, we can visualize them as defining different types of surface. Unfortunately our geometric intuition, untrained in these subtleties, abandons us when we ascend to three dimensions; and we are bound to lose ourselves in an unprincipled contemplation of what appears to be just "*space*" and remains ill-defined, unless we use the formal analysis of the metric as a guide. There are, in fact, as many *spaces*, each with a metric of its own, as there are intuitable surfaces—indeed there are more. And every one of them may be characterized by its proper set of g_{ik}'s. Euclidean space, for example, when represented in Cartesian coordinates, has the following metric:

$$\begin{Vmatrix} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & 1 \end{Vmatrix}$$

Here the reader must suppose the rows and columns to be labeled 1, 2, 3; the element g_{ik} then stands at the intersection of the ith column and the kth row. Therefore the Euclidean metric for three-dimensional space is $ds^2 = dx^2 + dy^2 + dz^2$.

When the metric differs from this, its simplest possible form, one of two possibilities occurs: either the space is still Euclidean but a different set of coordinates has been chosen for its representation; or the space is non-Euclidean. Which of these is true can be determined by subjecting the g_{ik}'s to certain tests that are well known to mathematicians and need not concern us. In the latter instance we encounter strange phenomena—deviation of the sum of the angles in a triangle from 180°, departure of the ratio of circumference and diameter of a circle from the value π, etc.—facts often represented as incomprehensible. Actually they are merely unfamiliar.

The scientist does even stranger things. Suppose that he adopts a non-Euclidean space in which the sum of angles in a triangle is not 180°. He then proceeds to measure, and he finds these angles to add up to 180° exactly. Does he then abandon the use of his non-Euclidean space? Far from it! Under certain circumstances he may say that his measuring stick was distorted or that it behaved with the customary perverseness of all objects, going through tricks to feign the result: 180°. He clings to his original metric with a tenacity that almost defies all empirical evidence. At this point the reader may have visions of ivory towers or long beards, if not of padded cells. But we must recall him from his private diversions. The scientist is quite in earnest; he means that his assumption concerning the metric is *not* open to direct test.

The choice of metric is a constructive element of scientific method, similar, to be sure, to many others but more challenging in its implications, more jolting to familiar preconceptions. It falls in the C field at a considerable distance from P. The dx_i's, together with the specifications for measuring them, link C with points in P. The g_{ik}'s, when specified as mathematical functions, cause it to be embedded in a fertile nexus of logical and mathematical relations, *based on other postulates*. These postulates determine the choice of metric, which, as we have seen, is so strangely aloof from empirical confutation. In the theory of relativity, the important postulate is one which requires that the laws of nature (not the metric!) shall have a particularly simple form.[1] This alone does not fix the metric but allows it to be fixed when it is combined with the facts of direct observation. It is to be emphasized, therefore, that the choice of metric is not made in an arbitrary fashion, nor merely on grounds of convenience, for it must satisfy (*a*) the metaphysical requirements on constructs (here in a very special form) and (*b*) empirical facts. Details of this procedure are discussed in Lindsay and Margenau.[2]

An outline has been given of the modern method for imparting specific properties to conceptual space. This will now be followed

[1] The path of a particle shall be a geodesic.

[2] *Op. cit.*

by a brief survey of historical matters and of the physical meaning of various formal procedures. Euclidean space was the space of Newton and all his predecessors. As we have already seen, the potentials in Euclidean space are *constants* (when referred to Cartesian coordinates). The physical significance of this circumstance is twofold: Newtonian space has the same properties *in all directions* and *at all points*. It is therefore isotropic and homogeneous, or *uniform;* hence objects do not change their properties anywhere. The space is infinite because the constancy of properties does not allow it to come to an end. Whether it is absolute or relative in the philosophic sense of being an "entity or a relation" the metric does not say, though Newton's intuition undoubtedly accepted the former alternative, simply because it is not contradicted by the constant properties of Euclidean space. We shall see that, in accepting it, intuition yields to persuasion by the familiar. But there is another more important corollary implied by the constant g's; it is the invariance of spatial properties with time and hence with motion. In Newtonian space, lengths are unaffected by the passage of time and by motion.

Our experience makes this assumption neither obvious nor necessary. The operational procedures for determining the dx_i's, having reference to empirical situations, imply absolutely nothing a priori about independence of spatial and temporal measurements. The miracle is not that modern physics was able to recognize and handle spatiotemporal interdependence in its theory of relativity but rather that nonrelativistic description was so nearly successful. One may see in this fortunate contingency the reason why physics, sheltered by a favorable providence, was able to enjoy the simple and well-regulated youth that led to a vigorous manhood. Other sciences have run up against relativities in their infancy.

Newton's position respecting the independence of space and time held important consequences for time, also. Space does not intrude upon time. Hence motion leaves the time scale unaffected. To quote Newton, "absolute, true and mathematical time, of itself, and from its own nature, flows equally without regard to anything external." And what he said about space was equally sweeping: "Absolute space, in its own nature, without regard to anything external, remains always similar and immovable." What he called absolute and true had now better be rendered as conceptual or, in our more specific terminology as "constructive"; it signifies the contrary to "sensed."

The modern physicist no longer subscribes to Newton's credo. He has discovered spaces which are equally consistent with observations and which allow the laws of nature to take on fundamentally simpler forms. He has, in fact, found it preferable in view of the metaphysical principles discussed in Chap. 5 to combine space and time into one four-dimensional manifold. But before discussing these more technical matters it is well for us to ponder upon the philosophic benediction pronounced upon Newton's view by Kant.

7.7. Kant's Time and Space

Rarely has a physical theory received so persuasive and so careful a transcription into philosophic terms as has Newtonian mechanics. The transcription was Kant's epistemology. An examination of the latter doctrine therefore affords us an opportunity both to record its monumental stature and also to show in what respects the flight of modern physics beyond Newton has caused Kant's view to fail. Since space and time form crucial instances of such a critique, we feel at liberty to quote here *in extenso* several pertinent passages from the "Critique of Pure Reason." [1]

1. Space is not a conception which has been derived from outward experiences. For, in order that certain sensations may relate to something without me (that is, to something which occupies a different part of space from that in which I am); in like manner, in order that I may represent them not merely as without of and near to each other, but also in separate places, the representation of space must already exist as a foundation. Consequently, the representation of space cannot be borrowed from the relations of external phenomena through experience; but, on the contrary, this external experience is itself only possible through the said antecedent representation.

[1] Transcendental Doctrine of Elements (translated by Meiklejohn), Part I, Sec. I.

2. Space then is a necessary representation a priori, which serves for the foundation of all external intuitions. We never can imagine or make a representation to ourselves of the nonexistence of space, though we may easily enough think that no objects are found in it. It must, therefore, be considered as the condition of the possibility of phenomena, and by no means as a determination dependent on them, and is a representation a priori, which necessarily supplies the basis for external phenomena.

3. Space is no discursive, or as we say, general conception of the relations of things, but a pure intuition. For in the first place, we can only represent to ourselves one space, and when we talk of divers spaces, we mean only parts of one and the same space. Moreover these parts cannot antecede this one all-embracing space, as the component parts from which the aggregate can be made up, but can be cogitated only as existing in it. Space is essentially one, and multiplicity in it, consequently the general notion of spaces, of this or that space, depends solely upon limitations. Hence it follows that an a priori intuition (which is not empirical) lies at the root of all our conceptions of space. Thus, moreover, the principles of geometry—for example, that "in a triangle, two sides together are greater than the third," are never deduced from general conceptions of line and triangle, but from intuition, and this *a priori* with apodictic certainty.

4. Space is represented as an infinite given quantity. Now every conception must indeed be considered as a representation which is contained in an infinite multitude of different possible representations, which, therefore, comprises these under itself; but no conception, as such, can be so conceived, as if it contained within itself an infinite multitude of representations. Nevertheless, space is so conceived of, for all parts of space are equally capable of being produced to infinity. Consequently, the original representation of space is an intuition a priori, and not a conception.

Critique. If the gist of §1 is taken to be the assertion that conceptual space is not borrowed from immediate experience, modern physics can only be said to confirm it. But it denies the allegation that a determinate metrical space must already exist as a logical presupposition of experience. Indeed it strongly suspects Kant's tendency to unbridled generalization, his transcendental leap from noting that certain traits are common in experience to claiming that they are necessary conditions for all experience. True, spatial aspects are found profusely in sensation; but so is, for example, the awareness of color. Why, then, is not color one of the antecedent representations of external experience?

Kant's phrase "is only possible through" must be subjected to scrutiny, for it is the gate to many unwarranted conclusions. *Possible*, even in the mild rationalism of Kant, meant *imaginable*, or capable of being thought; but we know now that imagination is a docile faculty which gradually assimilates what science teaches. Poincaré is known to have affirmed his ability to visualize four-dimensional space, and modern physics has given its disciples familiarity with highly abstract procedures which will doubtless amplify our range of intuition. The word possible has thus lost its claim to ultimacy as a descriptive term. Its meaning is now purely logical and refers to freedom from contradiction. Space *is* free from contradictions, to be sure; but this does not set it apart from many other constructs and is a lesser distinction than what Kant wishes it to be.

In §2 the faulty implications of "possible" are driven to extremes. Space becomes the *a priori condition of the possibility of phenomena*. In order to evaluate this pronouncement we are forced to take issue, once and for all, with the mystifying adjectives a posteriori and a priori. No doubt these terms, which signify an epistemological contrast, were fashioned to reflect in the epistemological sphere the logical contrast between *synthetic* and *analytic*. In Kant's works they attained a massive importance quite incommensurate with their meager substance, and they were always at the philosopher's disposal when heavy ammunition was needed in his battles.

According to Kant, a judgment can be classified in two ways: *epistemologically* it is either a posteriori or a priori, the former when its content is drawn from experience, the latter when it can be known to be true without, or prior to, experience; *logically* it is either synthetic or analytic, the former when the judgment is nontautological, the latter when it is true by definition of the terms involved. The availability of *two* apparently unrelated principles for classifying judgments gave Kant cause for concern, and he investigated carefully what happened when both principles are applied to the same judgment. To put the matter simply, let us make a table consisting of two columns and two rows, with the columns denoting the logical and the rows the epistemological distinction.

	Synthetic	Analytic
a posteriori. .	*a*	*b*
a priori.	*c*	*d*

Any judgment must fall in one of the spaces *a*, *b*, *c*, and *d*. If the entries in *b* and *c* were absent, *i.e.*, if the cross classification were diagonal, we should conclude that the two principles were, if not identical, then at least indistinguishable in applications. Kant did not find this to be the case; he believed there were entries in all the places.[1]

Spatial propositions, like the theorem concerning the sum of the angles in a triangle, were *synthetic judgments a priori*, of type *c*. Hence our knowledge of space has this same character of being positive and yet coming from nowhere, and this is the premise upon which Kant rears the idealistic view that space and time are forms of pure intuition imposed upon conception by the nature of our own faculties of comprehension.

Such conclusions are no longer tenable because the distinctions exhibited in the foregoing table have undergone important modifications. The logical division can still be maintained, though not with the absolute incisiveness which attached to it in Kant's day. Logicians and scientists have learned the importance of definitions in this respect; the decision as to the analytic nature of a given proposition depends critically on how terms are defined. If, following Poincaré, we regard the laws of nature largely as definitions, the class of analytic propositions in science is much greater than it would be if we were to follow Planck or Whitehead, for whom such laws have synthetic content. Thus, although the logical distinction has become more fluid, the terms *analytic* and *synthetic* still have respectable meaning.

[1] To be accurate, we should remark that Kant himself considered judgments of type *b absurd.*

This cannot be said, however, about the epistemological classification. Few scientists would nowadays ascribe to it any basic significance whatever. As we have repeatedly emphasized, all cognition is within experience, and while a distinction between immediate experience and a more formal kind is to be maintained, the two shade into each other without a sharp line of demarcation; they are bridged and united, furthermore, by an important type of experience here called correspondences, which are neither immediate nor purely formal. In summary, then, Kant's dichotomy of the a priori and the a posteriori has lost its basis in actual science, and we shall not use these words in Kant's specific sense. It is permissible, of course, to retain the table and to make the entries diagonal; but this is only another way of saying that the epistemological distinction agrees with the logical one.

The contents of §3 of the material quoted from Kant clearly reflect the limits of eighteenth-century geometric knowledge. Non-Euclidean geometries had not yet been discovered, and the extravagance of announcing theorems with apodictic certainty was natural to that day. All this has changed completely, and we may well dismiss §3 as expressing views which have become factually false today.

In §4 Kant shows that space is not a universal concept in the ordinary sense, a concept which "is contained in" an infinite number of particulars, but that it has a singular aspect of infinity all its own. It provides an intuition of infinity, not an infinite series of instances. Here he calls attention to a truly unique feature of space and time, one highly worthy of emphasis. But he attributes it to the space of immediate experience—all of Kant's space is the space of sensory perception—whereas we have seen this abstract potentiality to be an attribute of conceptual space only. The spatial qualities of perception involve no infinities, and indeed conceptual space may well be finite: if Kant's final conclusion were replaced by a milder one, stating that conceptual space is not compounded from immediate experiences, it would be wholly acceptable.

The inadequate commentary above cannot do justice to Kant's remarkable conjecture which installs time and space as ideal organizers of cognition. Only its failures to align itself accurately with later developments in fields from which its original support was drawn could here be sketched. Our conclusion is that this view, great in its day, can no longer be maintained; that time and space have a status commensurate with all the other constructs of physics. This conclusion will perhaps be strengthened when it is combined with the later developments in this book.

It seems unnecessary to conduct a detailed critique of Kant's views on time, for they are similar, stated in a similar form in Sec. II of the same chapter on Transcendental Aesthetics, and would elicit very similar comments.

7.8. Einstein and Minkowski

To omit so important a figure as Poincaré from the present account is unpardonable. But we intend to turn to him at some length in another connection. Moreover, the reader has doubtless discovered that much of Poincaré's reasoning has colored the preceding discourse anonymously and that we have thereby already paid him homage. We therefore move on to an examination of the ideas developed in the theory of relativity, first in its special form. Here the fusion of space and time into one four-dimensional continuum takes place.

When stripped of all irrelevancies, the circumstances which led to this fusion are very simple indeed. It was discovered, following predictions by Lorentz and Einstein, that the length of rigid bodies is different when it is measured by an observer moving relative to these bodies from what it is when measured by an observer at rest with respect to them. Similarly, clocks have their tempo changed when read by moving observers. These are empirical facts which are not subject to metaphysical interpretation; they are true and real in every ordinary sense of these words. Nothing is gained or clarified by an insistence that these unfamiliar effects are produced or feigned by new methods for measuring lengths and times, for these methods have not been altered in an essential manner, though they have been refined. In other words, nothing important has happened to the rules of correspondence. New experiences have forced us to modify the conceptual texture in which space and time had been embedded.

On the mathematical side, physics had to take cognizance of these facts in the formulation of the metric. The line element ds, or let us say its square, took on coefficients g_{ik} which were functions of the velocity of motion of the observer relative to the measured coordinates dx_i. In the same way, an interval dt had to be "transformed" when it was referred to a moving system. These relations are expressed by the so-called Einstein-Lorentz transformation formulas. Then an interesting mathematical fact was noticed by Minkowski, who observed that, when the transformed dt^2 was multiplied by c^2 and then subtracted from the transformed ds^2, that difference was independent of the transformation. The change occurring in ds^2 is precisely offset by the change in $-c^2\,dt^2$. The quantity $ds^2 - c^2\,dt^2$ is an *invariant*. (Here c is the velocity of light.)

What is more natural under these circumstances than to supply this difference with a name? And what should it be called? Suppose we use the symbol τ for ict, where $i = \sqrt{-1}$, so that the difference in question becomes $dx^2 + dy^2 + dz^2 + d\tau^2$. Now this is exactly the same as the Euclidean ds^2 (see page 141) except with respect to the number of coordinates, which is now four and not three. If we are willing to look upon τ as a fourth coordinate, and upon $ds^2 - c^2\,dt^2$ as a generalized line element in four dimensions for which we may introduce the symbol dS^2, nothing very extraordinary seems to have happened. Our space is still Euclidean; it retains its well-known properties, such as homogeneity and isotropy; only it has become more difficult—perhaps impossible—to visualize.

Let us resist the temptation to think of this new space-time as less "real." It is a construct, to be sure, but so was three-dimensional space, and so are many components of reality, as we hope to show. Ease of intuition is not a significant criterion of anything, for it can be acquired by training and has as wide a range of variability from person to person as has color vision. Whether or not the expert on relativity can learn to represent to himself a space of four dimensions as vividly as a space of three, he can think as clearly and accurately about world lines (four-dimensional paths of moving points) as he can about complex geometrical figures. And if the truth must be told, you are in fact thinking in four dimensions every time you visualize an object as *moving in ordinary space*, since such an intuition associates with the three-dimensional space continuum a fourth degree of freedom, namely, time.

Nor should one take the generalized continuum for more than it claims to be. The "discovery" of the fourth coordinate has no mysterious aspects and brings forth no ghosts. It is a discovery in the constructional sense just discussed. The fourth coordinate is *imaginary* as mathematicians use that term, and this linguistic usage has occasionally given rise to ridiculous misapprehensions in the popular mind; it has, of course, no connotations aspersing the usefulness or the validity of the fourth coordinate. As already mentioned, our universe has many imaginary and yet important properties. We do not regard an inductive electrical circuit as less real than a resistive one because the former has an imaginary, the latter a real electrical conductivity.

The use of an imaginary fourth coordinate is not forced on us; we may very well choose the real t itself for this role. In that case, however, the coefficient g_{44} is not 1 but $-c^2$, and the formula for dS^2 indicates that the resulting space-time is not truly Euclidean. Physicists use both representations, for they are obviously equivalent.

The union of space and time achieved by the new metric has an important bearing on the problem of simultaneity. If Newton and Kant were right, nothing spatial could possibly affect the time, the metric ds^2 being independent of t, and conversely; thus time would be the same everywhere, and a single instant would embrace the universe. But according to the theory of relativity time and space are in collusion, and the time scale depends on the motion of the observer. When the details of the problem are traced through—either by a physical consideration of the changes in intervals and lengths or by a mathematical analysis based on dS^2—the following is found to be true:

We consider two events e_1 and e_2 which take place at two points P_1 and P_2, at times t_1 and t_2. These events may be recorded by two different observers O_1 and O_2, one of whom moves with a certain velocity relative to the other. To make things impressive, we might suppose e_1 and e_2 to be explosions of two atomic bombs,

P_1 and P_2 two cities, O_1 to be an observer on the ground, and O_2 a person in a rocket plane. All observations are assumed to be made in the best possible manner, with the use of light flashes and with the finite velocity of light always corrected for. Let us first take the case in which O_1 records the two explosions as simultaneous. Then if P_1 and P_2 are very close together, they will also be simultaneous to O_2. But if they are widely separated in space and if O_2 is flying away from P_1 and toward P_2, e_2 will be earlier than e_1; if O_2 flies in the opposite direction, e_2 will be later. Conversely, assume now that O_1 judges the events to be separated in time by an interval Δt. They will then in general also occur at different times to O_2. If, however, Δt is smaller than the time which light would require to travel from P_1 to P_2, the observer O_2 can manage to make the events appear simultaneous by choosing a proper velocity of flight. This is what the physicist means by saying: e_1 and e_2 have been transformed to simultaneity. It is seen that the concept of simultaneity has lost its universal character; events simultaneous to one observer may not be simultaneous to another.

What is described is of course a "thought experiment" whose results are here proclaimed as facts. Even the fastest rockets are not swift enough to make the effects under discussion observable by ordinary means. How, then, can we be so sure? Because of the empirical correctness of all other observable conclusions derived from the *unified metric;* because of the way in which it hangs together with everything else we know; and, last but perhaps not least, because of its conceptual elegance. Hard-boiled classicists sometimes insist that the change in our attitude toward simultaneity has been brought about *only* by the recognition that light, the fastest of all signals, travels with a finite velocity and that, *if* a signal capable of infinite velocity were found, universal simultaneity could be restored. All this we must grant. But if our critic goes on to a conclusion which makes the restricted version of simultaneity into a mere stopgap forced upon us by our inability to find the proper signal of infinite velocity—which after all is available to *thought*—then we must object. For he is now talking like the little girl who wondered whether her wings, if she had any, would be white or blue. Universal simultaneity is a perfectly respectable construct, but apparently not a valid one.

According to relativity theory the idea of simultaneity has not lost its meaning but has become somewhat more complex. While retaining its old standing for all observers who are at rest with respect to one another, it takes on wider relations in moving systems of reference. Indeed it provides amusing opportunities for fanciful reflections based on science. One which seems not to have been exploited as evidence for immortality is the fact that a soul, if it moved away from the body at the time of death with the speed of light, would observe the last dying moment as an eternity of bliss or of agony.

7.9. Absolute vs. Relational Space. Dimensions

We wish to return briefly to the time-honored question of absolute vs. relational space. It can be put in this form: Would there be space if there were no objects? The positivist may refuse to answer it because of the unrealizable character of the state of affairs which the question envisions. Yet it seems to us that the question is meaningful and that it can be answered. The advocate of absolute space bases his attitude on the simple fact that he can intuit three-dimensional space even when it is vacant of objects. This kind of space is a possible construct, and it is absolute within the framework of the initial question. But it is not the space which scientists have adopted. It is not the kind of space which functions as a *valid* construct in the face of the requirement laid down in Chaps. 5 and 6.

These requirements do not contain a postulate of intuitability, and the earlier sections of the present chapter have indicated how far the accepted notions of space transcend Newton's simple vision. In our discussion the metric formed the focal center, and we have seen how a slight formal change in its properties sweeps far across the bounds set by intuitability. Now there exists another, nonmetrical approach to the idea of space which allows its relational character to be demonstrated still more clearly. It is the group-theoretical approach.

A group is a set of operations to be performed on a given object so chosen that, when several operations are performed in succession, the result is equivalent to just one operation of the group. That is to say, any set of operations can be undone by some other operation. One of the simplest groups contains two operations and may be illustrated by a familiar activity: pulling a chain or string attached to an electric light. The two operations are (1) doing nothing; (2) pulling the chain to light the room. The first must be considered an operation and must be included in order that the two shall form a group. We shall speak of these operations as elements. Any succession of element 1 reproduces element 1 (leaving the room dark); any odd number of applications of element 2 is equivalent to element 2 (room light); an even number of applications of 2 is equivalent to element 1. A powerful calculus, called group theory, has been developed on the basis of this and similar less trivial situations. A group is a very distinctive set of operations, and most activities do not form a group.

Now if we replace the light switch by a rigid body and the two operations of not pulling and pulling by the infinite set of all translations and all proper rotations of the rigid body, we also have a group. Not only is this interesting in itself, but the resulting group has the same abstract properties as three-dimensional Euclidean space and provides therefore a medium for its study. Sophus Lie made group theory the foundation of geometry. Poincaré was an advocate of this type of study and preferred it to the metric approach; he also indicated the way in which non-Euclidean geometries are generated by an application of group theory to nonrigid bodies.

If space is a group of operations on *bodies*, the controversy between the proponents of absolute and those of relational space has been resolved. The former have clearly lost their case. This appears to be the verdict of modern physics.

The view just formulated affects also the number of dimensions to be attributed to space. Here again, the geometer who relies on intuition "sees" the answer, three. But as soon as we curb our instincts and proceed abstractly, a different result appears. Relativity led to the use of more than three dimensions, and so does group theory. For it happens that translations and rotations of *two* rigid bodies also form a group, namely, one which corresponds to a space of six dimensions. By introducing more bodies, the number of dimensions can be arbitrarily increased.

Similar conclusions are reached through the study of ordinary mechanics, the science of moving bodies. The complexity of the relations between one body and a coordinate system is less than between two bodies and a coordinate system. Now the number of dimensions of the space required for a description of these relations is the minimum number of variable parameters by means of which they can be specified. It is this fact which is reflected in the practice of the physicist when he describes events in a space of four dimensions or the motion of n particles in a space of $3n$ dimensions. Nor is it a recent practice. Philosophers sometimes believe multidimensionality to be a novel feature, perhaps an objectionable one, introduced by relativity or by quantum or wave mechanics. It is at least as old as Lagrange, who published his famous treatise on "Mécanique analytique" in 1788. And there is nothing strange or inherently difficult in Gibbs' theory of statistical mechanics, which operates with phase spaces of 10^{24} dimensions [1] and more.

7.10. Continuity of Space and Time

Physical theories generally involve the differentials dx, dy, dz, and dt, and their presence reminds us constantly of the continuous character of space and time. However, it constitutes no proof of continuity, for the physicist often permits himself the use of differentials for quantities which he knows to be discrete. In statistical mechanics, the symbol dN stands for an integer and denotes a number of particles; in electrical problems, dq is used for an element of charge despite our knowledge of the quantum nature of electricity. Justification for such apparently paradoxical procedures comes from the smallness of dN relative to the total number of particles and of dq relative to the total charge considered. Hence, if the *quantum* [2] of length and the *quantum* of time were small in comparison with ordinary lengths and times, the successful use of differentials might not be at odds with their existence.

[1] See Chap. 14.

[2] By quantum is meant an irreducible quantity, the smallest possible length which cannot be further subdivided. Its existence would make space discontinuous. We call it the *hodon* (Greek *hodos*, path) in analogy with the word chronon (Greek *chronos*, time) which already occurs in the literature.

In recent times, the degree of indulgence in speculations about discrete "hodons" and "chronons" on the part of physicists has been an index of the failures of their theories. A notable instance of such speculation occurred when Bohr had introduced his theory of quantized energy states of atoms. The electron, according to the early theory, could reside in so-called "stationary" states. It could also jump from one state to another, but theory refused to illuminate these jumps. To save the theory, there was a temptation to push the mystery further back, to install blind intervals in the time scale, intervals during which the laws of nature were suspended. Fortunately, the impasse was only temporary, and continuous time was restored in the Schrödinger theory of atomic behavior.

A word might be said about the reasons why physicists are often reluctant to accept discreteness. If it were to be established as the ultimate property of time and space, one or the other of two drastic changes in the theoretical description of nature would have to take place. One is the recasting of all equations of motion in the form of difference equations instead of differential equations, and this is most unpalatable because of the mathematical difficulties attending the solution of difference equations. The other possible modification would involve the elimination of time and space coordinates from scientific description. Little is known about the possible success of such an undertaking, but Heisenberg has recently become a powerful advocate of it and has already carried this problem through its initial stages. His theory has profound effects upon the whole methodology of science and will be described; but before we turn to it another facet of the continuity-discreteness question must be examined.

Space and time can be discontinuous in two different ways. They may show fundamental discreteness in their very structures, or they may function as continuous backgrounds for discontinuous events. The first possibility is illustrated in the finite geometries [1] of Veblen and others; so far as we are aware they have not been considered by physicists as possible carriers of theory. Nor do they recommend themselves as likely prospects.

What is usually contemplated in physics is the second possibility, a continuous space-time of the Minkowski form, a mani-

[1] See O. Veblen and W. H. Bussey, *Trans. Am. Math. Soc.*, 7:241–259 (1906).

fold wherein points are defined everywhere but in which events form a discrete lattice. This quasi-discreteness is envisaged whenever we speak of quantization. Energies are quantized against a *continuous* ideal energy scale; indeed, quantization loses its meaning when this background is wiped out. Quanta themselves vary continuously in size, being different for one physical system than for another. (A photon of frequency ν, for example, has an energy $h\nu$, and ν may have any value; h is Planck's constant.) The situation is altered a little when we come to electric charges and angular momenta, where one encounters only one basic quantum (electron charge in one case, $h/4\pi$ in the other). But here, too, one usually thinks of it as a quantity lifted from, or projected against, an ideal continuum. When the physicist speaks of a smallest distance and a smallest time interval, it is usually this idea which he has in mind. With this understanding we return to the question of the hodon.

Heisenberg's suggestion [1] of a "smallest length" arose because of certain difficulties in quantum electrodynamics. We shall attempt a brief summary of the pertinent facts in so far as they relate to the problem of space and time, and we ask the reader to accept here without explanation a few things relating to quantum mechanics which we hope to clarify later. Hitherto all theories about atoms, nuclei, and radiation have started very close to classical lines; in fact their first step was to discover a "Hamiltonian function." This is a name for the energy of the system to be described, expressed as a function of generalized momenta and coordinates. By the rules of quantum mechanics, this function is then converted into an "operator," for which we shall use the symbol H. An operator is a mathematical construct which, when applied to a function, changes the latter into a different function.

Now the basic equation of quantum mechanics, which tells how the state ψ of a system is altered as time goes on, is Schrödingers's [2]

$$H\psi = ih\frac{\partial\psi}{\partial t}$$

where h is again Planck's constant. In the choice of H the physicist has a good deal of freedom, and he has exercised it without

[1] W. Heisenberg, *Zeits. f. Physik*, **120**:513, 673 (1943).

[2] See Sec. 17.8.

restraint. Nevertheless he has found that every reasonable choice of H leads to serious difficulties, for certain quantities like the energy of a single electron, the polarization of empty space, and several others turn out to have infinite values. Such results are, of course, not tolerable and mark the theories predicting them as defective. After many vain attempts, physicists have come to wonder whether the Hamiltonian approach, and therefore the equation above, may not have to be abandoned. Heisenberg says yes, and he gives as his reason the observation that according to Schrödinger's equation H acts like a time derivative; hence its use presumes continuous changes of ψ in time. H is inapplicable because t is discontinuous (in our second sense).

The positive contribution made by Heisenberg, though very important, need not concern us here. He eliminates t from the picture, introduces a new quantity called the S matrix in place of the Hamiltonian, and shows that it avoids the renowned infinities and allows some reasonable conclusions. Its ultimate fate is unsettled. But Heisenberg's proposal contains one further point of very great interest: it implies that nothing can be known about the interaction of particles at very small distances. The "smallest length," which he regards as a universal constant of the same basic character as the charge of an electron, he takes to be of the order of magnitude 10^{-13} cm, that is, of nuclear dimensions.

Other theories substantiate the assumption of a smallest length in so far as they fail to make sense when applied to minute domains of space. Dirac's theory of elementary particles, for instance, predicts an incomprehensible trembling motion with an amplitude of about 10^{-13} cm, which is commonly taken as an indication of the theory's failure. This, then, is perhaps the approximate value of what we have called the hodon. It happens to be equal to the "Compton wavelength," h/mc, where m is the mass of the particle.

The corresponding chronon, the smallest interval of time, would have to be of the order of magnitude 10^{-24} sec. Two lines of evidence lead to this conjecture. First, the smallest time in which the quantum of length, ρ, can be traversed is that required by the fastest signal, namely, light. That time is ρ/c, c being the velocity of light, and this yields the value given above. Second, we may compute the period of the radiation emitted when a nuclear particle of mass m is annihilated. Because of the Einstein mass-energy relation the energy residing in this mass is mc^2.

According to the quantum theory, this energy is h/τ, if τ is the period of the radiation in question. Hence $\tau = h/mc^2$. But ρ, we recall, is the so-called Compton wavelength and is given by h/mc. We therefore arrive at the same value, $\tau = \rho/c$, as before.

These considerations are still speculative; nevertheless they suggest that time and space may well be quantized in the second sense discussed earlier. If the conjecture as to the numerical values of their quanta is correct, ordinary measurement will not be affected, for they are too small to be observed. The smallest distance at present capable of measurement is the size of an atom, about 10^{-8} cm; the smallest measurable time interval is 10^{-10} sec.

7.11. Time's Arrow

The flight of time is into the future; no amount of scientific elaboration can set aside this fact which all experience undeniably affirms. Nor is this true only of our immediate intuition of time; conceptual time, too, in so far as it corresponds to sensation, is one-way time.

This is often forgotten in view of a remarkable but not decisive quality of mathematical time: it is possible to reverse the sign of t in the equations of mechanics without destroying their meaning; indeed, the equations with the sign of t reversed define dynamically possible motions. From this circumstance physicists sometimes draw the conclusion that mathematical time is symmetrical, or *two-way* time.—But on reflection this conclusion makes only artificial sense; thinking about it, one is somehow prompted to pound the table, saying: Nevertheless time does not move backward. All that the physicist can do is to *read* it backward. A printed line can be read backward, too, but this does not prove that it makes sense when thus read. Or does it?

Here we require a careful definition of terms. On analysis one sees that there are *two* ideas involved in almost every discussion of the directionality problem, and these are rarely separated. The first has to do with the *one-wayness* of time, the second with its *reversibility*. Both these terms will be explained in the following.

To illustrate the meaning of one-wayness we compare time with space in a simple, formal manner. Consider a space of two dimensions, a line. We say, a line has two directions. When we subject this statement to analysis, it reveals itself as meaning that we can move to the right or to the left, or that a body can move from a point x_1 to x_2 and back to x_1 again. This also sums up the content of the assertion that the two directions of a line are *physically equivalent*. More succinctly: Let a particle move so as to be at the points x_1, x_2, x_3 at times t_1, t_2, and t_3, respectively. We have two-way space because the following simple set of conditions can be satisfied:

$$x_3 = x_1 \qquad x_2 \neq x_1 \qquad t_1 < t_2 < t_3$$

Thus we should have two-way time if the same conditions could be satisfied with an interchange of x and t:

$$t_3 = t_1 \qquad t_2 \neq t_1 \qquad x_1 < x_2 < x_3$$

It will be seen that this is impossible because it would require the particle to be in two different places at the same time t_1. We have thus shown the presence of a genuine asymmetry of the time scale, in contrast with the symmetry of space; and we have noted how it arises from these simple facts: A body can be in the same place at different times, but it cannot be at the same time in different places. Here we find the essence of the one-way character of time, and in this respect time *is* different from space. Only by supposing that objects do occupy different places at the same time can this difference between time and space be removed.

Modern physics comes very close to this supposition as we have already indicated and as will be more fully shown in later chapters, but means other than the denial of time's one-wayness have thus far been employed to regularize the anomalies introduced by quantum mechanics. Hence time is still to be taken in the traditional one-way sense. Our present conclusion may be stated simply in this way: Time does not flow in both directions at once.—But nothing in our analysis so far prevents time from flowing uniformly backward!

Here the problem of *reversibility* arises, and this problem goes deeper than the foregoing considerations. It presents itself when one wonders how *instants* differ when all *spatial* aspects are the same. For instance, the question may be asked: If, at two different times, all properties of the universe (that is, all components of our experience including what we should call our own mental state) were precisely the same, would we not regard these times as one identical instant? This is the crucial question at issue in the controversies over the reversibility of time. Although it is admittedly academic, it is not quite in the same category with absolute simultaneity and other scientifically impossible situations—for the laws of nature do not forbid us the contemplation of a recurring universe.

If we did judge the two instants as one, then a reversal of time can occur provided that the processes of the entire universe can be reversed, and our problem becomes identical with the physical problem of *reversibility of natural processes*. We propose to deal with it fully later. Here we note that all strictly mechanical laws imply reversibility, whereas the laws of thermodynamics do not. To affirm the reversibility of time is therefore tantamount to accepting the thesis that thermodynamics is a science in its own right with irreducible laws, and this cannot be said with certainty. Whether the latter are completely derivable from the laws of mechanics is simply not known at present, since a reduction has hitherto not been achieved.[1]

At present, therefore, two opposing points of view are perfectly possible. One, based on the assumption of ultimate mechanical laws, would hold time to be reversible. It would imply that, if at any instant the velocities of all particles in the universe were reversed, we should relive all our experiences backward. This view seems to be held by G. N. Lewis.[2] It is not in contradiction with our first conclusion regarding one-way time *except at the instant of reversal*, an instant which is obviously most singular in all respects. But if we can reconcile ourselves to the possibility of this one fateful moment, the view of a reversible time is entirely tenable on the premise of the universal validity of mechanical laws.

On the other hand, the second law of thermodynamics may never be derivable from the laws of mechanics. To many modern physicists this seems more likely. There will then always be some

[1] See comments on the ergodic hypothesis in Chap. 14.

[2] G. N. Lewis, *Science*, 71:569 (1930).

processes which go on in the same direction, and progressive time is reckoned against them as a background. Even though some parts of the universe return to their former state these other tell-tale changes tick off irreversible time. Increase of entropy is an obvious directional change, and it is the one upon which Eddington[1] and many others have seized in an effort to establish the irreversibility of time. To invoke two kinds of time as Bergson did, a reversible time which plays its role in the phenomena of the inanimate world and an irreversible time for biological use, is no longer defensible. For there seems to be no reason for assuming different types of law in the two spheres of investigation. Biological laws are undoubtedly more complex than many of the known regularities in the inorganic world and, being more complex, they may indeed be inapplicable in mechanics for the same reason that the laws of crystal structure are inapplicable to single molecules. But to maintain a basic cleavage and to blame it on a duality of time is less plausible now than it was in Bergson's day.

Insistence on irreversible time is very strong in Wiener's recent book on feed-back mechanisms.[2] He feels that the statistical law, as it is known to operate in thermodynamics, has by far the wider range of application and is therefore worthy of higher rank in a methodology of science. Exact laws, like those which regulate astronomical motions, are to be regarded as exceptions to the rule, very much like instances forming a set of zero measure and therefore of no concern. But Wiener's enthusiasm for statistical aggregates and his proficiency in manipulating them have carried his judgment too far. There are, after all, some exact laws in most of physical science. From the point of view of statistics their action is a miracle, notwithstanding the cybernetic proclamation of *nil mirari*. But if exact laws are assumed, statistical regularity is *not* a miracle, and this is the reason why philosophy's interest in exact laws will persist despite the numerical preponderance of statistical regularities.

As we have noted, so long as we do not know whether thermodynamics is a branch of mechanics, whether a principle like Hamilton's is applicable to *all* constituents of nature, a decision

[1] A. S. Eddington, "The Nature of the Physical World," The Macmillan Company, New York, 1929.

[2] Norbert Wiener, "Cybernetics," John Wiley & Sons, Inc., New York, 1948.

between those views which hold time to be reversible and those which do not is impossible. If the second law of thermodynamics is autonomous, as it might be, only the second view is tenable. For all we know the universe may be periodic, running first in one direction and then in another with what might appear as an act of creation at either end. But here we must curb our eager imagination.

7.12. Are Time and Space Infinite?

On this, perhaps the most interesting question of all, present science is unfortunately noncommittal. The reason lies in the uncertainties which still surround the exact form of the metric, dS^2, for it is by an appeal to the detailed mathematical structure of this quantity that the decision as to the finiteness of time and space must ultimately be made. To specify dS^2 is not, as Poincaré would have it, a matter of convenience alone. Einstein has taught us what is meant by the correct metric, and there is very little doubt that we should recognize it as correct if sufficient observational data were at hand. These data are chiefly astronomical and are difficult to obtain; their desirability was prime motivation for the construction of the great telescope at Mt. Palomar. Of the detailed forms so far proposed for dS^2 perhaps the most successful ones imply a finite space-time. Some suggest a finite space and an infinite time; some are finite in both components.[1]

The Minkowski metric used in the special theory of relativity is infinite in both. It is satisfactory for all terrestrial purposes and comes closest to habitual intuition. But it seems certain to fail in the far reaches of the universe where the galaxies exhibit their renowned runaway motions. According to some theories, finite space can account for the recession of the distant nebulae.

Avoiding all detail, we shall merely consider here the questions so frequently asked: What is meant by a finite space? If space is finite, what is beyond it? Poincaré's example should answer them. We gave it in its two-dimensional form on page 134. Let us return to it in three dimensions. Our universe is then a large sphere with a radial distribution of temperatures, and $T = 0$ at the boundary. All objects have sizes proportional to T, and we have supposedly

[1] See A. Einstein, "The Meaning of Relativity," Princeton University Press, Princeton, N.J., 1946.

no sense of temperature, no sight. (Poincaré takes care of visual observation by postulating a special form for the index of refraction; we ignore this complication.) As we move toward the boundary, our own bodies, together with the objects we pass, are reduced in size but we are ignorant of all these changes. Our speed, though apparently the same, is actually diminished, and we never reach the boundary. If we relied on appearances, we should call our universe infinite. To be sure, there would be space beyond it, but that space would be inaccessible to us.

Another example, due to Einstein, will show how space can be finite and yet have no space beyond it. He invites us to consider a circle whose circumference is infested by creatures having cognizance of only one dimension. They can move fore and aft; left and right, up and down are unknown to them. Let the circle be very large, or else let there be no signposts telling the creatures whether they have been at a given point before. They will then conclude, after a great deal of crawling, that their world is infinite and extends in one dimension.

An animal of somewhat higher order, able to know two dimensions, will recognize the error made. It will say to the one-track creature: Your universe is finite, but it is curved in two dimensions. You don't know this because your observations were confined to one dimension.—Note that there is no one-dimensional space behind that of the creatures; the beyond is two-dimensional and therefore truly inaccessible to them.

Now let the two-track animal roam upon the surface of a three-dimensional sphere. Under analogous circumstances, it will conclude that its space is infinite in two dimensions. But man, with his superior wisdom, informs it that its space is really finite but curved in three dimensions.

Who will say that man's three-dimensional space, deemed infinite by him, is not finite but curved in four dimensions?

Summary

Chapter 7 deals first with some of the problems concealed in a very common assertion: "Real things must exist in time and space." In the beginning, care is taken to dissociate time and space from any preconceived notion of reality and to assign them their correct place in human experience. It is found that they are constructs with references to Nature through a number of rules of correspondence (Sec. 7.3). In fact, time and space are rather more abstract than many other scientific constructs since they possess no immediate counterparts in direct perception.

To establish this, various arguments purporting to show the existence of absolute space in and by itself through immediate intuition are examined and criticized. Modern physics has undeniably moved beyond Newton and has disavowed absolute space and time.

Having opened the view upon the distinction between perceptory and constructional ingredients of the ideas in question, our treatment goes on to examine both kinds, illuminating first the ways in which space and time are measured. In discussing them, the necessity for postulational (constructional) procedures at once intrudes itself, and this leads to the analysis of the conceptual aspects of space and time (Sec. 7.6). Here the important role of the metric in geometry is briefly explained.

Kant's epistemology was a transcription of the postulates of Newtonian physics into philosophic terms. For that reason it has seemed necessary to inspect and analyze with some care Kant's doctrine of transcendental aesthetics and to comment upon his belief in the possibility of synthetic a priori judgments (Sec. 7.7). The remaining sections are devoted to particular details of philosophic interest; they are designed to emphasize once more, and can hardly be understood without clear recognition of, the large admixture of purely postulational elements in the scientific meaning of space and time.

Attention is first drawn to the relativistic conception of four-space (Minkowski), then to the role played by an even greater number of dimensions in physical theories. Section 7.10 presents comments on the continuity of space and time, introducing the hypothetical "hodon" and "chronon." In particular, Heisenberg's recent arguments which have led to the assumption of a smallest length of the order 10^{-13} cm and his introduction of the S matrix are reviewed; the chronon is found to have a probable value of 10^{-24} sec.

The problem of irreversibility, when carefully considered, involves two questions: Is time just like space in possessing complete symmetry with respect to directions? Are phenomena reversible in time? The first question is definitely answerable and is to be negated. The second has received a variety of answers; final discrimination between them must await a verdict on the problem of the ultimacy of precise (mechanical) vs. statistical natural laws, a verdict which is not yet at hand. Similarly, the question as to finiteness or infinitude of time and space cannot be definitely decided at present; most cosmological theories favor a finite space. Finally, consideration is given to the customary query: If space is finite, what is beyond its boundaries?

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CHAPTER 8

Systems, Observables, and States

8.1. Does Science Describe or Explain?

Before entering upon the substance of the present chapter, which is done in the next section, we turn briefly to the question above, which will raise its head annoyingly throughout our work unless it is partially dealt with beforehand.

Whether science "describes" or "explains" phenomena is a problem we are not prepared to consider fully at this point. Only preliminary comments for the purpose of orientation will be made. To settle the problem one must first of all expose *what* is to be described or explained. Since our concern is experience, and not a presumed reality, a crucial occasion for this discussion has not yet arisen. And its solution is likely to be obvious when reality has been crystallized from the matrix of experience.

Reserving, then, our final judgment on this problem, we nevertheless recognize that the distinction between explanation and description is a genuine one in scientific discourse; some theories are *de facto* said to describe, others to explain. The former are often called *phenomenological*, the latter *causal* theories. We have already met a similar kind of division, namely, that between correlational and exact (or deductive) theories; and the difference here under consideration might for reasons of precision be identified with it. But somehow this does violence to the suggestion of polarity carried by the words *explanation*, *description*, and the identification is in fact not quite correct.

For while it is clear that all correlational sciences are descriptive (even though correlations might be established between factors which in another interpretation can be looked upon as cause and effect), the exact sciences also contain theories which describe rather than explain, as for instance the following:

a. Mendel's laws of genetics were deductively fertile; they were an exact mathematical theory despite many imperfections. However, they can hardly be regarded as more than descriptive.

b. In chemistry the theory of valence bonds occupies a similar position, except perhaps in the minds of persons who have become so habituated to the use of bonds that they are willing to endow the atoms with arms extending between them. This example is perhaps not without some interest inasmuch as it shows the effect of familiarity upon the issue here in question. Twenty years ago many chemists would have defended the theory of bond arms as a satisfactory *explanation* because they had become accustomed to thinking of it as unique and as ultimate. Today, with the electronic theory of bond formation in common vogue, one thinks of it as an interesting *descriptive* approximation to the "truth," which is conceived to lie in the reduction of the problem to electronic orbitals.

c. In physics, too, theories are constantly proposed as preliminary phenomenological devices in the hope that they will facilitate the discovery of more adequate explanations.[1] A great deal of interest attaches at present to the behavior of some substances, chiefly the superconducting metals and helium, at very low temperatures. Theories are available to account for it, theories which have made predictions with quantitative accuracy. But in so far as they involve *ad hoc* hypotheses, *i.e.*, special assumptions not derivable from other known fields, their authors are not willing to claim for them the final status of explanations.

In contrast to Mendel's descriptive laws, the modern theory which locates the genes within material carriers (the chromosomes) is usually looked upon as an explanation. The reason most likely to be given is that it answers the question *why* hereditary traits are transmitted in certain ways, whereas Mendel's laws merely show *how*. It seems unnecessary to argue at length the superficiality of this distinction, for it is perfectly clear that the why is nothing more than a disguised how. If the problem of heredity were simpler and we had observed all along how particles with specific proper-

[1] In psychology, Clark Hull's theory of "intervening variables" seems to be the equivalent of the physical theories here under study. See C. L. Hull, "Principles of Behavior," Appleton-Century-Crofts, Inc., New York, 1943.

ties had been handed on from generation to generation, we should certainly want to know *why* this happened. What now goes as explanation would then be mere description.

Although few would admit it, there is an admixture of anthropomorphism in most judgments regarding what constitutes an explanation. Heredity is an example which presents this feature clearly enough. If we wanted to make sure that a certain property appears at a certain place, we should put something there which has or produces that property, and we cannot see how nature could proceed otherwise. Hence our satisfaction over finding localizable genes. Such anthropomorphism tends to prefer mechanical artifacts and to look upon a mechanical explanation as an ideal type. As we shall see in Chap. 10, the science of electromagnetism has suffered from this mechanistic tendency, and its development has probably been retarded by it.

If we consider the two explanatory phases of heredity without preconceptions, we find nothing more than this: One is a prior stage of the other. Historically Mendel was prior to Morgan, the discoverer of the gene, but Morgan's theory is logically prior to Mendel's laws. It is the logical, not the chronological, relation which interests us here. We regard the theory of genes as an explanation because Mendel's laws can be deduced from it.

Thus it turns out that the distinction between the how and the why is primarily a logical one. As such it has an interesting corollary in the theory of knowledge, which permits the problem of "description vs. explanation" to be dealt with in another way. We have previously drawn attention to the variation in "distance" of constructs from the plane of perception; concepts can be related to Nature by very obvious rules, and in the other extreme they can be quite abstract. We mean by a *descriptive* theory one which involves constructs of the former sort; an *explanation* involves a further progression into the constructional domain. We explain by going "beyond phenomena."

If regression from the immediate is the character of explanation—and this is the view we have now formulated—then explanation can never be final. Nor is the distinction between description and explanation an absolute one; it is not always true, as it seems to be in the case of genetics, that one theory is descriptive and another explanatory. In some sciences we find more than two stages of logical reduction; as might be expected, this is especially true for problems which have had a long scientific history.

One of the simplest instances is gravitation. Aristotle presented a rather descriptive thesis according to which a terrestrial object fell because it sought its natural place. We shall call this the first stage of reduction. Galileo formalized and corrected Aristotle's assumption by postulating, and of course demonstrating, that bodies fall as they do because their acceleration is constant (stage 2). Newton generalized and corrected Galileo's theory by enunciating the law of *universal* gravitation, which holds that every particle attracts every other particle in the universe with a force inversely proportional to the square of the distance between them (stage 3). In our time Einstein generalized and corrected Newton by interpreting the gravitational force as connected with the metric of space (stage 4). And we may be sure that there will be further stages.

Here we have what appear to be four different theories, all designed to permit an understanding of the same perceptual experience. Which of them describe, and which explain? We would say that they have been arranged in an ascending order of "whyness." The significant, and presumably correct, features of stage 3 are derivable when stage 4 is used as premise or postulate; stage 2 is thus deducible from stage 3, and so forth. It is of course perfectly proper for the positivist to say that none of them explains, that each of them represents a phase of symbolic description. Yet it is customary and accurate to maintain a difference of the kind we have endeavored to establish. The sequence from description to explanation is a gradual and a never-ending one.

We did not ask which of the four theories is true. They cannot all be correct because they contradict one another in particulars. The best way to state their relative degrees of validity is to say that each of the first three is an approximation to the fourth and that the approximation is improved from stage to stage. (We are assuming here, perhaps erroneously, that Einstein's theory is borne out by present observations. This has not been settled to everyone's satisfaction.) Or we may put the matter more provocatively as follows: Aristotle's theory became false when Galileo's was accepted—Newton's theory became false when Einstein's was accepted. The latter, with the parenthetical proviso above, is now true or valid. There is very little chance that it will remain true forever. But why, after all, should scientific truth be a static concept?

8.2. The Framework of Physical Description

The sketch we have drawn of the scientific process in the broad strokes of immediate and rational experience, bridged by rules of correspondence, is too rough to serve as a local guide among the various theories of physics. More detailed maps for special territories are now to be presented. These maps must show clusters of constructs from which epistemic correlations extend to particular experiences.

No single formula can be provided which unifies each cluster. Indeed every attempt to make science appear as one uniform texture of so-called "facts" must seem artificial when judged against the fundamental diversity of the modes of description, not only in the different sciences, but even in a single mature science like physics. What unity there is arises from the way in which these facts are established and, more importantly, how they fit into a certain design peculiar to the knowing process. When our eyes are carefully focused on this process, we may perceive a fairly general scheme of description running through the whole of science, and the following developments endeavor to take cognizance of it.

The features which stand out on a topological map are mountains, rivers, and cities: emphasis upon them makes for clarity and yet leaves room for a variety of other features. In the same way, we shall here represent a number of theories with the aid of a pattern which exhibits clearly three cardinal elements, *systems*, *observables* and *states*, but again without prejudice to a fertile maze of minor constructs which supply vitality to the pattern and make each theory into an evolving enterprise.

Scientific discovery is a refinement upon ordinary perception, and we should expect to find within the cruder forms of cognition the prototypes of higher forms. Let us see, then, how we represent to ourselves the deliverances of simple sensations. We perceive a complex of colors, shapes, motions, mingled with fragrance and perhaps tactile data, all suffused with an awareness of "out there"; the whole experience is summed up in the declaration: This is a flower. The "out there," which is the part of sensation that leads to the concept of space, is merely the lingering kinesthetic consciousness that, by taking another step or in some other way displacing ourselves, we can achieve proximity with the flower. Thus, by virtue of a largely unanalyzed rule of correspondence, we transcend the realm of immediacy and construct the external object, flower. The postulation of an *external object* is the first phase of the cognitive act.

External objects form the simplest and the least complicated example of a class of constructional entities to which we shall apply the name physical system, or briefly *system*. From the present point of view, an external object serves as a carrier of observable properties, such as size, color, smell, energy, angular momentum, and so forth. Any construct which, like an external object, functions in this substantival role as a carrier of observable properties will henceforth be called a system. Examples of systems which interest the physicist are: mass points (with the properties, mass, position, velocity, and others), electric fields (with the properties, field strength, potential, derivatives of field strength, and others), electrons (with the properties, mass, charge, size, and others), and photons (with the properties, energy, momentum, frequency, and so forth). We shall sometimes say, although this usage is loose, that systems stand in adjectival relations to their observable properties.

The so-called properties are constructs, not elements of the *P* field, although they often lie very close to it. They are themselves joined to sensation by rules of correspondence of their own. This is fairly obvious in the physical examples cited, much less so in the case of so simple a system as an ordinary object. Yet it must be recognized that the property, blue, with which I invest a substantival flower is other than the sensation I have of it in a particular act of perception. The construct blue may be related very closely to the sensed blue; it may be the latter's reproduction in memory or in imagination, supplied with the quality of persistence; but it is not the spontaneous, incommunicable, rhapsodic blue that is seen. Thus, in assigning properties to systems, we are remaining within the *C* field and are not mixing our discourse.

The preceding discussion probably does violence to the psychology of perception. For it is hardly true that, in seeing a flower, we first reify that entity and then assign properties to it. Rather, the process would seem to occur the other way around: properties are postulated first, and these somehow settle upon a substantival construct. All this may be granted without occasioning any need to modify the preceding remarks or the terminology they establish.

But in one respect there is need for caution. The so-called adjectival relation must be clarified; for what appears in the simple instances of common-sense objects as an obvious and unquestionable assignment of properties to systems may be a more complex and a more deliberate act on the level of scientific procedure. We shall return to this contingency in a moment.

It would be gratuitous, of course, to follow at this stage a method of analysis which science has often proclaimed as peculiarly its own. We mean the uncritical version of the process of knowing which starts with a physical object, makes it reflect light rays which are refracted by the lens of our eyes, and, finally, ends up with a stimulus upon the retina, there to elicit neuron impulses and a final sensation in our brain. Such an analysis, while it may be entirely accurate as the final scientific version of certain types of experience, confuses the beginning with the end of the process of knowing and takes for granted the ontological status of constructs whose genesis is here at issue. We therefore return to the epistemological situation which we started to describe.

How many properties does a flower possess? The fullness of immediate experience leaves their number indefinite; to the probing investigation no limit can be set. Yet a finite number suffices to identify the object in question as a flower. This crucial set of properties, selected from an infinite number, is sufficient to induce us to apply the term *flower* to the experience of these properties. Among them are many which change in time, such as color and size, and a combination of these allows us to specify a *state* of the flower at a given time. Changing properties, in so far as they are of interest to science, are *measurable* and therefore statable as numbers. When properties have reached this degree of definiteness, they are often called *quantities*. This phraseology will be adopted here.

The popular mind accords to measurement a unique importance for science which is sometimes a bit exaggerated. It should be noted that only properties can be measured; the flower itself as an object of experience is not measurable but is the carrier of a number of quantities. In the same way science, in its higher stages, assigns measurable quantities to nonmeasurable entities, and it is erroneous to say that science confines its interest to measurable relations. An atom, for instance, is intrinsically unmeasurable but has properties which can be quantified. (Whether properties are ever intrinsically unmeasurable is difficult to decide, for it has usually proved true that science, when its interest in a given quality was aroused, has also found means for measuring it. Yet the beauty of a flower, for example, has not at present yielded to measurement, and this among other things accounts for the fact that aesthetics is not part of science.)

A heavy analysis of this sort, when conducted with reference to ordinary experience, sounds farfetched and pedantic. It is like a ten-inch mortar set up to shoot sparrows. We nevertheless thought it desirable to include it because it provides an element of continuity from ordinary sensation to scientific description and because it illustrates our use of terms. Indeed, we propose to give another moment's thought to a distinction which may at first seem equally labored but which will later turn out to be of crucial interest.

Objects enter into reflective experience in what was called a substantival role, and properties cling to them by adjectival relations. The precise meaning of adjectival relations will now be carefully examined. Speech is rather ambiguous in expressing them; it permits the simple unspecific proposition (B. Russell's "descriptive phrase"), the blue flower, to be transliterated into, the flower has the color "blue." The two statements mean of course the same thing, but there occurs in this transition a barely noticed amelioration of an incidental adjective into something *possessed* by the object with some sort of continuous and continued ownership. This is more than mere sensation warrants, for the flower is blue only when it is perceived. The reader has a right to know why we insist on this with seemingly neurotic tenacity; it is because modern physics is incomprehensible unless this fact is recognized!

On the plane of ordinary perception no harm is done by assuming the assignment of properties to be a possessive one. However, if we wanted to be perfectly accurate we should have to say: The flower is invested with an *observable*, named color. It does not have this color at all times but yields it when it is seen. The observable is a kind of abstract quality, assigned as a latent attribute to objects; it somehow fills itself with content at the instants of observation. The need for this more careful statement is clearly absent in cases like the present, where the observable invariably yields the same specific content, namely, blue. If the same flower were sometimes yellow and sometimes red in a series of consecutive observations, the interposition of an abstract container of possible properties, called observable, to be filled by specific observations, would indeed become necessary and the possessive relation would be false. Atomic and subatomic objects are like a flower which changes its appearance on repeated instances of looking; observables take on different values on different occasions and are yet in another sense unique. We see that only when properties are relatively invariable may we say the object possesses them; otherwise they must be assigned as observables, the values of which emerge on observation. It is as though the flower had a latent color, to be sure, but as though its color, a mere possibility, took on a definite value only by *interaction* with an observer. In quantum theory, the position of a particle is such an observable; in general its numerical value cannot be assigned uniquely.

To sum up this rather unfamiliar business: We have recognized a necessity for refining the usual interpretation of an adjectival relation. This is the relation between a system and an observable. Either the observable may be *possessed* as a property, or it may be *latent*. In the first case we can afford to call the observable a property or a quantity in the ordinary sense. In the latter case we speak henceforth of latent observables. When the difference is to be emphasized I shall use the names property-observable for the one, latent observable for the other.

Classical physics is a branch of science in which failure to distinguish between properties and observables is not fatal to its philosophy, in which the possessive assignment of quantities to objects invites no practical difficulty. It has three major branches, mechanics, electrodynamics, and thermodynamics,[1] and we shall now turn our attention to these. In each of them there will be discerned physical objects with property-observables. But the physical objects are not necessarily of the simple kind thus far considered. They may also be such elusive things as gravitational or electromagnetic fields. Yet they always function as carriers of observables and are nonmeasurable themselves. We shall call them *systems*. Their property-observables change in time. Being further removed from immediate perception than the flower of our example, their properties often do not enjoy the richness of intuitive experience, and the number of significant properties may be small. A number of observables such as number of stamens and pistils, color, shape and composition of petals may be combined to form a *state* of the flower. That state is readily visualized in this instance. But when the observables are latent ones, as in the case of an electron, their composition may yield a state which is highly abstract and not representable in visual terms, as we shall see later.

In simple perception one is usually not greatly concerned over how many properties form a state; practical considerations decide when we have looked enough to know that the object before us is a flower in a certain stage of its bloom. In more intricate problems of science, however, where the full background of intuitive reference is lacking, it is necessary to set down precisely what and how many observables are chosen to define a state. To do this, criteria for sufficiency must be invoked, but these are best explained by means of examples. I fear that we have already overdone a job—innocently meant to be a general introduction to the problems of scientific description—by packing the preview with most of the action and much of its significance.

[1] Acoustics and optics are, of course, parts of mechanics and electrodynamics in the wider sense.

Summary

When the theories of science involve constructs lying near the plane of perception, they are said to be descriptive or phenomenological; when they penetrate more deeply into the constructional realm, they are said to provide explanations. There is no intrinsic difference between scientific description and explanation. Such points are illustrated by reference to physical theories dealing with gravitation.

There is a general schema running through all of physical description, or explanation. The epistemological process usually starts with the construction of physical *systems* (particles, waves, electromagnetic fields—in general any external object is a physical system in a looser sense) which serve as carriers of certain properties. The properties of interest to science are called *observables* (*e.g.*, among the observables of a particle are its mass, position, velocity, energy, possibly its color, odor, and so forth).[1] To understand atomic physics it is very essential to regard observables as *not* simply possessed by, or permanently assigned to, systems. Thus the concept of *latent observables* is introduced; upon it the whole theory of quantum mechanics is founded.

Finally, a certain set of observables is chosen for the purpose of explaining or describing the nature of the system. This set is said to define the *state* of the system. The reasons for selecting a certain set, and not all observables, in formulating a state are left for consideration in later chapters, chiefly in Chap. 19.

Selective Readings

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[1] The system-observable apposition may be viewed as a modern version of Aristotle's metaphysical substance-predicable relation.

CHAPTER 9

Physics of Discrete Systems

9.1. The Method of Mechanics

Mechanics, sometimes vaguely called the science of motion, is a discipline which deals with all experience that can be represented and understood with the use of three kinds of *system*: (1) mass points (more loosely called particles) or sets of mass points; (2) rigid bodies; (3) deformable, continuous material media. Accordingly, the subject has three large branches, point mechanics, mechanics of rigid bodies, and mechanics of continua. It is true, of course, that these systems are often considered in motion, but the conditions under which they are at rest are just as important as is a description of their motion. We shall take up the three subjects one after another. Most space will be allotted to the first because it sets the stage for the other two, many of the principles of point mechanics being utilized in the other fields.

A mass point, or particle, is a very convenient idealization which can be set in correspondence with a great deal of our experience. In practice we often forget its idealized character and take it to be an object in the usual sense, like a stone or a flower. It is indeed one of the most obvious systems of the whole of science and has its place in the C field very near the P plane (cf. Fig. 6.1). From the infinite variety of properties (position, color, odor, taste, perhaps its chemical composition) which might be assigned to it, mechanics chooses a very limited set as being of physical interest. A mass point has one intrinsic property which never changes (we are not at present concerned with the relativistic modification of point mechanics), its *mass*; in addition it possesses *position* and *velocity*. If the particle has its full range of motion, each of these must be broken down into three individual properties or components, the position into the three coordinates x, y, z measured from some chosen origin, the velocity into its components v_x, v_y, v_z. In the final analysis, then, the number of significant properties is seven.

It is possible and often convenient to reduce it by one. This is achieved by inventing a combination of mass and velocity, called momentum. Like the velocity, this is a vector with three components: $p_x = mv_x$, etc., m being the particle's mass. In the immediate sequel we shall use this simpler scheme and work with momenta rather than velocities.

A particle is said to have three degrees of freedom. These represent the minimum number of coordinates in terms of which the position of the mass point can be stated; it is a fixed number independent of the choice of coordinates. To wit, in rectangular coordinates, the three numbers needed are x, y, z; in spherical coordinates they are r, θ, φ, and so forth, three in every system. This is the *minimum* number because the position could also be recorded by giving x, y, r, and θ, one of which is superfluous. If a particle moves in a plane, it has two degrees of freedom; on a line it has but one. Thus it would seem as if the number of degrees of freedom were identical with the number of dimensions of the space in which the mass point moves—a rather common fallacy. For if the point moves along a preassigned curve which is twisted in space, like a roller coaster, its motion is in three dimensions and yet it has only one degree of freedom since only one coordinate, namely, the distance from its starting point, would be required to indicate where it is. For the following we note that motion with one degree of freedom is not the same as motion in one dimension and follows in general quite different laws.

Returning now to the six significant properties of a mass point, we note that they are possessed, not assigned as latent observables. We assume a degree of consistency in the behavior of the particle which will allow us at least to expect this: No matter how the point moves, if in a very small interval of time a number of observations on its position and on its momentum are made, they will all yield nearly the same values of x, the same values for y, for z, and also for each of the components of momentum. Of course it is perfectly proper for the particle to change these values in time, as it naturally will when in motion, but we ask here that these observations be made in an interval so short that its progression has been negligible or, if it seems more plausible to the reader, that many observers note the properties at the same time. In the latter case, they will all report the same values. After all, actual particles of visible size do behave in this consistent way.

The six properties under discussion form a *state* of the system. But why? If we are not going to include the smell and the taste of our particle, perhaps one should not be so meticulous as to include its momentum, for it seems, offhand, that its position alone would do. To see the answer one must probe further into scientific method. For it happens that a knowledge of its present position alone is not sufficient for the prediction of the future or past positions of the particle; knowledge of position, momentum, and smell is too much, and knowledge of the six properties in question is just enough.

By this is meant that *laws* have been discovered which are self-sufficient with respect to states defined in terms of these six quantities. Without these laws, sufficiency would not have been in evidence. We shall see in a moment how the mechanical laws operate, but it seems well to emphasize at once the interplay between states and laws: states can be defined in a great variety of ways; to be significant, the quantities composing a state must be so chosen that available laws fully mediate between them at different times. This apparent regressus, this peculiar interdependence which allows no states to be accepted as significant before laws regulating them are known and no laws to be pronounced as valid before states have been defined, makes the start of every science extremely difficult, makes it indeed an act of genius. Were it not for this dilemma, any field of knowledge could be converted into an exact science by accurately defining states.

To see more clearly how the whole scheme works we shall discuss the details of Newton's theory of point mechanics.

9.2. Newton's Mechanics

In so far as it affects the motion of a single particle, Newton's theory is summed up in what is called his second law:

$$m\frac{dv}{dt} = F(x) \qquad (9.1)$$

By writing it so simply we are applying it to a particle moving in one dimension, the x direction, this being satisfactory for our present purposes. And we are also returning to the original states compounded from position and velocity. Here we have in addition to the fixed property m a reduced set of two variable quantities, x and v, the latter being dx/dt. The function F, whose argument is the particle position, is called the force; it is assumed to be given.

The equation above is a typical law of nature. It is a differential equation in which the independent variable is the time; and the time does not appear *explicitly* in the law. It enters only through the derivative. But the variables of state, x and v, do appear. Consequently this law of nature does not tell what happens in detail. It presents an account of processes, as it were, in nuclear form, for the law must be solved before it becomes explicit. The solution of the differential equation has the form $x = f(x_1, v_1, t)$, and this will be called the *equation* of motion.

The law of motion is in this case an ordinary differential equation of the second order in x and t. As is well known, the solution of such an equation introduces two constants of integration which may be taken as the value of x itself and the value of its derivative at some arbitrary fixed time t_1. These two are here denoted by x_1 and v_1, and they appear as parameters in the equation of motion. An interpretation becomes possible only when both are given. We see, then, how the law decides what number of quantities is necessary and sufficient for the definition of a state: being a second-order differential equation, it needs supplementation by two data in order to become descriptive of particular situations: hence these data define a state.

In other branches of science, laws of nature are not of necessity differential equations, nor are variables of state related to laws in so succinct a way. Mutual dependence of states and laws, however, is always present. Thus discovery in science is a dual event involving both the selection of crucial variables and the establishment of relations between them, as we have already noted.

If a particle moves without constraints in three dimensions, there are three laws of motion similar to the above. Each law introduces two constants of integration, and the entire situation can be handled by means of six numbers. When these numbers, that is, x, y, z and v_x, v_y, v_z, at time t_1 are given, then x, y, z are known at all other times as functions of t. By differentiation, v_x, v_y, and v_z are obtainable, so that we are in possession of a complete description of the way in which the state varies in time.

The word *law* as it is used in science has a considerable variety of meanings. In mechanics, however, it is customary to apply it to differential equations and to other propositions of equal generality. It is not good practice to confuse the laws with the afore mentioned equations of motion, which are specific solutions or applications of the laws, equipped with special constants of integration. But the laws themselves can be stated in a variety of forms, some of which will be discussed in the next section. First we wish to draw attention to their equivalence with conservation principles.

By simple integration it is seen that the law

$$m \frac{d^2x}{dt^2} = F(x)$$

leads to the relation

$$\frac{1}{2} mv^2 - \int_0^x F(x)\, dx = \text{const}$$

Logical'y the two equations have exactly the same content. In the latter, however, it is customary to define the quantity $\frac{1}{2}mv^2$ as kinetic energy, $-\int_0^x F(x)\, dx$ as potential energy, so that the relation becomes a statement of the conservation of energy. It is thus seen that conservation of energy, at least in this simple instance, is not a new principle of nature but an interesting way of stating the law of motion.

Another equivalent way is this: On integration with respect to time Newton's law takes the form

$$mv - \int_0^t F\, dt = \text{const}$$

The first quantity on the left is interpreted as momentum; the integral $\int_0^t F\, dt$ is called the impulse of the force. The result therefore states the well-known fact of a constant difference between momentum and impulse, another conservation principle. Laws lead automatically to conservation principles; they are equivalent to conservation principles. The constants appearing on the right-hand side of the last and the second last equations are often called *constants of motion.*

The situation is not always as elementary as that encountered here, for we have restricted our attention to motion in one dimension. In two and three dimensions similar conservation statements can be derived; they may, however, be quite uninteresting because of the analytic nature of the integrals encountered.[1] Only under certain further conditions do they have satisfactory properties, and only under these conditions are they used. Nevertheless they are again derivable from Newton's laws when these conditions prevail.

By way of summary it is desirable that we abstract from the method of point mechanics, so far as it is now explained, its philosophically salient features. Most notable is the construction of a type of system called particle which is related to perception by special rules. The specificity of these rules of correspondence at once stabilizes the construct for use and limits its relevance; it is through them that laws become restricted in their range of application. They permit us, for example, to identify a small stone with a mass point, but not an airplane with a mass point. The system is endowed with six properties each of which can vary in time, and these properties form a state. When a state is specified, the law of motion regulates its propagation in time. In more extended version, the law allows the prediction of future states when the state at present (or at any other instant) is known. From the predicted state one may go by similar rules to Nature and verify the prediction. The theory of particle mechanics is correct, and the constructs which it involves are valid, because this circuit of verification has been successful in numerous instances in the past.

The account which has been given assumes the force function

[1] Except for special forms of the force functions, which for motion in three dimensions depend on x, y, z, the integrals are not single-valued functions of these coordinates.

to be part of the law. Particles subjected to different forces are therefore subsumed under different laws. This is not the customary interpretation of Newton's law, which is taught in the form: "Force equals mass times acceleration." In the sense we advocate, this statement is nothing more than a definition of force and not a law. It becomes a law when a proper force function is inserted. These matters, however, will occupy our attention later when we consider the role of definitions in a more general way.

9.3. Integral Principles

Forces are not "taken" from Nature; they are constructs so designed as to make the differential equation which requires them a correct one. Like all scientific constructs they are ultimately linked to Nature by epistemic correlations (which accounts for their intuitable character as pushes and pulls), but their methodological origin lies in informed guesses as to what kind of function will make the equations work. This circumstance adds a third element of conjecture to the uncertainties already involved in the formulation of a law: at the start the physicist does not know the form of the law or the variables which it is to connect, nor can he be sure of the force function involved. If a crude figure of speech is allowed, one may say that, in order to discover a law, he must lift himself by three bootstraps.

In view of all this one begins to wonder whether force laws, thus spun together, are at all unique. Are there not other functions which, when introduced into mathematical equations of a different form, will also reproduce experience? There are indeed; and to one class of them we now wish to draw attention.

As an example we select a law of motion called Hamilton's principle, enunciated in 1824 by Hamilton in pursuit of some earlier ideas of Lagrange. He found that it is possible to determine a function L of the variables of state (again our former set of six) such that

$$\int_{t_1}^{t_2} L\,dt \tag{9.2}$$

has a stationary value. Almost no generality is lost if we assume this integral to have a *minimum* value. To understand what it means we consider again the motion of our typical system, the mass point. We know its state at the time t_1, and we wish to know its state at t_2. This knowledge is conveyed to us by the function L, called the Lagrangian function, which it is the business of this science to determine. For the moment assume it to be given. Then we still do not know what path our particle will take as it proceeds from its state at t_1, namely, from the condition characterized by x_1, y_1, z_1, v_{x1}, v_{y1}, v_{z1}. Hamilton's principle asserts that it will take the path along which the integral (9.2) is a minimum. Hence, in order to find it, we must perform the indicated integration over all imaginable paths and see which yields the smallest value. It is difficult here to remain insensitive to the obvious implication, frequently stressed by philosophers, that nature in her superhuman wisdom automatically selects a minimal path (in the sense of this principle) which human ingenuity can determine only after considerable labor (see Chap. 19, Sec. 11).

In practice one need not perform an infinite number of integrations to determine the minimal path. The calculus of variations is a mathematical discipline designed to handle problems of this sort; with its aid it is easy to write down immediately a set of conditions ("Euler equations") which the minimal path must satisfy. And strangely, these conditions are identical with Newton's laws [Eq. (9.1)].

Moreover, one perceives when going through the analysis that the Lagrangian function for the case here considered is nothing other than the *difference* between the kinetic and the potential energy, both of which are obtainable when the force function F of Eq. (9.1) is known. The use of Hamilton's principle is thus seen to be largely equivalent to an application of Newton's law and leads to no new material information. To some it seems a mere camouflage of Newton's laws, to others an interesting way of stating them, fraught possibly with far-reaching philosophic consequences. Later we shall see that differential equations, like Eq. (9.1), are representative of *causal* analysis whereas integral equations involve teleological considerations. For the present we are content to relate the facts of the situation.

The laws of point mechanics can be stated in two entirely different forms, as differential and as integral laws.[1] Each form contains initially unknown functions (F and L) of some or all of the variables of state, functions which experience only can yield. Both types of law make the same prediction, which is verified by observations. Finally, and this will later prove to be of some importance, it is possible to show mathematically that *the two forms are equivalent in the following sense: Eq.* (9.1) *is the necessary condition for the minimization of integral* (9.2).

Throughout the present and the preceding sections one simplifying assumption has been made. The forces on the particle have been treated as conservative ones.[2] Newton's laws as well as Hamilton's principle can be extended and made applicable to nonconservative cases. In the solution of concrete problems this is often a useful generalization, but it is of little interest here. For if a system is not conservative, it can always be made conservative by an inclusion of other systems—provided that mechanics is to be able to deal with it at all. For example, the motion of a single pendulum is conservative; if one pendulum is attached to the bob of another, the motion of one cannot be described by conservative forces. But if both are included into one system of two mass points (cf. the next section), we have again a conservative system. The availability of systems which are both finite in extent and conservative (systems with these two attributes are sometimes called "closed") is a basic postulate of particle mechanics.

Newton's second law and Hamilton's principle are specimens of the two classes of laws of motion called differential and integral (or minimal) principles. Each class contains numerous other formulations. Among differential principles we may mention D'Alembert's and the equations of Lagrange. Both can be shown to be reducible to and derivable from Newton's laws, but they are often more convenient in use than the latter. They enjoy particular favor in situations where a particle moves under the action of given forces but is constrained to remain on a surface

[1] The further possibility of expressing them in the form of conservation equations, discussed in the last section, is here left out of account.

[2] The mathematical reader will recognize this as implying two conditions: (1) F is a function of x, y, z alone, not of t and v_x, v_y, v_z. (2) The curl of F vanishes.

or a curve. Here D'Alembert's principle leads to a solution more directly than Newton's laws.

Other well-known integral principles are the principle of *Least Action* and that of Fermat. The first of these is similar to Hamilton's but uses a different function in place of L and also a somewhat modified integration. It seems to have been first introduced into mechanics by Maupertuis (1747), though in a somewhat defective form. Certain it is that Maupertuis announced it with enormous vigor and with an exaggerated sense of its metaphysical importance, for he saw in the circumstance that bodies move with the expenditure of a "minimum quantity of action" an exemplification of the perfect wisdom of God.

Fermat's principle of least time, here listed as a law of mechanics, actually deals with the propagation of light in media having a varying index of refraction. It may be expressed in the form

$$\int_{s_1}^{s_2} \frac{ds}{v} = \text{minimum}$$

provided that s represents the distance traveled by a light ray and v its velocity. Snell's law of refraction can be derived with its use, but the principle is now known to be inexact because it fails to take account of the wave nature of light.

No attempt is here made to distinguish between a *law* of motion and a *principle* such as Hamilton's or D'Alembert's. In using the latter term we are following a nomenclature sanctioned by the history of physics, and we wish to emphasize the absence of any logical or generic difference between these formulations. Properly, they should all be called laws of motion in order that the word principle can be reserved for more general theses like relativity and causality, theses which cast their restrictive effects upon many different laws.

9.4. Many-particle Systems

To illustrate the use of systems, observables, and states the preceding developments are quite sufficient, and we might well pass off the mechanics of particle aggregates as mere elaborations upon the principles already discussed. We prefer to include the subject nevertheless, for it happens that here, in the transition from a single mass point to aggregates of mass points, is hidden the substance of a frequent but poorly analyzed assertion, namely, that in mechanics *the whole is no more than the sum of its parts.* What may be meant by it will perhaps become clear if we briefly examine how composite mechanical systems are formed and how their behavior is described.

Sun and planets form such an aggregate, and the molecules of a gas form another. The latter is perhaps the more interesting from our present point of view. Considering it, we observe first, although it seems trivial, that the total *system* is a spatial juxtaposition of simpler ones. Here is one of the characteristics of mechanics not shared by other disciplines. In electrodynamics, for example, where a *field* represents the simple system, a more comprehensive system is not produced in so simple a way. As a result of this difference it is possible for two fields to annul each other, but two particles, though they may be joined in intimate union, always remain two. In a simple arithmetical sense, therefore, a mechanical system is indeed the sum of its parts. Nor is this wholly trivial. It could not be said of an aggregate of photons mixed with material atoms, for photons are constantly absorbed and reemitted, and their number is therefore indefinite.

To show how the behavior of mechanical aggregates is described we return to the molecules of a gas and use the method of Newton, granting of course that any of the other "principles" could also be employed if suitably extended. Again we must try to find a function which represents the force on one given particle, but this function will now depend on the coordinates of all the other particles as well. Let there be n molecules in the gas, and consider one, labeled by an index i, whose mass is m_i. Its law of motion is

$$m_i \frac{d^2 x_i}{dt^2} = F_{ix}(x_1 y_1 z_1 \ldots x_n y_n z_n) \tag{9.3}$$

together with two others for the y and z components of motion. Altogether we are called upon to solve, simultaneously, $3n$ second-order differential equations like (9.3). Notice that all dependent variables $x_1 \ldots z_n$ occur in every one of these equations, a feature which renders them in general extremely difficult, if not impossible, of solution. Only for two particles can exact solutions be found.

The physicist avoids this mathematical impasse by using what he calls *two-body forces*, and he thereby effects a very great simplification in Eq. (9.3). Relying upon a fairly general experience[1] he assumes an F_{ix}, not of the complicated form indicated above, but one which is the sum of *pairwise* interactions. Mathematically this amounts to making F_{ix} a sum of functions each of which contains the coordinates of only two particles, one of them being the ith. Even with this very radical simplification Eq. (9.3) cannot be solved exactly for n particles ($n > 2$) except when one special kind of force (Hooke's law force) is adopted. Yet, while the solution of the equations of motion may not be possible, certain approximations to them can be obtained, and certain general propositions respecting the total energy of the system and the average distribution of its molecules can be demonstrated. Many of these results are of interest in statistical mechanics, a discipline with which, however, we are not concerned at present.

Is the aggregate *behavior* of the gas still accounted for by the individual behaviors of its constituent molecules? This is a second and perhaps a more significant way in which the question as to the whole and its parts can be asked. And here, surprisingly, we get one answer if we look at the laws of motion, another if we look at their solutions. For provided that we employ ordinary two-body forces, the laws of motion are nothing but a tedious repetition of similar differential equations, each of which appears to reassert for molecule $i + 1$ what the former said about molecule i. Certainly, the statement of the basic *laws* for n particles involves nothing that transcends the laws for two particles. Their solution, however, involves features that are wholly unexpected.

Take, for example, the chemist's concept of a phase, or state of aggregation. It has no reference whatever to a single molecule—

[1] Suppose molecule 1 is in the neighborhood of molecules 2 and 3. We ordinarily suppose that the total force on molecule 1 is the vector sum of the forces due to 2 and to 3, each being a function of the distance of separation of the interacting pair. This corresponds to the action of two-body forces. If the presence of molecule 3 made a difference in the force of 2 on 1, the relative position of molecules 2 and 1 remaining unaltered, we should be confronted with a three-body force.

the molecules of argon and most other nonpolar substances are quite the same whether they are in the gaseous, liquid, or solid state. Nor do the forces between them predetermine the phase, for they, too, are the same in all three states. Obviously, phase is a concept referable to aggregates and not to single mass points; it expresses group behavior not contained in the character of individual states, nor in their laws. For the same set of Eqs. (9.3) describes the gas, the liquid, and the solid. What determines the phase is a set of parameters like temperature and pressure which again are blind to the behavior of individual molecules and have meaning only for many. We repeat, then, what is so obviously true: When attention is turned to the descriptive properties of mechanical aggregates, the sum is indeed different from its parts; and since it contains features (*e.g.*, phase) that are meaningless with respect to parts, the sum may well be said to be more than its parts.

Viewed from a logical standpoint all this is very strange. For how can you get new features by the analytic process of solving differential equations? To set things straight we should acknowledge that the qualities called new and emergent (like phase—temperature and pressure of an assemblage of molecules) are *logically* implied by the laws regulating individuals, but this does not prevent them from having the *psychological* character of novelty which the term *emergent* expresses. Knowledge need not be synthetic, in the logical manner, to be interesting or surprising, or even revolutionary. Every person working in theoretical science develops at times a feeling of genuine amazement at the creative consequences of purely reflective activities—their analytic nature notwithstanding.

We have noted what can come from the simple assumption of two-body forces. The mechanics of many-body forces is a field hitherto largely unexplored: Primakoff and Holstein [1] have considered some of their aspects in connection with nuclear problems. It is certain that the complexity of aggregate phenomena engendered by the use of such forces is extreme and that many new aggregate properties can be made to emerge. And all this seems possible without departing from the fundamental attitude of particle mechanics with its characteristic systems and states and

[1] H. Primakoff and T. Holstein, *Phys. Rev.*, 55:1218 (1939).

its postulate of spatial juxtaposition. Perhaps the biologist who frowns upon mechanical explanation as inappropriately simple and naïve, as some do, had better take another look. Mechanics is usually abandoned as a means for understanding organic systems, not because it is too simple, but because it is too complex and difficult.

Finally, there is a third meaning to be given to the whole and its parts. We have seen that the behavior of a mechanical manifold can display integrative features which have no specific reference to particles. Such integrative features are phase, temperature, and pressure. Now it can also happen that the aggregate manifests a kind of superdifferentiation, a specificity which presumes the whole in a sense in which integrative properties presume the parts, yet without being determined by the whole. I refer to such things as organic function and reproduction. Two-body forces do not seem to account for them; whether many-body forces will yield them is at present unknown, and speculation on this point is idle because of the scarcity of analytic knowledge concerning the general solution of equations of type (9.3).

Our conclusions regarding many-particle mechanical systems can be summarized in these three statements: (1) As a spatial *system*, a mechanical aggregate is precisely the sum of its parts. (2) In its collective *behavior*, it displays traits which have no relevance for individuals; hence, behaviorally, the whole is more than the sum of its parts. (3) The occurrence of superdifferentiation, evident in organic systems, has as yet not been traced back to individual laws of motion. The mathematical difficulties inherent in this undertaking would at present seem to forbid it for practical reasons.

In Chap. 20 we shall once more return to the problem of organization and show how certain physical principles, when superposed upon quantum mechanical laws, do produce the rudiments of superdifferentiation.

9.5. Mechanics of Rigid Bodies

In this branch of mechanics, which involves relatively little of methodological interest and therefore need not detain us long, the system is a rigid body (to be defined below); significant properties are its mass, its three principal moments of inertia, six coordinates, and six velocities. Mass and moments of inertia are intrinsic properties which do not change in time, while the other twelve are time dependent and can be compounded to form a state.

The laws of motion are differential equations whose complete solution admits twelve constants of integration, and the relation of these constants to the variables of state is similar to its counterpart in the case of a *particle* already treated: the constants of integration can be chosen to be the values of the state variables at one given time. The complexity of the differential equations and the difficulties attending their solution do not concern us here. Suffice it to say that they are usually treated under numerous simplifying assumptions, such as complete absence of external forces and equal moments of inertia about different axes. The spinning, precessing, and nutating top, whose theory fills books, is one of the simpler examples of rigid-body mechanics.

The system of the present discipline, the rigid body, is not unrelated to the system of particle mechanics. Indeed it may be conceived as an assemblage of particles in which all relative interparticle distances remain fixed. The laws of motion of a rigid body can in fact be derived by imposing constancy of all interparticle distances as conditions upon Eqs. (9.3). Hence it is proper to regard this branch of mechanics as a division of particle mechanics, although convenience suggests for it a separate place. This is because some of the results of the theory of a rigid body, particularly when the body has symmetry and is restrained to rotate about a fixed axis, are so simple and so reminiscent of the mechanics of single mass points that it becomes advantageous to dismiss all thought of composition and to treat the rigid body as a system in its own right.

There is also, we should add, very good intuitive evidence for the number 12, the number of variables defining a state. A rigid body has six degrees of freedom. To see this we note first that three numbers are necessary to specify the position of a given, fixed point in the body relative to a coordinate system. In addition, two parameters are required to define the position of a line fixed in the body and passing through a fixed point, while the sixth quantity measures a rotation of the body about this line. The three last-mentioned variables are the so-called "Eulerian angles" or some other equivalent set (*e.g.*, Cayley-Klein parameters). These six variables, together with their time rates of change, are the quantities which compose a state.

A summary of the salient points developed here is given in Table 19.1 (page 413), where the connection between the schema of systems, observables, and states and the requirement of causality is examined.

Summary

This chapter illustrates the use of the concepts: system, observables, and state, in mechanics. A distinction is made between laws and equations of motion. Several equivalent ways of stating the laws (differential equations, integral equations, and conservation principles) are discussed, and a brief investigation is made of the senses in which a mechanism is, and is not, the sum of its parts.

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CHAPTER 10

Physics of Continua

10.1 Mechanics of Continua

The two alternatives with respect to the structure of matter, continuity and discreteness, have been recognized in the earliest stages of science and have at all times inspired controversies. They raise a tantalizing problem, for though matter appears to be continuous, it does show atomic structure on more careful examination; and though its structure be atomic as it is now conceived to be, who can say whether the stuff composing the elementary particles that make up an atom is not ultimately continuous? It is not likely that the age-old problem will ever receive an empirical solution. The "true" answer, if there be one, may hide itself forever in the uncertainties of immediate experience (Chaps. 4, 6). The question, then, is not whether matter is continuous but how theories succeed when they regard as a continuum the construct which they take to be their *system*.

Modern physics has largely lost interest in, and sensitivity to, the conceptual delicacies of the continuum problem, while mathematics, perpetuating the curiosity of ancient Greece, is appropriately aware of them and is finding ways for dealing with old paradoxes. It is perhaps even more important that the mathematician is seeking to make the discrete compatible with the continuum by studying and learning to handle continuous functions with discontinuities or singularities. Measure theory of point sets has already achieved a great deal in this direction. But on the whole the physicist, as we have said, shows at present little interest in these developments. It seems likely that set theory will be applied to the structure of matter in the near future, but the convenience and the success of traditional methods for describing "smooth" variations of entities in space and time reduce the urgency of such attempts. As a result, physics harbors a conceptual hybrid in the form of a discipline called the mechanics of continua, which treats matter as a continuum while admitting at the same time its atomic structure.[1]

The continuum[2] occupies part—or all—of three-dimensional Euclidean space. The continuum is the *system* of this branch of science. Its properties are quite different from those heretofore encountered, for a continuum is a nonlocalized entity which has its properties distributed throughout space. Thence follows the need to use *four* independent variables, x, y, z, t, instead of the former t alone, and upon these the properties must be made to depend. Whatever the properties are, they will be functions of these four independent variables.

Having thus specified the system, we turn to its significant observables. These are found to be first of all the mass density, an intrinsic property corresponding to the former mass and not depending on t (but of course depending on x, y, z); and second a set of quantities called strains. They are analogous to the coordinates of a mass point or a rigid body. The complete state of our system is known when all strains are given as functions of x, y, and z at one time. One may think of a strain as the momentary displacement of a point normally situated at x, y, z, within the medium.

In the general continuum there are altogether six components of strain, and these are advantageously compounded into a sym-

[1] When seriously questioned, the physicist concedes the *approximate* character of this discipline; he assumes it to describe phenomena in regions of space which are small compared with macroscopic dimensions but large enough to contain a great number of atoms or molecules. He thus states very plausible *rules of correspondence* which prevent him from applying the continuum theory where it fails.

[2] The vagueness of the term is intended. We are here dealing with a material continuum, of course, which is the nearest thing to the good old "substance." But continua need not be material, as the next section will show. What distinguishes a material continuum from others is that *mass density* is one of its properties. Clearly, any other property which can be related to observation by rules of correspondence may replace the mass density without destroying the significance of the continuum construct.

metric tensor. The equations are therefore tensor equations, but this is of minor interest here. More important is the circumstance that, in view of the use of four independent variables, the laws of motion have become partial differential equations and thus open up new and wider vistas of construction.

It would be well if the reader at this point could reflect upon the difference in the structure as well as in the basic implications of *ordinary* and *partial* differential equations. If he has the mathematical experience he will profit greatly at this stage by reviewing two simple examples: (1) the solution of the ordinary second-order equation representing a particle in simple harmonic motion; (2) the solution of the three-dimensional wave equation in some convenient system of coordinates. The philosophy of physics remains forever mysterious to the casual onlooker, who omits mathematical method entirely from his concerns. On the other hand one need not be an expert in mathematical analysis in order to form a correct appreciation of the general function of physical description; it seems that the circumspection acquired through attention to other abstract problems of philosophy is a fair substitute for mathematical technique. Acting upon this conviction, we shall attempt to outline here the main differences between ordinary and partial differential equations for the reader whose acquaintance with mathematics is slight.

The solution of an ordinary differential equation is made definite by the fixation of constants of integration, and we have already seen how the number of constants needed determines the choice of variables of states. The connection between type of equation and variables of state is equally interesting in continuum mechanics, but less simple. The solution of a partial differential equation needed here is made definite not by an assignment of constants but usually by nothing less than a complete description of the independent variables over the whole continuum, at a given time. Let S be a strain component, *i.e.*, a representative of the state of our continuum. The "law of motion" subjects S to a partial differential equation. We wish to know S as a function of x, y, z, and t. But the law will not allow it to be ascertained unless we insert in it as initial condition a known $S(x, y, z, t_1)$, that is, the distribution of S over the whole continuum at some fixed time t_1. We thus see why a *function* of x, y, z is needed to specify the state of a continuum instead of the ordinary *numbers* [1] encountered in the mechanics of particles.

After all, this is a fairly obvious result which we had already obtained by simpler means and which serves to emphasize once more the cognate structure of states and laws. But the same consideration also leads to a result which is less trivial and could not have been foreseen. In the mechanics of mass points the description of a state in terms of coordinates alone would have been incomplete; velocities, *i.e.*, rates of change of coordinates, are also needed. In the mechanics of continua a state is completely defined by the strain components alone; their rates of change are *not* needed. The reason for this lies in the peculiar character of the laws of motion. A partial differential equation of the type used in the theory of continua can be solved if the dependent variable alone (S in our case) is given; one does not have to know its time derivative. The use of partial differential equations thus makes description in one sense more complex, in another sense more simple.

We should draw attention to the schematic and indeed oversimplified character of the preceding account. Partial differential equations often raise unpleasant problems, some perhaps of deep philosophic importance. One curious feature is the way in which they couple time and space: If S is some special function of x, y, and z at the present instant, the equations may imply that the state has had this form throughout eternity and will have it forever.[2] Alternatively, an arbitrarily chosen $S(x, y, z)$, while possible at a moment, cannot persist for any finite time.

One other point of interest will here be raised. We have seen that the laws of motion predict $S(x, y, z, t)$ when $S(x, y, z, t_1)$ is given. But why single out the time variable for special duty when applying this scheme? Will the laws not predict $S(x, y, z, t)$ when $S(x, y, z_1, t)$, for example, is given? This is to ask whether the

[1] x, y, z, v_x, v_y, v_z, the variables of state in particle mechanics, form a set of numbers at any fixed time t_1.

[2] For example, if a wave is sinusoidal now, then it is sinusoidal at all times.

behavior of the continuum can be known everywhere and at all times when its states on a *surface* are given at all times. The answer is affirmative in most cases.

Our inquiry can be made more pointed. Will the laws predict $S(x, y, z, t)$ when $S(x, y, z_1, t_1)$ is given? That is to say, does knowledge of S on a *surface* at an *instant* suffice to predict what happens everywhere and at all times? The answer is generally no. But if both $S(x, y, z_1, t_1)$ and $\frac{d}{dt} S\,(x, y, z_1, t_1)$ are given, the answer may well be yes. Media for which the answer is affirmative are said to satisfy Huygens' principle.

The meaning of S has been left quite vague in this account. We have said that it represented a strain, that is, a displacement of points from their normal positions, and that its form was a tensor. In general it is an assemblage of six different functions. This would be true for a solid continuum with different properties in different directions. However, as in the case of the mass point, the number of variables can be reduced under simplifying conditions. For the particle this required limitations upon its motion; here it requires conditions upon the nature of the continuum. The simplest possible instance is a fluid (which sustains no shear and is isotropic) where S reduces to a single function. It is this S which appears as independent variable in the equation for sound waves passing through a gas.

Had we included convective motion of our medium, the whole description of the system would have been vastly more complex. But the method would remain the same despite the addition of new physical principles, and we omit its discussion.

10.2. Electromagnetic-field Theory

The method of science reaches its fullest development in the theory of the electromagnetic field. Here it reveals itself as a thing of abstract beauty and of impressive power, affording an insight into activities both of brilliant rational construction and of refined observation. The logical component of scientific method is in evidence in the field equations with all their mathematical elegance; the empirical component comes to the fore in the amazing experimental techniques which this subject has created—if physical reality is to be meaningful, it must draw its substance in adequate measure from the science of electromagnetism and not look exclusively upon the hard and rigid particles of mechanics, as it has so often done in the past. In the present section we analyze field theory in terms of our pattern: system, observables, and states.

The subject should be introduced historically. For it presents one of the rare instances in which history, usually groping and stumbling toward organized knowledge, steadfastly approaches and finally seizes the goal in an ingenious sweep. In the person of Faraday it shows an example of inspired experimentation, a genius with a divining rod for scientific treasure, and yet a man unwilling to speculate beyond the confines of his visual intuition. To him, as to all his precursors and many of his successors, the *system* of electrodynamics was a material medium, later called the ether, which could be strained in the manner of the *material* continuum of the last paragraph. To him, electrodynamics was an extension of the mechanics of continua. Even Maxwell, the great synthesizer, with a confidence in the formal truth of his equations so great as to be willing to draw from them what must have seemed grotesque predictions,[1] was unwilling to let material substance go. He, too, thought of his subject as a branch of mechanics. Nowadays one wonders at his restraint. His system of equations was so complete, so beautifully coherent and integral as a logical entity that it might well be conceived as defining something autonomous, something nonmechanical. But Maxwell saw in it only the footprints of another kind of matter. The contrast between Maxwell's attitude and today's is great, for our modern youth[2] will eagerly start with any set of equations possessing formal credentials half as good as Maxwell's and assume, with enough hope for success to warrant great labor, that they define a new kind of reality in nature.

This reversal of attitudes came about, of course, through the failures of all attempts to detect the material ether. We cannot

[1] For example, the existence of radio waves.

[2] Cf. almost any recent issue of the *Physical Review*, and read the papers on quantum electrodynamics.

take the space to discuss these interesting episodes and hence refer the reader to other books.[1] Suffice it to say here that the ether did not die an accidental death, that during its incurable illness it had the services of the most notable surgeons, who strained to keep it alive, and that when it did die it had its departure attested by trustworthy authority. Thus we may be sure that it is really dead as a material entity. Some still say it may come alive again, but its prospects at the moment are poor.

What, then, is the system which carries electromagnetic phenomena? It cannot be described in simple demonstrative terms because it does not correspond directly to any immediate experience. It is like the material continuum in some formal aspects, but with its inertial and its gravitational qualities removed. It has in fact many of the paradoxical traits with which the popular mind sometimes invests a ghost, except that there is nothing vague about it. Though itself nonmaterial and able to subsist according to its own laws in this disembodied state, it can nevertheless interact with matter. Its name is the electromagnetic *field*.

The only concise way to define it is to say that it is the bearer of certain very determinate properties; through them does it attain scientific status and, if you please, existence. This barrenness with respect to visual suggestions is not a defect of the field; it is a symptom of logical purity and certifies the absence of adulteration. When we think of a mass point, a host of extraneous associations with weights, tactile experiences, and the like, swarm in upon us, often to confuse its meaning. Actually a mass point, when correctly understood, is that which has the property "mass" as it appears in Newton's laws (or some other) and can be localized by means of three coordinates. Everything else is unnecessary, though at times psychologically useful, baggage. In the same way, then, the field *is defined* by its properties.

These properties are two vector quantities called electric and magnetic field strengths. Their usual symbols are **E** and **H**. Being

[1] See, for instance, J. Larmor, "Aether and Matter," Cambridge, 1900; E. T. Whittaker, "A History of the Theories of Aether and Electricity," Longmans, Green & Co., Inc., New York, 1910; Blackwood, Hutchisson, Osgood, Ruark, St. Peter, Scott, and Worthing, "Atomic Physics," John Wiley & Sons, Inc., New York, 1933.

vectors, they are each equivalent to three scalars; hence we are dealing with six independent properties. To be sure, they are not entirely unrestricted because they must satisfy the transformation rules for vectors, a fact which imposes on them certain constraints which we are not considering here. Together, these six quantities form a state of the field.

We have called them "properties" and "quantities" without much discrimination. In the general sense they are observables. Earlier (Chap. 8) we introduced a distinction between possessed and latent observables, the former being assigned when the specific content of similar observations does not change—*e.g.*, the rose is not red, blue, white, etc., in different observations upon the same physical state. This consistency was present in the properties of all the systems studied thus far (mass points, rigid bodies, mechanical continua), and it is assumed to be present with respect to the observables **E** and **H** as well. For that reason we are permitted to call them quantities possessed by the field.

In modern quantum electrodynamics they have been treated more generally as latent observables whose measured values "spread" upon observation. While this has led to some success,[1] the theory is not yet ripe for philosophic analysis. If Heisenberg's conjecture is correct, this approach may have to be abandoned. We therefore turn back to "classical" electrodynamics, where **E** and **H** are quantities of the usual type.

As is to be expected, **E** and **H** are functions of the time. But since the field is assumed to pervade space, they share with the strains of the mechanical continuum the character of being also functions of x, y, and z. From here on the story is rather similar to the one told in the preceding paragraph. The "laws of motion" —of course there is no motion in the material sense—are now Maxwell's equations, four partial differential equations so constituted as to propagate a state. That is to say, if $\mathbf{E}(x, y, z, t_1)$ and $\mathbf{H}(x, y, z, t_1)$ are given for some fixed time t_1, the equations permit $\mathbf{E}(x, y, z, t)$ and $\mathbf{H}(x, y, z, t)$ to be calculated for all times t.

The equations do not form a complete scientific theory, however. So far as they are concerned, the field would be a world of

[1] Cf. W. Heitler, "The Quantum Theory of Radiation," Oxford University Press, New York, 1944.

isolated ghosts which could not make their presence known. Maxwell's equations need to be supplemented by a mathematical statement saying what field strengths do to matter, and such a statement has been supplied by Lorentz, who succeeded partially in introducing matter, under the form of electrons, into the field. The science of electrodynamics has thus also become a hybrid, embracing continua as well as particles, and to many thinkers this necessity has marred its beauty. But hope is alive that some day, when more puzzles are solved, the particle will present itself as a singularity of the field vectors and will thus merge with the concept of the field. If this ever happens, particle mechanics will become amalgamated with electrodynamics. As we know from Chap. 5, the metaphysical principles drive science in that direction; but further comments on this prospective union must wait until it is achieved. It is well therefore that we return to the field equations in their simple and unmixed form.

Maxwell's equations have several formal properties worthy of note. In the first place, they can be combined to yield what is known as the wave equation, and this can be interpreted by saying that field strengths spread through space with a finite speed, namely, the speed of light, c. In other words, any change in $\mathbf{E}$ or $\mathbf{H}$ taking place at the point A cannot be detected at the point B unless the time elapsed is greater than the distance between A and B, divided by c. This has been a cause for relief to philosophers who were troubled by the thought of action at a distance, for they were now able to say in a manner sanctioned by the mathematics that an influence travels from A to B. Thus the field has come to be looked upon as a mediator between cause and effect, and the good old notion of "contact action" (as distinct from action at a distance) has been formally revived. What good this does is not altogether clear in view of the nonmaterial nature of the field, but it seems to give the mechanically minded a goodly measure of satisfaction. There is nothing wrong, logically or scientifically, with the concept of action at a distance; the only respectable pleasure which may be derived from the role of the field as a temporal messenger comes from the greater degree of uniformity which it bestows on different scientific theories.

Another consequence of Maxwell's laws is the existence of "constants of the motion," to use a term already introduced in

Sec. 9.2. The equations can be integrated and thus lead to the postulation of energy and momentum in a more or less familiar way. We note again that these conservation principles are an analytic consequence of the laws of motion, not something induced from direct observation. An electromagnetic field has energy and momentum because of Maxwell's equations, not aside from them, and to observe the constancy of electromagnetic energy and momentum is one way of checking the equations.

A particularly beautiful branch of electromagnetism is electrostatics, the subject in which the time variable is arbitrarily eliminated from all equations. Physically, this amounts to asking what forms of **E** and **H** are consistent with Maxwell's equations when the field strengths are constant in time. Then, instead of predicting states at future times, the laws specify certain possible spatial distributions of **E** and **H**, rejecting all others as impossible. This circumstance follows from the "partial" character of the differential equations and is therefore unknown in particle mechanics, where *any* static configuration of particles is a possible one under suitable forces.

Up to the present, our account has favored the constructional elements of electromagnetism, to the exclusion of epistemic correlations. This must be rectified. We have already indicated that the field itself is not linked to immediate experience by direct rules of correlations. In the symbols of Fig. 5.1 it is represented by a construct, not of the isolated and therefore objectionable kind, but by one which allows passage by logical rules (from a system to its properties) to quantities which do have arms stretching toward Nature. These quantities are **E** and **H**. The rules of correspondence are again operational definitions, namely, the procedures by which we define **E** and **H** in elementary physics courses. In its simplest version, **E** is the measurable force on a unit charge, **H** the measurable force on a unit pole. In view of the nonavailability of unit poles, and also because of the practical turn of mind of the electrical engineer, who has impressed his prepossessions in increasing and indeed wholesome measure upon the recent developments in this subject, we now tend to define **H** by reference to currents. This is all to the good of science, for rules of correlation should provide the most satisfactory linkage with actual experience. But let us note that these rules afford one kind of definition for **E** and **H** and that another kind is already inherent in Maxwell's equations. Aside from showing, perhaps to the dismay of the physics teacher, that scientific quantities are susceptible of more than one good definition, this observation serves to consolidate the meaning of electric and magnetic field strengths; it will concern us again in a more fundamental way when we treat of the general role of definitions in science (Chap. 12).

The quantities **E** and **H** share with the positions and velocities of mechanics the status of being the basic variables, that is, the variables linked to Nature by the simplest rules of correspondence. There are other variables, related to them in definite ways, which allow equally satisfactory descriptions. In mechanics momenta, angle and action variables come to mind. Similarly, vector and scalar potentials are often used with profit in electrodynamics. Maxwell's equations take on a particularly appealing form when they are inserted. In order to be unambiguous, however, the resulting equations have to be augmented by certain extraneous conditions (gauge invariance).

In conclusion, a brief comment on the use of fields in other parts of science seems indicated. By reason of its success in electrodynamics the field idea has invaded mechanics, where the term *force field* is often used. But its application is limited to static problems, wherein the rich potentialities of a continuum with time-dependent properties are not allowed to unfold themselves. Gravitational fields, for example, are largely treated as static fields, and little is known about such things as the speed of propagation of the gravitational influence. In hydrodynamics, where the system is a material continuum much like the one treated in the preceding section but with entirely different properties (mass density and flow vectors), the field concept rises to greater power and refinement.

We cannot review here the various biological fields[1] which

[1] For example, Driesch's entelechy (H. Driesch, "Analytische Theorie der Organizationsentwicklung," W. Engelmann, Leipzig, 1894); Spemann's organizator (H. Spemann, "Embryonic Development of Induction," Yale University Press, New Haven, Conn., 1928), embryonic fields (P. Weiss, Morphodynamische Zelltheorie und Genetik, *Verh. 5, Internat. Kongr. Vererbungswiss*, 2, 1928).

borrow suitable features from several physical types in an endeavor to account for organization and growth. One serious difficulty with most of them is their failure to grasp clearly the methodological requirements involved. Our analysis will have shown that, to be successful, they must (*a*) unambiguously define a sufficient and necessary set of state variables and (*b*) state the laws which regulate their behavior. These laws may be those already recognized in some physical discipline, or they may be sui generis; but they must always be clearly formulated.

Of special interest is the attempt of H. S. Burr and F. S. C. Northrop [1] to correlate growth patterns in living organisms with electrostatic fields in their surroundings. It is the thesis of these investigators that the influences which impose special "configurations" upon the molecular constituents of living tissue are physical in their nature and require the same treatment as the fields of ordinary electrodynamics.

Summary

Continuum mechanics has two main subdivisions: the mechanics of material media and electromagnetic-field theory. The present chapter discusses what is meant, in both these branches, by the system, its observables, and its states. The character of the laws of motion, which are here of the form of partial differential equations, and the nature of their solutions are examined in some detail. Finally, we indulge in a brief reflection upon the use of the field concept in nonphysical sciences.

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CHAPTER 11

Thermodynamics

11.1. THE METHOD OF THERMODYNAMICS

THERMODYNAMICS IS the most empirical of the physical sciences. We do not mean that it is a mere mass of coordinated facts held together by their observational validity. Indeed the pattern of systems, observables, and states is impressed upon thermodynamics with extraordinary firmness, and it is by most meticulous attention to these methodical elements that this science proceeds. Its empirical character comes from the fact that it deals with a large class of systems whose laws are not identical in detail or derivable from some common source, but need to be fashioned after observations. Or, to put it in another way: the laws of thermodynamics[1] do not enjoy the logical generality of the laws of electrodynamics. Outwardly this is evident in their form; instead of being differential equations, they are primitive relations between the variables of state, a fact which becomes significant when we recall that differential equations are generators of *classes* of primitive equations. The laws of thermodynamics, being particular primitive equations, are therefore of a lower order of generality than the laws of motion. These preliminary remarks will now be made more explicit and, we hope, more meaningful.

The systems of thermodynamics are ordinary bodies with clearly defined spatial boundaries, and our interest centers in their reaction to the various modes of heating them. Typical systems are gases, liquids, mixtures of gases and liquids enclosed in containers with fixed or variable volumes, and solid bodies.

We shall confine our attention for the most part to the simple

[1] We do not refer here to the so-called "first" and "second laws" of thermodynamics, which are not laws but principles in our sense. See below.

instance of a single substance existing in a single phase, like a gas in a vessel; this will allow us to see the essential details of method without the encumbering features of more complex cases. Chemists are accustomed to classifying their systems in accordance with the number of *components* (different kinds of substance) and *phases* (homogeneous parts of the total mass, separated from the rest by recognizable boundaries) which are present. Thus an aqueous solution of sugar in water in the absence of its vapor is a system of two components and one phase: a mixture of ice, water, and steam is a one-component system of three phases. Each class of system has its own set of variables of state. What we are about to discuss is, in technical terms, a one-component system with a single phase.

If we wished, we could define such a system as one capable of description by two properties, or observables, and these are again observables of the possessive—as distinct from latent—kind. But it happens that experiment easily yields information about a greater number of interesting variables than is needed for a concise description of the system. Hence thermodynamics operates with a profusion of variables of state, and its business is the determination of a given one through others. Our simple system is completely described by its pressure (P) and its volume (V); in other words, there exist laws which allow all facts concerning it to be ascertained when these two quantities are given. P and V therefore define a *state*. However, P and V, while sufficient, are not the only carriers of maximum thermodynamic information; certain other quantities like temperature (T) and entropy (S) would do as well. It is possible, therefore, by observing more than two variables to overdetermine the state of a system. Of this circumstance thermodynamics makes a virtue; it uses the overdetermination to construct its laws.

The law is called an equation of state. For our simple case we write it in the customary manner where it relates P and V to T: $f(P, V, T) = 0$. The function f, with arguments P, V, and T, is different for different gases; its form for the ideal gas, the reader may remember, is $(PV/RT) - 1$, R being the so-called "gas constant." By suitable transformations, a relation of this nature can be written for any three variables of state, for example,

$\varphi(V, T, S) = 0$. The variation of f or φ as we pass from one system to another was alluded to in the introduction, when the laws were said to differ in detail for different systems. It will now also be apparent in what sense they are primitive relations between the variables, and not differential equations.

From the point of view of conceptual elegance this specificity represents a defect. The mathematician would be far more pleased if the laws could be written in differential form from which, by integration and adjustment of constants, special laws for special systems could be derived. Unfortunately, nature has not yielded to his desires, and it is unlikely that she will. Primarily for this reason, many scientists look upon thermodynamics as a discipline of lesser purity or, indeed, as a science which starts with the wrong variables and therefore becomes bogged down with burdensome impediments. They accord prior rank to statistical mechanics, which does not suffer from these defects and is able, albeit with the use of certain nonmechanical assumptions, to produce analogues of thermodynamic laws. Even the exact form of the equation of state can be derived for ideal as well as for some nonideal gases. Despite all this, we choose here to treat thermodynamics as a self-sufficient branch of science and analyze its method without prejudice to its ultimate place within the hierarchy of the sciences.

Had we examined a more complicated class of systems, we should have found it necessary to introduce a greater number of independent variables and hence a different representation of states. One of the great contributions of J. W. Gibbs to the subject under study was the discovery of laws relating to systems of many components. He showed the need for including among the variables of state the masses of the different components, in addition to P and V. The equation of state then becomes a relation between P, V, the masses, *and* some other variable. If electric, magnetic, and gravitational forces are present, further properties must be introduced. In a crystal, the pressure loses its function as a variable of state and must be replaced by strain variables, and so forth. The basic method of obtaining laws through an empirical overdetermination of states, however, remains the same.

Knowing readers will probably ask: Is this method of obtaining laws truly peculiar to thermodynamics? Are not the laws of mechanics equally due to an overdetermination of states? These questions should be examined with care.

Against the view which ignores a distinction is the form of the laws. In mechanics they are differential equations, in thermodynamics they are not. And one does not derive differential equations directly from observation, any more than universal propositions are *derived* from particulars. In favor of the dissident view one may argue as follows: A sufficient set of state variables in particle mechanics is x, y, z, v_x, v_y, v_z; another sufficient set is x, y, z, p_x, p_y, p_z, where the p's are the components of linear momentum. If observation determines both sets, it seems that "empirical" relations between velocity and momentum of the same sort as the equation of state in thermodynamics can be established.

This apparent resemblance fades away, however, when we look a little further. For in the first place the relation between velocity and momentum, $p_x = mv_x$, is *not* the equation of motion and is not fertile in predicting mechanical states. It is a sort of auxiliary relation which allows the true equations of motion to be written in simpler form. Still more significant is the fact that the momentum is not an independent observable but is compounded from m and v. True, p can be measured directly—but only *with the use of Newton's law*, which cannot be established by measurements of p and v.

The situation is different in thermodynamics. Here P, V, and T are independently measurable by unrelated experimental procedures. Each quantity refers to Nature through its own rule or rules of correspondence, and the relation between these quantities is established without intrusion of other considerations: this is the main reason for calling thermodynamics an empirical science. We therefore regard the distinction we made between mechanics and thermodynamics to be genuine.

Curiously the equation of state, though it is our basic law, does not contain the time coordinate. This raises the important question as to the manner in which it predicts states. Here we face another radical difference from the other branches of physical science; *thermodynamics is not concerned with temporal prediction.*

Accordingly its purpose is seen to be the "prediction" of unknown variables on the basis of a set of known ones, all having reference to the same state. This state is presumed to persist indefinitely in time.

Such a procedure is possible only for those special conditions of the system which in fact do not change in time, or which change infinitely slowly. Thermodynamics is therefore applicable only to states called equilibrium states, defined by the foregoing requirement, or somewhat more generally to "quasi-static changes." [1] It is well known that its laws say nothing about the speed with which actual thermal processes take place, though they do indicate their direction and their final goal. In a way, then, our subject allows a weak measure of temporal prediction. Returning to our gas, suppose, for instance, that P, V, and hence T are given. It is then possible to find from the equation of state the value any one of these variables will take on a very long time after the other two are changed.

In mechanics, time is the independent variable. The equations of thermodynamics do not discriminate between the quantities which describe a state to an extent that allows one of them to be regarded as independent; hence the equations of thermodynamics do not contain a proper independent variable. Any one may be chosen to be independent at will. Such freedom imposes upon the equations of thermodynamics, when they are written as differentials, the peculiarly cumbersome form which is known to every student of the subject. For not only does it force one to use partial derivatives—this is true in many sciences—but the need for stating which of several variables is regarded as independent adds undecorative subscripts to the derivatives.

11.2. The Principles of Thermodynamics

The laws of thermodynamics, which we have taken to be the equations of state of the various systems, were shown to be less general than their counterparts in the other sciences. Nevertheless the subject has a universal goal and a degree of elegance all its

[1] Technical term for very slow changes, sometimes inaccurately called reversible changes.

own. Paradoxical as this may seem at first, it is brought about by principles operating above the plane of laws, principles unable in themselves to generate the laws but able to give them scope and substance. They are often called the Laws of Thermodynamics for historical reasons. Since they function as super laws in a manner we are about to explain, we shall refer to them as (capitalized) Laws. They have no analogue in mechanics or in electrodynamics.

Law I. The first Law is often expressed by saying: Heat is a form of energy, and energy is conserved. So blunt a statement of the Law, though widely accepted, is unfortunate; being halfway correct and saying something fairly factual, it chokes all inquiry into the finer meaning of thermodynamic principles. For as it stands, it is a sheer, uninteresting tautology unless an independent formula is given by which the energy contained in a heated body can be computed. In mechanics, the law of conservation of energy is significant because we have definite formulas for calculating the various kinds of energy (*e.g.*, kinetic and potential energy) which can appear, and we may check their balance by empirical procedures. In thermodynamics, however, the theorist has no such standards; he invents queer forms of energy like latent heats, heats of formation, of absorption, and so forth, when his equations fail to balance. What, then, saves the first Law from being trivial?

An answer is not available unless attention is given to the sort of analysis conducted in this chapter; the answer involves an understanding of what is meant by a variable of state and of the rules of correspondence which link these variables with Nature. Nor can it be given without a modicum of displeasure to those who abhor mathematical symbolism. Our desire is, however, to minimize their discomfort and yet to provide the answer.

Every application of the first Law involves three quantities relating to the system under consideration. This system is assumed to undergo a change—for instance, an expansion or a change in temperature or pressure. Such a change can be detected by observers who watch the boundaries of the system; its interior is regarded as being closed to view. Two of the quantities in question are thus observed, namely, the heat absorbed by the system, ΔQ, and the work done by it, ΔW. Through operational definitions involving reference to certain instruments (thermometers, manometers, means for measuring volumes), both ΔQ and ΔW are uniquely determinable. This is not true for the third quantity to be introduced, the change in internal energy, ΔU. In fact, ΔU is *defined* by the first Law:

$$\Delta U = \Delta Q - \Delta W \tag{11.1}$$

So far, then, the statement amounts to nothing more than a direction for determining an unknown quantity, ΔU, which is itself unrelated to observable properties of the system.

Now it happens that Q and W are not proper variables of state. If we tried to base a description of thermodynamic behavior upon them, our laws would be ineffective, as ineffective indeed as if we tried to study mass points by reference to colors and odors. But the Law says, in addition to Eq. (11.1), that, despite this defection of Q and W, *U is a variable of state.* By this further stipulation Eq. (11.1) becomes more than an empty definition; it is made empirically testable.

To see how this comes about we write Eq. (11.1) in its differential form appropriate for infinitesimal changes and indulge in a bit of analysis.

$$dU = dQ - dW \tag{11.2}$$

Let the variables of state sufficient for the description of the system (two in the case of the one-component system of single phase considered in the last section, but in general more) be denoted by $x_1, x_2, \ldots, x_n$. It is then always possible to write

$$dQ = \sum_i q_i \, dx_i$$

$$dW = \sum_i w_i \, dx_i$$

and the q_i, w_i are measured functions of the x_i since dQ and dW are measurable. We then have

$$dU = \sum_i (q_i - w_i) \, dx_i \tag{11.3}$$

If U is a variable of state, dU is an exact differential. At this point we use a theorem due to Cauchy, which states: If

$$dX = \sum_i X_i \, dx_i$$

is an exact differential, the coefficients X_i satisfy the relations

$$\frac{\partial X_i}{\partial x_j} = \frac{\partial X_j}{\partial x_i} \qquad i, j = 1, 2, \ldots, n$$

Applying the theorem to Eq. (11.3), we have

$$\frac{\partial(q_i - w_i)}{\partial x_j} = \frac{\partial(q_j - w_j)}{\partial x_i} \qquad i, j = 1, 2, \ldots, n \tag{11.4}$$

Since these relations represent identities when $i = j$, their number is equal to the number of pairs which can be formed from n things, namely, $n(n - 1)/2$. The number of measured quantities, the q_i and w_i, is $2n$. We have thus derived from the innocent postulation that U is a proper variable of state a host of equations which can be tested experimentally. The initial statement, which makes Law I appear as the principle of conservation of energy, misses its point completely and underestimates its fruitfulness.

Conservation theorems, as we have shown, are derivable from the laws of mechanics and from the laws of electrodynamics; they are therefore equivalent to the laws. In the present case, the first Law is not derivable from the equations of state which function as laws in thermodynamics but generates relations of the type (11.4) which enter into useful collaboration with the laws themselves.

Much of the intrinsic appeal of thermodynamics arises from the adroit use this science makes of the niceties of method, from the way it turns to profit even such indeterminate bits of knowledge as whether or not a given property fully describes a state. This methodological orientation seems indeed to be peculiar to thermodynamics.[1]

Law II. The first Law provides the *quantitative* framework for thermodynamic changes; it says essentially by how much, numerically, a given quantity will have its value altered in a process.

[1] The *occurrence* of properties that are not variables of state is common in all fields. In mechanics, our model science, we do not employ colors and smells, these being too irrelevant to the problem; but we do use kinetic energies, which are not proper variables of state inasmuch as different states can have the same kinetic energy. Here, however, we note another important difference: in mechanics, the same state always bears the same kinetic energy; in thermodynamics the same state may correspond to a variety of Q's and W's.

But it is insensitive to the *direction* in which the process occurs. One can see this at once from Eq. (11.1); for if each symbol appearing there were to be given a negative sign, the equation would obviously still be true. Of course, this is the character of all equations, not only those of thermodynamics. Why, then, should it concern us here and not in mechanics or in electrodynamics?

Strangely, the insensitivity to direction is borne out by experience in the latter two disciplines, but not in thermodynamics. In mechanics, for example, the energy principle would require that $\Delta E(\text{kinetic}) = -\Delta E(\text{potential})$. Apply this equation to a swinging pendulum, and you will see that, when the pendulum swings toward the center, the left-hand side is positive and $\Delta E(\text{potential})$ correspondingly negative. When the pendulum moves away from its center, however, the opposite choice of signs is correct. Hence *both* possibilities occur. Now consider a thermodynamic example, the flow of heat from a hot to a cold body, which is one of the simplest applications of Eq. (11.1). Here the system comprises both bodies; ΔQ is the sum of the heat absorbed by the hot body and that absorbed by the cold body, $\Delta Q = \Delta Q_1 + \Delta Q_2$. Also, ΔU and ΔW are both zero. The equation therefore says that $\Delta Q_1 = -\Delta Q_2$. Since heat flows from body 1 (hot) to body 2, ΔQ_1 is negative and ΔQ_2 positive. But heat never flows from cold to hot bodies in an isolated system; hence the other choice of signs does not occur.

This one-sidedness, or irreversibility, is true of all naturally occurring thermodynamic processes, but is not implied by the first Law. The second Law postulates it in a succinct and unambiguous way. Qualitatively, Law II is a refinement and a generalization of the statement: Heat does not flow from low to higher temperatures. (Notice that water does flow uphill if it has a sufficient velocity at the bottom!) There are various ways of stating the Law. Since we are not going to make specific use of it, we present the simplest formulation in terms of *entropy*, omitting details. Entropy is a quantity which is both a proper variable of state and measurable.[1] To many students of science

[1] Its well-known epistemic definition is: Change in entropy equals heat received divided by the temperature at which the heat is transferred.

entropy is something very abstract, something closely related to probabilities. This attitude represents an improper approach to thermodynamics, where entropy enjoys the same status as pressure and temperature. The concept would have perfectly good meaning even if probabilities had never been invented—and we beseech the philosophic reader to dismiss at this point all associations with bags of mixed-up marbles and decks of shuffled cards if he desires a correct view of things. Entropy is as definite and clear a thing as other thermodynamic quantities. With this in mind we proceed to state the second Law as follows: The entropy of a closed system never decreases.

To the thoughtful observer it is at once a source of amazement and of satisfaction that so simple a statement encompasses the whole range of irreversible phenomena. It accounts alike for the facts that heat does not flow uphill, that an ocean liner cannot draw its power from the heat of the ocean, that a refrigerator cannot function without a motor, and that the fast molecules of a gas cannot be separated from the slow ones. When the second Law is combined with the first and use is made of the "exactness" of dU, which was illustrated in the preceding section, a very imposing array of useful information can be obtained whose exposition forms the major part of textbooks on thermodynamics.

The customary view sees a peculiar aura of duplicity about the truth of the second Law. Somehow Law II is supposed to be not literally true but *only highly probable;* it is to occupy a precarious status of its own. To many minds, however, it is the prototype of all laws because it declares its content with honesty as being probable, as any self-respecting physical proposition should do; the categorical form of the mechanical laws, on the other hand, is held by these skeptics to be a sin against the code of science or at best to be a simplified version of what a law should be. Why is all this caution, all this epistemologic circumspection, released by the second Law of thermodynamics when the equations of mechanics gave us no similar scruples?

It is because of an inconsistency in our basic attitude. We find it difficult to keep our eyes fixed upon thermodynamics and are tempted to cast a roving eye beyond its boundaries. As already mentioned, there is another branch of science, known as statistical mechanics (cf. Chap. 14), designed to encroach upon thermodynamics in hopes that it will ultimately explain it, and a good deal of confusion arises from mistaking one for the other. We are postponing our review of statistical mechanics until an exposition of the theory of probabilities has prepared the ground for it. To resolve the issue confronting us now and to curb harmful indulgence, we state here a few relevant facts.

By using the laws of mechanics plus certain hypotheses which are of the nature of rules of correspondence between probabilities and observations (and hence conceptually different from all mechanical principles), statistical mechanics is able to define a quantity whose mathematical behavior is *statistically* the same as that of the entropy in thermodynamics. Let us call the entropy S, its statistical analogue S'. The second Law says, quite categorically, $\Delta S \geq 0$; statistical mechanics says $\Delta S' \geq 0$ with *overwhelming probability*.

Now, are we going to identify S and S'? Two perfectly legitimate stands may here be taken. One is to assert the autonomy of thermodynamics. Clearly, we do not ask that Maxwell's equation be derived from particle mechanics; the success of electrodynamics and the artificiality of all attempts to derive it from mechanics dispose us to look upon Maxwell's equations as true beyond the need of ulterior justification. If we are willing to look upon thermodynamics with the same benignity, the second Law is as true as the laws of mechanics.

But there is, after all, the tantalizing possibility of *almost* accounting for the second Law by mechanical reasoning. This makes us willing to extrapolate our partial success and to say that ultimately S and S' will be found to be identical. The validity of the second Law then becomes a matter of high probability only; the pail of water on the fire has an extremely small but a finite chance of freezing; even more amazingly, the hot iron, thrown into a vessel of water, after cooling down and heating the water, could once in a very long time (a time far greater than the two or three billion years which our universe has existed) get heated again, leaving the water cooler, and jump out in fiery incandescence. Practically, of course, the stand taken on this issue is of no importance.

We have shown that there is no *logical* ambiguity in the situation: the premises for the two possible points of view are perfectly clear and distinct. Above all, it is impossible to draw inferences from the probability aspect of the second Law regarding the probability aspects of other laws of nature. The fallacy of such an undertaking becomes evident when it is realized that the second Law talks about probabilities *if the mechanical laws are exact.* Whoever wishes to make out a case for the inductive and hence the noncategorical character of all propositions about experience must not take the second Law of thermodynamics as a starting point. My own preference was stated in Chap. 6. I regard it as simpler, and far more in harmony with the procedures of the exact sciences, to locate all uncertainties in the realm of immediate experience, leaving the laws exact. There seems to be so little need for messing up both constructs and Nature.

11.3. Looking Back and Forward

In the last three chapters we have reviewed the basic structures of the several classical branches of physics from the vantage point gained by our system-observable-state distinction, which we regard as a methodologically useful one. No artificial attempt was made to thrust a uniform mold upon all these branches aside from this important internal classification; indeed within its framework the differences between the various parts of physics stand out quite as perceptibly as their similarities. We do not wish to suggest that the methods of the different sciences are the same.

Yet I believe that there is something more natural in the proposed distinction and the classification which it permits than in the classification which is based upon the *contents* of the sciences. One would like to claim for it the kind of superiority which the natural system of botany has over the counting of stamens practiced by Linnaeus.

The selection of subjects in the foregoing chapters was made in the desire to cover the range of physical sciences as completely as possible without introducing ideas which need to be clarified before use. The ideas we did not want to inject are those connected with the probability calculus. This has forced us to leave out statistical mechanics, an increasingly important branch of modern chemistry, and quantum mechanics, which is likewise engulfing both physics and chemistry. Our further program is to prepare the stage for these. But even before embarking upon probabilities, a highly controversial subject, it is necessary that we discuss a problem whose neglect seems to have caused most of these controversies. This is the general role played by *definitions* in an exact science; it will be treated in the next chapter.

A book, like a public building, should have emergency exits. We provide one here for the reader who has become impatient about the meaning of reality. He may, if he wishes, skip the next few chapters, alight for a cursory look at the chapters on quantum mechanics, then read Chap. 15 and the last chapter of the book.

Summary

Thermodynamics is a peculiar science in many respects. Its system has a great variety of forms; its observables may vary from system to system; its laws (equations of state) are primitive equations between the variables of state, not differential equations as in other parts of physics. Thermodynamics is the most "empirical" of all the exact sciences and produces its laws by an over-determination of states.

The so-called Laws of Thermodynamics are principles operating above the plane of ordinary laws. They are explained, and considerable attention is given to the common attitude which regards the second Law of Thermodynamics as true in an overwhelming majority of observed instances but not true in the sense of the laws of mechanics.

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CHAPTER 12

The Role of Definitions in Science

12.1. On the Uniqueness of Definitions

THE SCIENTIST prides himself on the clarity and precision of the concepts he employs. Many minds see in accurate thought and speech the prime characteristic of every science, and it is the professional creed of the physicist and his interpreters that terms must not be used unless they are clearly defined. Like most creeds, however, this utterance has become hollow by reason of its being at times thoughtlessly proclaimed.

What has happened with respect to definitions in recent years may be summarized as follows: To ensure clarity and precision, scientists have done what appears obvious at first sight; they have insisted that every concept be defined in a single way. Then, realizing that there are in fact numerous good definitions of some physical concepts (for after all it does not matter in a fundamental sense whether we define time by reference to the sun or to a pendulum clock), they were at first perplexed; but soon, with single-minded determination, influential philosophers and scientists decreed the elimination of obnoxious definitions, leaving only *Zuordnungssätze* (Carnap), operational definitions (Bridgman), and the like. Controversies arose as to which of several definitions of a given physical quantity (force is a favored victim) was indeed the proper one, and physics teachers clustered into groups, each with an allegiance to its own set of recipes. As they looked at time-honored phrases, they found them meaningless, and they made the shocking discovery that great men had indulged in word play and circumlocution when they defined their terms. Even Newton committed the atrocious crime of circularity when he said: Mass is density times volume.—For what is density if not mass per unit volume?

It is our desire to show that there can be no exact science at all under the rigid rule of singleness of definition. When consistently applied, this rule can generate a well-articulated body of knowledge like civil law, with its quaint but definite [1] conceptions, or descriptive botany, but not the organic unit which we call a science. Living science, we hope to demonstrate, owes its vitality to the fruitful interplay of two different modes of definition, one closely related to theory and law, the other to the rules of correspondence. Before dealing with these positive aspects in their fullness, we review some partial ones and give the indefinite criticism inherent in the foregoing remarks a more specific aim.

What was called the rule of singleness presents itself in two related attitudes, here to be labeled A and B. View A, primarily held among scientists, accords to every physical quantity *one* and only one proper definition. View B recognizes the possibility of several definitions but insists that they be of *one* certain *type*. In the present section we deal mainly with the first of these hypotheses.

Certain physical quantities, like length, mass, electric and magnetic field strengths, lend themselves readily to a substantiation of view A and are therefore almost exclusively chosen by its proponents to exemplify it. Length is defined uniquely in terms of congruences between parts of rigid bodies, (relative) mass is defined as a ratio of accelerations, field strength as the force on some unit entity like mass, charge, or unit poles. The reader will recall the details to which these indications refer.[2]

The difficulties in this simple procedure cannot be long concealed. Lengths are not always measured by rigid rods but often by means of sonic, radar, and light waves. Masses are usually determined by weighing, followed by an unstated theoretical

[1] According to old Austrian law a housefly is a domestic animal!

[2] The belief that every physical concept has its one proper definition finds great favor in the classroom, for to inculcate it in a student is to direct attention to precise particulars. It is a device of great pedagogic usefulness, and I, for one, am guilty of using and advocating it in introductory courses. But it seems just that one should do penance for this sin in advanced courses, when the student begins to see the landscape of science as well as the singularly defined objects that occupy it.

conversion from weight to mass. Magnetic field strengths are in practice never found by the definition above because of the unavailability of unit magnetic poles. However, no serious objection to the view in question is occasioned by these facts, for it remains possible to say that the original definitions are *primary* and can be shown, by logical transformations and by laws of nature, to be equivalent to the ways in which the physical quantities are empirically determined. Formally, then, the view can still be maintained. But if it is maintained it seems well to note the intrusion, at this stage, of a new methodological element into the widened scheme for definition: Secondary definitions need *laws of nature* in order to be established.

View A becomes more hazy in the neighborhood of thermodynamics and the quantum theory, in fact in all regions where visualization fails. Consider a fairly familiar quantity, the temperature. Here the kind of definition widely considered as primary, which relates temperature to the measurable value of a common property of objects (length of a mercury column, color of a flame, volume of a gas), fails to be unique. Even if the same property (such as length), is chosen, one still gets different temperatures for different thermometric substances. To select one special substance, *e.g.*, mercury or an ideal gas, now appears as an arbitrary act which lacks the distinction of simplicity that marked the rigid body as peculiarly suitable for defining lengths. But this act can still be performed. If it is, the laws of nature (equations of state!) must be drawn upon in an even more drastic way than before to mediate between the different definitions of temperature. Were it not for the laws of nature, the proposition relating T to the pressure of a constant-volume hydrogen thermometer and the proposition relating T to the length of a mercury column could not both be called definitions of temperature. They would not define the same physical quantity. One thus gets the feeling that laws of nature somehow have a great deal to do with some definitions and that their role should be recognized in a discussion of the definitory process.

View A, which associates with every physical quantity a single definition, or at any rate a single primary definition, represents predominantly a classroom attitude on the part of scientists and is rarely held by philosophers. Being aloof from the specialization which scientific work entails, the philosopher (as well as many a scientist) sees at a glance the diversity of scientific definitions. And to achieve a semblance of unity he announces what we have called view B, namely that all definitions, despite their dissimilarity in details, follow a *single type of procedure*. This is done, for example, by Carnap and by Bridgman, who have analyzed the procedure in illuminating ways. To a critical review of their contributions we devote another section. Here we merely state the opinion that view B suffers from the same rigidity and inarticulation as did the more primitive view A. But that remains to be set forth more carefully.

12.2. The Definition of Force

Before attacking the question of definition on the plane of method it is desirable to discuss in some detail a specific example, one that will bring into evidence the various positions which have been or can be taken. We choose the physical quantity, force, and list a number of definitions in an arbitrary order somewhat corresponding to their degrees of refinement, but without an attempt to systematize them.

1. First is the crude, prescientific statement that force is the cause of motion. It is mentioned here mainly to exhibit its inadequacy and to discourage its popular use. To be sure, there was a time when this statement was satisfactory because motion was understood in a very specific and technical sense as increment in vector velocity and the word *cause* stood for dynamic enforcement. D'Alembert's principle, in its original form (before Mach converted it into the hybrid which now goes by this name), was based upon this definition.

2. Next comes the anthropomorphic formulation: Force is a push or a pull. It clearly relates the concept to kinesthetic human sensations.

3. To eliminate the human element, particular devices are often used to explain what a force is. One of the most popular is the spring balance. When the spring is extended this apparatus is said to exert a force proportional to its extension. The definition

has the advantage of permitting a simple quantitative specification of the magnitude of the force and hence its measurement.

4. A force is anything that causes a spring balance to be extended, and its value may be found by connecting the agency exerting the force with the spring and noting its extension.

5. Force is anything which neutralizes a weight, and its measure is the amount of weight which it holds in equilibrium.

6. Force is an agency causing departures from straight-line, uniform motion in material bodies.

7. Force is mass times acceleration, or, what amounts to the same thing except for very great (relativistic) velocities, force is the time rate of change of momentum.

8. In advanced dynamics, other quantities like the Lagrangian function, or the Hamiltonian function, or the potential energy are sometimes simpler concepts than forces, and it happens that a definition of force in such terms may be the most practical. An example would be: Force is the negative gradient of the potential energy.

Our list could be extended indefinitely. This feature is worthy of note: a given definition is often capable of unlimited proliferation into other forms which are not mere analytic transformations of it. How does it attain this strange potency?

Aside from the points of view already outlined in the preceding section, there is one other which requires comment. Definitions are sometimes classified as *qualitative* and *quantitative*. The former are those which give a sort of flavor of what the concept *is* or tell what it is not. The latter allow it to be fixed quantitatively. It will be seen that definitions 1, 2, 6 are qualitative in this sense. But let us not be deceived about the significance of this rather customary distinction. For in the first place it must be recognized as having no relevance to those scientific entities or constructs which by their nature cannot be measured. These comprise the large class of systems which were treated in the previous chapter. An electron, as an entity distinct from its properties (charge, mass, position, wavelength), has substantival significance only. With respect to it, even the best definition remains qualitative. As for quantities (and forces are quantities), a good definition must indeed be quantitative or refer to procedures for measurement. Now some of our definitions of force are inherently quantitative (7 and 8), others imply instrumental quantification (3, 4, 5). Turning our attention again to the remaining so-called qualitative definitions (1, 2, 6), we see that they owe their character of being qualitative to a defect of statement or understanding, not to an intrinsic peculiarity. Definition 1 becomes quantitative if cause and motion are quantitatively defined, definition 2 if pushes and pulls are assumed to be measured; to the mechanical engineer this definition is entirely satisfactory and quantitative. Definition 6 also takes on quantitative precision when means are found for numerically evaluating the departures from uniform straight-line motion. Hence, in a basic way, all definitions of our list aim to be, or can be made, quantitative ones. The only type of definition which is intrinsically qualitative is a denotative one. To point to a sensory situation and utter the word *force* is a process which per se contains no nucleus developable into quantitative precision. For this reason denotative definitions are never allowed in an exact science except to the extent that language intrudes them unavoidably. All definitions of quantities are therefore quantitative at least in a rudimentary way, and we shall omit further consideration of this distinction for the present.

We now discuss our list in the light of what was called the thesis of singleness, which was further analyzed into view A and view B. The first of these would accept any definition as the standard one and adhere to it with determination. Items 2, 3, and 7 have borne the brunt of this endeavor. There is, in fact, hard feeling between the engineer and the physicist in our present era of enlightenment because the engineer likes to select definition 2, whereas the physicist perfers number 7. The fight is being waged mainly among textbook writers. Definition 3 is sometimes accepted by both factions as a suitable compromise.

The more liberal version of view A, which acknowledges one primary and tolerates a number of secondary, or derived, definitions, is typified by either 4 or 5. Each of these can be extended empirically, giving rise to further measures of force. For instance, we can attach any other elastic body to the spring, pull on it, and find a correlation between the simultaneous extensions. Or we can correlate definitions 4 and 5 by seeing what weights extend the standard spring through given amounts.

Now comes the proponent of view B, who claims that physical definitions must be of similar *type*. He is most likely to be an empiricist and will therefore select as satisfactory numbers 3, 4, 5 from our list. If he chances to be a rationalist he will prefer numbers 2, 7, and 8; but rarely will he allow them to be mixed. The two "types" are to be subjected to further study in the sequel, but we see already one remarkable difference between them. The first appeals directly to observation; in fact it becomes powerless except when the definition has one leg in Nature. The second gets along without such a direct appeal; it may be said to define force in terms of other quantities (which may themselves be defined more directly by observations). But lacking directness, it somehow compensates for it by saying *what* force is meant to be, while the more empirical definitions seem merely to indicate *how much* force is present and leave the internal meaning of the quantity vaguely suspended.

12.3. The Empiricist's Approach

In recent philosophy, the processes by which science forms its definitions have been subjected to most careful scrutiny by the logical empiricists. Carnap,[1] in particular, has penetrated the problem successfully to the full extent compatible with the positivistic limits of his philosophy. We review his results in some detail.

The formation of physical concepts, according to Carnap, proceeds in three stages, the qualitative, the quantitative, and the abstract. The first stage comprises the evolution of largely prescientific concepts such as things with constant properties, and Carnap holds that things and their properties are recognized and defined by induction. To this point, and to what he conceives a thing to be, we must return later. The property of a thing is its mode of reaction to certain conditions; the constancy, or permanence, of properties is not given but is inferred from observations.

[1] R. Carnap, "Physikalische Begriffsbildung," G. Braun, Karlsruhe, 1926.

By far the largest part of Carnap's treatment concerns the quantitative stage, the establishment by definition of physical quantities. A physical quantity has no meaning except through the ways in which it can be measured; its meaning (*Sinn*) exhausts itself in the circumstance that definite numbers are ascribed to definite objects. The definition of a physical quantity is the fixation of rules whereby this assignment of numbers is accomplished. And it is the logic of the assignment of numbers which is worked out by Carnap with impressive clarity.

A distinction is made between the topological and the metric definition of a physical quantity. The topological definition aims at knowing when two objects have the same value, when one has more, and when it has less, of a given quantity. To establish it the scientist must find two relations between objects, one which is transitive and symmetric and one which is transitive and asymmetric. The first permits the specification of equality, the other the sign of a difference. One might comment here on the fact that, as far as physics is concerned, topological definitions are rather academic, no attention being given at present to quantities for which a metric definition is not available.

In forming a metric definition the physicist does three things: (*a*) he chooses the form of the scale upon which he wishes to measure the quantity; (*b*) he chooses the zero point on this scale; (*c*) he chooses a unit of measurement. An example will illustrate what is meant. If the temperature T is to be measured, let us say by noting the length of a mercury column, l, one must (*a*) decide what form of function T is to be of the argument l, (*b*) what temperature shall be called zero, (*c*) how many degrees there shall be between the zero point and some other fixed point of the scale.

The meaning of this analysis, put more succinctly, is this: One wishes to establish a functional relationship, $T = f(l)$. This requires (*a*) specification that f is linear; a linear function, $f(l) = al + b$, requires the fixation of the two constants a and b. One sees from this version, however, that Carnap's three requirements, (*a*), (*b*), (*c*), are not quite general. If the relation were not a linear one, more than two constants would be needed to tie down the function, and the choice of zero point and unit would not be sufficient. Fortunately, however, physicists always prefer the simplicity of the linear scale. Using the method thus established, Carnap discusses the most important concepts of classical physics (length, temperature, time, velocity, acceleration, mass, and electrical charge) in a manner at once refreshingly factual and precise.

The abstract stage of definition involves the construction of a four-dimensional (relativistic) continuum. It is quite certain that this chapter in Carnap's booklet, if it were written today, would be far more extensive, should, in fact, make up the major part of the work. Modern physics has become populated with abstract entities to an extent which allows it to be said that its whole method needs revision and that the moral drawn from older branches stands in need of essential generalizations. It is from this wholly unforeseeable contingency that Carnap's careful analysis now suffers. Its logic of measurement, which constitutes its core, remains intact, and we shall try to indicate now in what important respects it should be modified. Some of the points which follow receive their substantiation in Chaps. 16 to 18, but most of our critique will center in matters already discussed.[1]

If we tried to apply the technique of measurement and its attendant forms of definition to the position of an electron or of any elementary particle of physics, or indeed to the state (φ function) of any atom, molecule, or photon, it would fail. And it fails chiefly because the system, or the carrier of observables, must be *more* than all the observations performed on it when there is no assurance that the observations possess stability. We are referring here to the presence of latent observables. To put the matter more concretely, when we see a particular stone we always find it gray, hard, of determinate shape, heavy, and so forth, and there seems to be nothing worth saying about the stone which is not summed up in these properties. In the case of atomic particles most observations scatter, and one achieves coherence by pulling them together in common reference to a *system* functioning, as it were, behind them. Now we desire to state clearly that there is no rigorous logical compulsion forcing us to insert such reference.

[1] For the reader who wishes a succinct critical review of logical positivism we recommend Max Black's article, *Mod. Quarterly*, 1:51 (1938).

The physicist (at least as long as he professes no interest in reality) *can* do his work without it. He would make a great sacrifice, however, for aside from incurring a need for stilted language, he would be forced to accept a very elaborate scheme for making sense out of the scattering welter of his observations. What in fact he does is to choose the simplest unifying procedure; he *constructs* electrons, atoms, and so forth, talks about them as systems, and uses them as the carriers of observables. In classical physics, the possessive nature of all properties suggested a carrier so obviously that its specific postulation was almost unnecessary. *In modern quantum theory the carrier thus suggested threatens to dissolve itself and has to be stabilized by a conscious and deliberate methodological act.*

Of course the importance of a clear recognition of systems even in classical physics is great, and it was emphasized in the preceding chapters. In so far as systems are different from their properties they cannot be defined in Carnap's second, or quantitative, stage, and we must conclude that they are either taken for granted or owe whatever meaning they possess to the more rudimentary first stage of definition. But here we encounter a difficulty. We are told how, in the establishment of a thing through its properties, we assign to it (of course there is as yet no *it*, but let us take the presence of a thing for granted!) a permanent property, like being red at all times, by an *inductive* procedure which generalizes an act of perception. Here, I take it, is the hard kernel of empiricist philosophy. What can be attained by induction is the *probable* validity of the statement: This rose will appear red to me whenever I look, but not the factual statement intended by the physicist with all its unadorned simplicity: There is a rose, and it is red. This requires more than induction—it requires what we have called *construction*.

Now it is perfectly clear that Carnap's analysis is not in error; he means the proper inductive statement, and he wishes to exhort the scientist to mean it too. But it seems that physics has gone and is going the other way. While the pattern of systems, observables, and states is genuine in classical physics, it is *indispensable* for an understanding of the quantum theory. And a scheme which does not permit us to face each of these entities directly is ill-suited to physical science.[1]

A more basic difference between Carnap's position and the one developed in this book, perhaps the difference which accounts for the criticism made above, lies in our unwillingness, and in the unwillingness of most scientists, to start philosophic inquiry on the level of language. The physicist attaches much less importance

[1] Carnap's later views on this problem depart even more widely from the tenor of science. In his Testability and Meaning [*Phil. Sci.*, 3:419 (1946)] he develops the following scheme: Let Q_0 be a physical assertion, *e.g.*, There is an electric current flowing through a wire. Q_1 is a sentence describing an electrical network; Q_2 expresses the deflection of a galvanometer needle. He then forms a reduction sentence:

$$Q_1 \supset (Q_2 \supset Q_0)$$

The negation of Q_0 is given by another reduction sentence:

$$Q_3 \supset (Q_4 \supset \sim Q_0)$$

Such a pair of reduction sentences specifies a condition under which we say a current is flowing and another condition under which we deny it. It alone, however, cannot form a definition. But suppose that we have a large number of pairs, all similar to the above:

$$Q_{4i+1} \supset (Q_{4i+2} \supset Q_0)$$
$$Q_{4i+3} \supset (Q_{4i+4} \supset \sim Q_0)$$
$$i = 0, 1, 2 \ldots$$

In terms of these, Carnap attempts to define Q_0 by saying

$$Q_0 \equiv Q_1 \cdot Q_2 V Q_5 \cdot Q_6 V \cdots V Q_{4i+1} \cdots Q_{4i+2} V \cdots \qquad (12.1)$$

This procedure is at odds with physics on two very serious counts: (*a*) Unless $i \rightarrow \infty$ and we have explored the entire universe, the definition of Q_0 is not complete. The physicist feels that a quantity can be defined even before *all* its incidental properties are known. (*b*) According to formula (12.1), Q_0 is identical with any or all the disjunctions on the right. The physicist thinks of Q_0 as being able to *generate* conditions of its occurrence which are not contained in its definition.

It seems that this theory pays for its logical explicitness by a loss of contact with science.

I should like to add that I am not certain whether Carnap holds the view here criticized at the present time. Indeed I have reason to believe on the basis of oral utterances that he would now accept our constitutive definitions. The foregoing remarks were prompted because the view in question is clear and unambiguous and is held by numerous logicians and scientists today.

to his physical language than does his generous interpreter; *experience and ideas are primary to him.* He thinks, no doubt with some flattery to himself, that he is more at home with experiments and ideas than with linguistic or any other kind of symbols. Certainly the psychological experience of every investigator is one of thought, of action, of adventure. Sudden recognitions break through unbaptized, ideas are born before they are named. Sometimes the results of a complicated theory are foreseen in general outline even before a word is spoken or a mathematical symbol is put on paper. Through his insistence on analyzing the physical language and little else, Carnap honors but restrains physical activity. We hold, with Lewis,[1] that "linguistic expression of what is meant and what is apprehended is the dependent and derivative phenomenon: it is meaning and apprehension themselves which are the fundamental cognitive phenomena, and these are independent of any formulation in language."

The main value of Carnap's contribution to the problem at hand lies in the clarity with which he formulated that kind of definition which leads to measurement. As we shall see, there is another kind of which the importance is usually minimized by the empiricist but which must be added to complete the picture.

Among physicists, Bridgman is the advocate of a special form of empiricism. His approach is not through logic; he advances across the less formalized but fertile fields of actual physical investigation and examines the elements which have made for progress in science. They are found to be *operational definitions.* His emphasis is similar to Carnap's inasmuch as the process of measurement is made the central part of all definitions, only the spotlight has shifted from the thing to the observer. According to Carnap it was nature whose mode of reaction to changing conditions determined measurable properties for the observer; according to Bridgman the observer actively determines them by operations. But this difference hardly matters. In his first publication [2] Bridgman had his eye chiefly on the theory of relativity

[1] C. T. Lewis, "An Analysis of Knowledge and Valuation," The Open Court Publishing Company, La Salle, Ill., 1948.

[2] P. W. Bridgman, The Logic of Modern Physics, The Macmillan Company, New York, 1927.

and on the early theory of quanta, which are both well suited to drive home the lesson of doom for errant theories which stray too far from the field of observation. Since that time the quantum theory itself has moved away from observation, and the lesson is not quite so clear.

Precisely what is meant by an operation is of concern to those who wish to understand Bridgman's views. Originally there seemed little doubt that *experimental* operations, processes leading to measurement, were envisaged. Later, however, Bridgman has spoken of paper-and-pencil operations and even of mental operations. This seems like a retreat, for if thought were included among the operations, nobody could possibly find fault with operationalism, nor would it be saying much.

Again, what we take to be the important contribution is Bridgman's emphasis on instrumental—and we shall take this to mean experimental—procedures as necessary parts of certain definitions. Idea and term, operational definition, are excellent and will be further employed as this book proceeds. But as we have said before, they require supplementation, and to this final integrating task we must now attend. In the light of the more constructive remarks which follow the foregoing criticisms will, we hope, take on a higher degree of coordination than is thus far apparent.

The remaining sections of this chapter are devoted to an effort to demonstrate the insufficiency of every monolithic theory of definitions—be it that of the physicist who insists on universal linkage with immediate observations or that of the philosopher who denies meanings which do not spring from measurements—for all quantities of importance in physical science. We hope to do this by first examining a few of the simpler concepts of physics.

12.4. Constitutive vs. Epistemic Definitions

The speed of a moving body is one of the easiest physical quantities to define. It is the distance traveled divided by the time, or, in general, dx/dt. This seems to settle the issue, and a physics teacher often considers his duty done when he has taught this definition. Because the definition relates velocity to distance and time, velocity becomes a "derived" quantity (Carnap) in contradistinction to distance and time, which are primary since they can be defined directly in terms of operations. The presumption is that there are two kinds of quantity because some are defined directly, others mediately.

But what is wrong with the instrumental definition of speed which involves a speedometer? This now very common device does not measure the distance traveled and divide it by the time. It uses a principle now recognized as saying (roughly) that the electromagnetic drag of a moving metal part on a magnet is proportional to its speed. The average driver does not know this; to him the speedometer indication is the primary quantitative definition of speed. Why, then, should speed be a derived quantity? We believe that it is not and that the second definition is as good as the first.

Take another so-called "derived" quantity, the acceleration. It is d^2x/dt^2, a mixture of the primaries, distance and time. But again we have accelerometers, devices which measure the quantity directly by instrumental means, and without recourse to presumed primaries. It is possible to show of course via the laws of mechanics that the quantity so measured is equal to d^2x/dt^2; this, however, confers no primacy upon either definition.

What is here illustrated with respect to speed and acceleration holds for all physical quantities usually regarded as "derived." *Area* can be defined without reference to measurement in many ways (for example, $\int dx\,dy$), but the insufficiency of all these definitions has in recent years become annoyingly apparent to physiologists who needed to know the surface area of the human body in their accurate metabolic studies. The derived definitions were of little avail; men had to devise means for measuring areas directly. In doing so, they produced the equivalent of an operational definition.[1] (A similar though more familiar story could be told about *volume*.) *Density*, usually defined as mass per unit volume, can also be obtained directly by a method involving Archimedes' principle and no volumes whatever. (*Power*, regarded

[1] See, for example, the discussion of actual methods used in measuring areas by E. Boyd, "The Growth of the Surface Area of the Human Body," University of Minnesota Press, Minneapolis, Minn., 1935.

as energy spent per unit time, has several operational definitions not equivalent to this simple statement. The electrical wattmeter, for instance, does not divide measured energy by measured time.) Examples which illustrate this *dual* mode of definition, one operational and the other somehow independent of measurement, are available without limit for all quantities ordinarily conceived of as derived from others.

But what about time, distance, and mass, physical quantities usually considered as primary? Brief inspection shows that they, too, are subject to dual definitions. *Time* is operationally defined by reference to clocks (cf. Chap. 7); on the other side we have already encountered a perfectly valid and very general nonoperational definition of the form: Time is the independent variable in the equations of mechanics. How *space* (distance) is defined operationally with employment of rigid rods or light signals has also been discussed in Chap. 7. A second, but not secondary, definition of distance would be the time integral of speedometer readings.

We shall leave the discussion of *mass* until later.

The relativity and multiplicity of definitions must be obvious when thus exhibited, and one is disposed to ask himself why it is not more generally recognized. The answer is probably twofold. First, operational definitions of "derived" quantities, like velocity, acceleration, and area were retarded in their development because devices for measuring them were not needed until fairly recently. They are not new (Hero of Alexandria invented a kind of speedometer) but did not impress themselves upon the minds of scientists as having the same potential significance as clocks and meter sticks. The absence of operational definitions in the case of some quantities made this type of definition appear the less universal, though possibly the more important, and led to the classification of these quantities as "derived" from others.

Second, the distinction between primary and secondary physical quantities tends to be kept alive by a confusion between the *definition* of a quantity and the assignment of a *dimension*. Hence a word ought to be said about physical dimensions.

In mechanics it is possible to build up the unit of measurement for every quantity from the units of three others, and in a certain way the definitions of the quantities themselves can be thus compounded. The three units most commonly chosen as basic are the units of length (centimeters), time (seconds), and mass (grams). For example, the reader may recall that force is measured in gm cm sec^{-2} (dynes) and power in gm cm^2 sec^{-3} (ergs per second). With some artificiality, this composition can be carried through in thermodynamics and in electromagnetism. Now a formula which displays the manner of composition from time, length, and mass of a given physical quantity, such as

$$\text{Force} = [MLT^{-2}] \qquad \text{or} \qquad \text{power} = [ML^2T^{-3}]$$

to use the customary symbolism, is said to represent its physical dimension. This analysis has naturally led the physicist to look upon mass, time, and space as basic entities from the point of view of dimensions.

Now it is true that dimensional analysis, which is not identical logically with definition, can indeed be made to yield one kind of definition. However, there are many others which are not related to dimensional analysis except through theories of nature. And above all, dimensional analysis is not unique. *Any* three mechanical quantities can be made the basis of a system of dimensions; engineers, for instance, use force, length, and time in place of the three used above. It is seen, therefore, that dimensional analysis has nothing to say about the logic of definitions, although it probably has predisposed scientists to the view that certain definitions are more basic than others.

Without assigning priority, we have noted a difference between two large classes of definitions, a duality which must now be more carefully specified. Let us return to one of our examples, time. One way to define it is by reference to a clock; another views it as the independent variable in the laws (and in the equations) of motion.

The first is operational, to use Bridgman's term; it might also be called instrumental in order to leave no doubt that an actual use of physical instruments is involved. In an earlier publication [1] I have called it *epistemic;* it establishes a rule of correspondence between the construct, time, and Nature. This represents the

[1] H. Margenau, *Phil. Sci.*, 2:48, 164 (1935).

true character of every member of this class of definition, as a review of the preceding instances in this section will show. Operational or epistemic definitions are always rules of correspondence.

The second definition of time does not address itself to Nature. It remains in the C field, linking together several constructs. It tells *what* time is in a way the epistemic definition, which serves mainly to assign numbers, does not do. But it provides no independent measure of time. For want of a better name let us call it a *constitutive* definition. Looking at Fig. 5.1 we now observe that every double line is the equivalent of an epistemic definition, every single line a potential generator of a constitutive one.[1]

12.5. Brief Survey of Epistemic and Constitutive Definitions in Physics and Mathematics

An exact science is an elaborate system of definitions of both types, the reasons for their equivalence being known through the theoretical structure of the science. In a sense, epistemic and constitutive definitions span the entire structure of physical and mathematical theory. It is true that the *facts* of any science are known when all its epistemic definitions and all empirical relations connecting them are known. This much, however, leaves the science a bare field of knowledge without the dynamism of self-evolution and without an internal gauge for self-correction. Such a body of knowledge can grow only by accretion of factual material from without. Only when there is the possibility of an interplay between constitutive and epistemic elements, when, paradoxically, the scene of science exhibits the tautological superabundance of descriptive factors which inheres in the multiple definitions of each physical quantity, does science possess the

[1] This issue is extremely important in economics and the social sciences, although it usually goes unrecognized. Crucial quantities like price indexes, degree of inflation, and national income are very clearly defined operationally, *i.e.*, on the epistemic side. Frequently there is a multiplicity of epistemic definitions. This clarity with respect to epistemic meaning is then often mistaken for theoretical significance. Let it be noted that, so long as a properly matching, constitutive definition of each of these terms is missing, their scientific use is insecure.

desired degree of deductive fertility. Positivism in its extremer forms is one-sided in not wanting to grant the importance of constitutive definitions; it pursues a *logical* ideal of simplicity, the avoidance of reduplication of physical description; but in doing so it sterilizes science.

A given physical quantity in general possesses more than one epistemic and more than one constitutive definition. Were it not for the coherence provided by the latter type, the existence of several epistemic definitions would be an absurdity, for there is no way in which two different instrumental operations can be certified on their own merits as specifying the same thing. The numbers established by meter-stick operations cannot be shown to have anything in common with what is operationally referred to by light-signal experiments. In calling both *lengths* we tacitly acknowedge, not merely a limited sort of empirical equivalence between the two operations, but a logical identity which draws its evidence from constitutive definitions.

Finally, we resolve another dilemma. We saw that epistemic definitions, such as those of the early Carnap and early Bridgman type, cannot function in giving meaning to *systems*. From this circumstance the positivistic school has often drawn the wholly unwarranted conclusion that systems do not "exist" in the same sense as physical quantities.[1] This is what they are forced to maintain because of their illiberal premises. Constitutive definitions *do* allow the establishment of systems. For the statement, an electron, or an atom, or a mass point, or a field, is *that which* has such and such properties, is a constitutive definition. One can, in fact, distinguish between systems and quantities in any science by noting that the former are capable of one kind of definition only (constitutive), whereas the latter can be defined in both ways.

The variety of examples for both types of definition scattered through the preceding discussion is perhaps great enough to be a fair sample of what science offers. To make an exhaustive classification is an encyclopedic undertaking which would fill many volumes. But there occur a few instances which are either instructive or important and will therefore be reviewed.

[1] We remind the reader of Mach's polemic against the reality of atoms.

The point of view here outlined is applicable to all exact sciences, including mathematics. It happens that in pure mathematics constitutive definitions play a greater role than in the applied natural sciences, and this because mathematics generates *systems*—for which direct epistemic definitions are not available—with greater freedom than do other disciplines. As examples we mention points, lines, triangles, numbers, matrices, and groups. To these are assigned *properties* which are determinable epistemically and which are sometimes measurable.

To refute the claim that constitutive definitions are idle restatements of empirical facts we recall the ideas of triangle and group. All theorems about the properties of every triangle, proved by Euclid, were contained in its constitutive definition. On the other hand the epistemic definitions of length, angle by reference to meter sticks, etc., render the constitutive implications of a Euclidean triangle applicable to sensory experience.[1]

The group is even more impressive as a construct, which, though defined only constitutively, has nevertheless immense usefulness.[2] Interest in it arises partly from the practical applications it permits, but more notably from the light it sheds on other large classes of mathematical entities. Indeed it seems certain that group theory would enjoy great favor among mathematicians if nature did not contain a single empirical group. Here, then, is a system so heavily weighted with constitutive content as to render its epistemic aspects almost insignificant by comparison. Many other ideas of advanced mathematics are of this kind.

As a last example we consider the physical concept, mass, the meaning of which sometimes eludes the beginner because of its common confusion with weight.

[1] It is quite true that these epistemic correlations would not be made except for their applicability.

[2] For a precise meaning of a group beyond the qualitative indication given in Sec. 7.7, the reader should consult W. Burnside, "The Theory of Groups," Cambridge University Press, London, 1927, or a book on modern algebra like G. Birkhoff and S. McLane, "Survey of Modern Algebra," The Macmillan Company, New York, 1941. A concise summary of group theory is also given in Margenau and Murphy, "The Mathematics of Physics and Chemistry," D. Van Nostrand Company, Inc., New York, 1943.

The usual definition of mass given by Mach [1] is a model of completeness, but also a curious mixture of epistemic and constitutive elements. As a proper epistemic definition it should run as follows:

Choose a body *A* as the standard body, and regard its mass arbitrarily as unity. Let body *B* interact in any way with body *A*, and measure the simultaneous accelerations of both. If they are found to be a_A and a_B, respectively, the mass of *B* is $-a_A/a_B$.—Everything else that is usually encountered in Mach's definition is unnecessary baggage.

Now this epistemic definition leaves us a bit unsatisfied, for it might happen that the mass thus formulated turns out to be a function of, let us say, the time. An equally unfortunate contingency would be the dependence of the mass on the choice of standard body and on the kind of interaction. It is easily conceivable that the epistemic procedure results in different masses when bodies *A* and *B* are connected by a spring, when they are joined by rubber bands, and when they repel each other by virtue of being electrically charged. If this were true, the mass, as a physical property of a body, would not show external convergence (cf. Chap. 6), would be not a *possessed* but at best a *latent* observable (cf. Chap. 8). To forestall such misgivings, the customary presentation of Mach's definition smuggles in concealed references to Newton's second and third laws of motion, whereby the time-free as well as the possessive character of the mass, epistemically defined, may be established.

To disentangle the logical skein we need merely recognize that, in addition to the epistemic definition above, we have another, based on Newton's laws. It states that mass is the ratio of force to acceleration. The glory of mechanics is that both definitions work.

Of course if we postulate Newton's laws, the two definitions are

[1] See the classic treatise, "The Science of Mechanics," by E. Mach (p. 216, Open Court Publishing Company, La Salle, Ill., 1907). For a brief modernized version, see R. B. Lindsay and H. Margenau, "Foundations of Physics," pp. 92*ff.*, John Wiley & Sons, Inc., New York, 1936. Familiarity with the details will here be assumed; but the example can be omitted without detriment to the continuity of our story.

not independent. Neither one is favored by the laws, however. To recognize only one kind of definition and then use the laws to get the other puts a philosophic slant on science which we want to avoid.

12.6. The Interplay between Definitions and the A Priori

The relation of our treatment of definitions to the analysis of scientific method in Chaps. 4 to 6 is fairly obvious and requires only brief comment. As already stated, a constitutive definition represents a postulated grouping of constructs, remains wholly within the *C* field, and is therefore empirically not verifiable. An epistemic definition is always a rule or a set of rules of correspondence.

Inasmuch as the boundary between the *C* field and the *P* plane is not sharp, and uncertainties may well arise as to the location in *C* or *P* of certain elements of experience, there can be corresponding ambiguities in the nature of definitions. Thus we have taken the definition of time by means of a clock as an epistemic one. The fact is, however, that in a very basic sense the clock itself is a construct so that this definition remains, after all, in the *C* field and might therefore be called constitutive. But the intent of the definition is certainly clear: it means to drive toward Nature in unmistakable terms; though it presumes them, it lacks explicit rules of correspondence which connect the clock itself with immediate sensation. To make the definition epistemic in form as in content we need to add this explicit reference, *i.e.*, the reifying correspondences which link immediate sensations with the external object, clock. Such is often the case; essential ambiguities, that is, ambiguities which cannot be thus removed, do not occur.

While an incomplete epistemic definition can be interpreted as a constitutive one, an incomplete constitutive definition often appears as an epistemic one. We have regarded the statement, Speed $= x/t$, as a constitutive definition (for a special kind of motion). This is correct provided that we mean by x and t the constructs which indeed they generally are. Such was our implicit understanding. If, however, someone chooses to mean by them a pair of numbers obtained operationally for the purpose at hand and adds this stipulation to the definition, he will have to view it as an epistemic one. The ambiguity here encountered can again be removed by a complete statement of the meaning of terms. We note once more that he who fails to distinguish between x, t as constructs and x, t as data of observation is forced by this restriction to view all definitions as epistemic ones. To him our remarks cannot make sense.

The interplay between the two types of definition makes science a going enterprise. When a sufficient number of equivalences between constitutive and epistemic propositions concerning a set of physical quantities is understood, we have a physical theory. The equivalences then take on the character of laws. And, conversely, laws may take on the character of constitutive definitions. This interesting methodological development runs through the entire history of physical science.

Its importance has been recognized and carefully studied by V. F. Lenzen,[1] who speaks of "a method of successive definition" and applies it to the subject of dynamics. He puts the gist of it as follows. "There is an abstraction of concepts from experience, the discovery of laws expressed in terms of the concepts, the definition of the original concepts to a higher order of approximation in view of the greater accuracy in the definition of conditions, the redefinition of concepts in terms of laws, the reinterpretation of original concepts in terms of the new."

To illustrate the process in the terms of our analysis, we apply it to the physical quantity, mass. Historically it was first defined epistemically by means of balances and so forth, in a manner which did not allow it to be distinguished from weight. Next, it was found to satisfy Newton's laws of motion—provided that it was more precisely defined as the ratio of force to acceleration. In this way, the discovery of laws led to a constitutive redefinition of the original construct which in its turn rendered the epistemic definition more precise (Mach's form; see preceding section). Then came the discovery of more general laws in the theory of relativity through which mass could be defined constitutively as a

[1] V. F. Lenzen, "The Nature of Physical Theory," John Wiley & Sons, Inc., New York, 1931. See particularly pp. 274*ff*.

certain parameter in the metric of space-time. Using this, a new epistemic definition, which links this parameter to observations, has become available.[1]—To study the whole history of physics from this point of view would be an interesting task.

The developments of this chapter have a bearing on the meaning of "a priori" knowledge. In Chap. 7 the impropriety of the distinction between the a priori and the a posteriori was suggested. The persistence of this distinction in philosophy is symptomatic of a continued failure to recognize the difference between constitutive and epistemic definitions. A proponent of the a priori is a person who, having accepted a constitutive definition and having become steeped in its analytic implications, then asserts its automatic applicability to nature. He forgets the intervention of rules of correspondence, that is, of epistemic definitions which were chosen *in order* to make the concept descriptive of actual experience.

Along with the a priori problem, the difficulty of understanding why nature obeys formal laws thus also vanishes. For it is not true that formal laws apply directly to immediate experience. There are further "parameters" available for adjustment, and these lie within our epistemic definitions. Our freedom in choosing them gives to science the flexibility it needs to grasp experience, and it takes away the character of the miraculous from the fact of nature's obedience to laws.

SUMMARY

In the first three sections of Chap. 12 the problem of how scientific quantities are to be defined is discussed from a variety of angles, and a somewhat general criticism is directed against attitudes that are too restrictive in according meaning to the concepts of physical science. Singled out for specific inspection are (1) the claim sometimes supported by teachers of science that every scientific quantity has but one proper definition; (2) the more elaborate theories of Carnap and of Bridgman, which pre-

[1] In the general theory of relativity the g_{ik}'s of Sec. 7.6 are functions of masses. Knowledge of the g_{ik}'s therefore determines mass and can be used for its definition. It should be clear to the reader, however, that this is a rather artificial procedure.

scribe unique methods to be followed in the process of definition. It is held that these views require supplementation of a sort which will bring into evidence the nonempirical relations between scientific concepts without, however, destroying their fruitful linkage with observations.

The last three sections are devoted to the more constructive task of showing how this can be accomplished. Two general classes or types of definitions are distinguished: those connecting constructs with immediate experience and those relating constructs with one another within the C field. The former are called *epistemic*, or operational, definitions; they arise from, and are equivalent to, our former epistemic correlations or rules of correspondence. The definition of time by reference to clocks is of this type. The second class is called the class of *constitutive* definitions, an example being the definition of time as the independent variable in the laws of motion. A given physical quantity may have a number of epistemic and a number of constitutive definitions.

The interplay between the two types makes science a going and self-correcting enterprise. Without epistemic definitions science degenerates to speculation; in the absence of constitutive definitions it becomes a sterile record of observational facts and its formulas take on the character of medical formulas. Physical laws may be regarded as mediators between the two types of definition for specific quantities. In the development of a science, the discovery of a law often leads to new constitutive definitions, and, conversely, a new definition of this type may generate a law.

The problem of the a priori is placed in a new light by the distinction here emphasized. The proponent of the validity of a priori judgments is a person who, having accepted a given constitutive definition and having become steeped in its analytic implications, then asserts its automatic applicability to nature. He forgets the intervention of epistemic definitions, which were adopted to make the concept descriptive of actual experience.

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CHAPTER 13

Probability

13.1. Probability and Inductive Logic

A subject as large and as ramified as probability cannot be dealt with, indeed cannot even be summarized, in a single chapter. It is important, therefore, that we select for discussion those phases which have the most direct bearing upon epistemology. In view of the necessity for such discrimination, two things cannot be attempted. First we must forego all pretense of giving an impartial account of the numerous philosophical attitudes which thinkers have taken toward the meaning of probability. The reader who desires one is referred to Ernest Nagel's competent and critical review.[1] Second we are forced to omit consideration of technical details except in so far as they are needed for basic understanding and for stable terminology. The number of books supplying such knowledge is large. A small list that is conformable to our purposes and also reasonably representative of different views is given below.[2]

There is another reason for the restrictions we are about to impose, a reason which makes the need for our limitation almost a fortunate circumstance. General discussions of the meaning of probability by philosophers have lately shown little evidence of agreement upon any common view, and the literature is becoming progressively more confused. As a case in point we cite a recent

[1] E. Nagel, Principles of the Theory of Probability, "International Encyclopedia of Unified Science," Vol. 1, No. 6, University of Chicago Press, Chicago, 1939.

[2] P. S. Laplace, "Essai philosophique des probabilités," Paris, 1814. R. von Mises, "Wahrscheinlichkeitsrechnung und ihre Anwendung," Leipzig, 1931. J. V. Uspensky, "Introduction to Mathematical Probability," McGraw-Hill Book Company, Inc., New York, 1937. T. C. Fry, "Probability and Its Engineering Uses," D. Van Nostrand Company, Inc., New York, 1928.

symposium [1] designed to clarify the problems in this field, but which achieved little more than a demonstration of this confusion. The volume of rhetoric enveloping the least important issues and the partisan tenacity with which they were argued upon that occasion raise justifiable doubts as to the desirability of reviewing all attitudes. In contrast to such performances, however, we note the increasing clarity which surrounds the idea of probability on the technical mathematical side, and such deepened understanding of it as has come about during the last decades by an application of the theory of sets.

It may be that, by concentrating attention upon the cardinal part of the problem, namely, upon its epistemology, the confusion can be resolved. Such happens to be my own conviction, and it arises from this simple observation: What appears in the philosophy of probability as a tangle of conflicting ideas falls into perfect order when viewed in the light of the dual mode of definitions, developed in the preceding chapter. The picture is like a stereopticon, hopelessly confused when seen by the unaided eye, but full of depth and order when seen through the two-color spectacles of that theory. This is one of the points we wish to make in the following sections. Our other aim is to provide enough factual equipment so that we may then proceed to study two modern sciences heavily weighted with methodological implications and equipped with signposts toward reality that cannot be ignored: these sciences are statistical mechanics and quantum mechanics.

As everywhere else in this book, our concern will be centered in theoretical science and will issue thence to embrace all of science. Hence the notion of probability will be studied as it functions and as it is now conceived within science. This, too, involves a drastic limitation, for there may be very good probability formulations differing from those in scientific use. We shall retain the attitude previously adopted when, for example, in studying four-dimensional space, we paid no attention to a popular belief which sees in the fourth dimension the habitat of departed

[1] "Philosophy and Phenomenological Research," Vol. 5, pp. 449–484, Vol. 6, pp. 11–86, 1945. Participants: G. Bergmann, Carnap, F. Kaufmann, Margenau, von Mises, Nagel, Reichenbach, and D. Williams.

souls; nor did we consider "pieces of land under cultivation" when discussing the physics of fields. The scientist insists upon a very definite understanding and makes a very definite use of dimensions and fields as well as probabilities, and it is only this understanding and this use which are of interest here. When the physicist says the probability of decay of a uranium nucleus is 4.3×10^{-18} per second, he means to say something quite specific, and we accept and respect that meaning. When he shrugs his shoulders in response to the question, What is the probability that the *theory of relativity* is correct, we respect that answer, too.

Part of the existing confusion is due to the missionary fervor with which everybody tries to tell the scientist what he *ought* to mean by probability. It is as though the politician told the physicist what he should mean by "power," the army general what he should mean by "forces." As is true with every other word, probability, as used in speech (and used with perfect propriety), has a variety of meanings, among which the scientist has a right to choose.

We believe that he has made his choice and that he does not identify the theory of probability with *inductive logic*, as is so strongly urged by Carnap, Reichenbach, Hempel, and others. What these men do in effect, and do successfully, is to propose a new formal science, the theory of inference; they are not making comments upon what is now conceived as probability in physics and in the exact sciences derived from it. In evidence for the appropriateness of this judgment one might point to the failure of the efforts on the part of inductive logicians to make contact with the crucial and burning problems of physical science. Lest our judgment seem harsh we remind the reader that this book is wholly devoted to exact, not to correlational, science. The difference was carefully outlined in Chap. 2. No criticism whatever is intended here of the methods of experimental inference, which, however, we regard as penultimate and as insufficient for a complete definition of physical reality.

Inductive inference is itself a science involving constructs and metaphysical requirements, and many of the judgments it makes are far from inductive. This should be fairly clear even from the very inadequate account we gave in Chap. 6 of the problems of internal and external convergence and of the theory of errors. In physics, chemistry, and large portions of biology, theories of inductive inference are employed in the adjustment of data, but they are employed like every other theoretical formalism replete with constructs and correlations. It is safe to say, for example, that, if someday the usual methods of adjustment were to prove unsuitable, new axioms and rules of correspondence with immediate observation would be devised. The whole subject of empirical inference is not more, or less, thoroughly grounded in Nature than is any other theory of science.

The incidental value of correlational methods is of course very great even in the exact sciences. For, as we have shown in Chap. 6, they are always needed in order to make the verdicts of Nature unique and unequivocal, to trim uncouth immediacies before they are allowed to enter the parlor of constructs. And there are many instances in physics where correlational methods have the last word, where the deliverances of Nature never even see the tidy household of constructs. In our measurement of the constants of nature we have instances of this sort. To obtain the best value of the speed of light, for example, very elaborate correlational procedures have been used,[1] and there the story ends, for theory is at present unable to predict the value of c on independent grounds. But, characteristically, physicists smart under the realization that this should be the case. They feel that some day they will have a theory which will relate the value of this famous constant to others: the circumstance that correlational methods leading to a brute-fact best value of c without context might be the last resort is generally felt to be unfortunate.[2]

Many scientists thus look upon the theory of inductive inference, with all its practical importance, as a *substitute* for true science. Inductive inference teaches what it is best to do in the face of *incomplete* evidence, where exact science fails. To be sure,

[1] See R. T. Birge, *Rev. Mod. Phys.*, **13**:1933 (1941). J. W. M. Du Mond and E. R. Cohen, *Rev. Mod. Phys.*, **20**:82 (1948).

[2] The reader who desires a philosophic account of correlational methods with an emphasis that counteracts the selective presentation of materials in this book is referred to C. W. Churchman, "Theory of Experimental Inference," The Macmillan Company, New York, 1948.

as a man of larger concerns the physical scientist indeed has an interest in this theory, as he has an interest in his meals. But he also recognizes those situations in which inductive inference is at present necessary—numerous though they are—as challenges for an application of scientific method. Thus he tries to avoid induction, and he inclines to look upon fields yielding only to inductive analysis as unconquered territory rather than his own proper domain. This feeling is of course not universal, but it is dominant and seems to be increasing among the practitioners of natural science.

The exact sciences are, after all, deductive in their major phases, and the correlational sciences, in striving to become exact, implicitly endorse the methodology of the former. As we have emphasized repeatedly and illustrated by an abundance of examples, the logical movement of every sufficiently developed science is from the general to the particular, from postulates to theories to theorems to specific predictions. History and the psychology of discovery often reverse this course, it is true. But to argue that this destroys the deductive character of science is to mistake temporal (or psychological) sequence for logical and epistemological structure.

Every individual scientific discovery refers, beyond the factual circumstance in which it arises, to some universal for its *significance*. The difference between noting a fact and making a discovery centers precisely in this crucial condition: that a discovery suggests a fairly general postulational proposition which presses for tentative acceptance, while the fact allows mere inductive generalization. The postulate, when analyzed, is replete with deductive consequences, each of which says more than the original discovery; the inductive generalization as such can say nothing save what might happen when similar facts are inspected.

The "suggestion" of significance by discovered facts is not inductive—it is constructive in a manner discussed heretofore, and the verification of the postulated significance occurs by the procedures outlined in Chap. 7. It is well, however, to mention here another interesting distinction between the method of science and that of inductive inference. The latter has a truth value commensurate with our confidence in the data that lead to it;

its validity comes wholly from the circumstances in which the induction originates. A scientific construction, however, has an important sense of being true aside from its factual genesis. Its universal claim is wholly unsupportable on inductive grounds. It can be *disconfirmed* by a single contravening fact, which is not the case for any proper inductive generalization. Its confirmation, on the other hand, is usually conceded on the basis of a limited number of confirming instances. This, too, is out of line with inductive practice. What happens here is evidently this: Because the deductive method is willing to admit the falsity of a tentative theory with the utmost readiness, we accept its truth, and this means its whole truth, on limited evidence. We never quarrel about its having a truth value of 50 or 90 per cent. When need for that decision arises, we reject the theory in favor of another.

13.2. Probability Is a Measurable Physical Quantity

Let us return to the main course of our discussion by way of summary. Science is a deductive discipline. It is inadequately described by the logic of inductive inference. When it uses probabilities, as it often does, science regards them, not as degrees of confirmation of empirical propositions, but as measurable (and calculable) physical quantities like lengths, energies, and wavelengths.

The probability of an error is determined by *measuring* the width of a certain distribution curve; the probability that a molecule has a given speed is found by *measuring* the density of molecules on a rotating disk (cf. page 255); the probability for different energies of an atom at a certain temperature is obtained by *measuring* the distribution of spectral intensities which it emits. Life expectancy is found by carefully counting cases. None of these instances is even faintly suggestive of a "degree of confirmation of a proposition." It is in the sense of these examples, however, that probabilities will here be understood.

Perhaps it is well to take brief cognizance of the kind of questions which, because of our restriction, we shall not be able to answer. The probability of a truly single event is intrinsically unmeasurable and will therefore elude us; thus science has nothing to say about the *probability* of an act of cosmic creation, though it may assert or deny its having occurred. Nor shall we be able to assign a probability to a theory or a hypothesis, these being more appropriately classified as valid, approximate, or false.[1] Finally, we must profess indifference to many of the common uses of the word *probable*. When a person, asked to do us a favor, answers, "I will probably do it," we are compelled to translate his words into: I will do it if certain conditions are met. It is easy to think of many other instances in which probability stands for subjective degree of belief or a vague sort of likelihood. All these are to be excluded from consideration here.

The English language in part sanctions this distinction. For it derives the word *probable* from the same root as provable, while *likely* has the origin of seeming. It is interesting, perhaps, that the French language also honors the distinction (*probable* vs. *vraisemblable*), whereas the German language does not (*wahrscheinlich* is a most undiscriminating term). In the absence of evidence one might be tempted to speculate as to the likelihood that German writers, with their linguistic propensity to ignore the distinction, have helped to eradicate it in other languages.[2]

It is all very well to call probability a measurable quantity and to justify this attribute by a few technical examples, as we have done above. But precisely how is probability measured? And if it can be measured, will its measurement be free from internal difficulties? The first of these two questions will be taken up in the next sections. It presents no problem, for science has already solved it, and we need merely look at the procedures by which probabilities are actually determined. But the second question seems embarrassing at first glance and may forthwith create sufficient doubt to disturb our attention to the first. For as we know from Chap. 6, the very measurement of *any* physical quantity involves probability considerations, and we appear to be approaching a vicious circle when we endeavor to make

[1] Validity, in spite of its flavor of all or none, is nevertheless a temporary attribute.

[2] This conjecture may be difficult to defend since the recent advocates of a general "likelihood" interpretation of probability are mainly British (Keynes, Jeffreys).

probability itself into something measurable. Such misgivings, however, can be removed at once.

To obtain the value of a physical quantity, one must measure it a number of times. Each measurement contains an error, and the "true" value is (usually) computed as the arithmetic mean of all measured values. This is as far as we need to trace the process of measurement, and it is well to observe that probabilities have not yet entered. They are needed later, when the distribution of errors is to be justified in a rational manner.

In determining probabilities through measurements we proceed in a similar way and measure the relative frequency (see below) of an event in a series of trials. Each relative frequency contains an error, and the "true" probability is computed as the mean of the relative frequencies over a number of series. At this stage of measurement there is no concern with the distribution of frequencies about the mean, nor is there any need for an appeal to higher-order probabilities, probabilities of probabilities, and so forth, to rationalize such undertakings. One merely hopes for agreement with some constructive theory designed to predict the measured frequencies.

The fact that measured probabilities (relative frequencies) contain errors is symptomatic of their kinship with physical quantities of the more orthodox kind. Indeed the way in which that error is indicated betrays the scientist's interpretation. Good practice requires the probability of an event to be written as 0.735 ± 0.002, let us say. It uses the same form for the speed of light, $299{,}776 \pm 4$ km/sec. When this is recognized, nothing strange or inconsistent is left in the idea of probability as a measurable physical quantity.

13.3. Two Modes of Defining Probability

The road ahead is now clear. Since every physical quantity, if it is to be effective in the business of science, must be definable in at least two ways, one constitutive and one epistemic, it is proper that probability should be subject to this rule. There are, in fact, two classical definitions which accurately perform the two required tasks. One is due to Laplace and is often called the a priori form of probability; the other was first developed by Ellis, Cournot, and others; it goes under the names of a posteriori and, more adequately, of frequency theory. But, instead of being pleased at Providence for equipping probability with both certificates it needs to enter science, the modern logician sometimes quarrels over which of the two is "right." Not seeing their connection through science, he mistakenly believes the two definitions to contradict each other.

Laplace's definition, which will be considered in greater detail below, takes the probability of an event to be the number of favorable cases divided by the total number of "equipossible" cases. To use a familiar example, the probability of throwing two eyes with two dice is $1/36$, because there is one case favorable to the event $(1 + 1)$ and there are $6 \times 6 = 36$ possibilities of combining the numbers on the two dice. The probability of throwing a 5 under the same circumstances is $1/9$ according to Laplace's rule, since there are four ways of forming a 5 $(1 + 4, 2 + 3, 3 + 2, 4 + 1)$ and 36 possibilities all together. In a similar way it can be shown that the probability of finding all four aces in one hand at the start of a bridge game is $1/64$. The method for determining all these values is "a priori" in so far as it allows them to be found prior to measurement—not of course prior to empirical knowledge about dice and cards.

The frequency definition pays no attention to equal possibilities. It takes probability to be the ratio of the actual number of times the event occurs in a series of tests to the total number of events. Thus the probability of throwing 2 with two dice is $1/36$, or close to it, because in a long series of throws with two dice, let us say in 363 throws, there have occurred ten 2's. The number $1/36$ is obtained by actually performing a sequence of observations; hence the term *a posteriori.*

Now Laplace's approach has all the earmarks of a constitutive definition. Tacitly, it commits itself to a certain theory, a theory which happens to work surprisingly well in games of chance. In accepting it we accept that theory. Like most other constitutive definitions, it has a limited range of application and becomes useless in many practical instances. A life-insurance company does not compute mortality tables by counting the various ways of dying against the ways of surviving. Finally, like all other constitutive definitions of physical quantities, Laplace's formula is *exact* and involves no provision for assigning errors. It is wholly analogous to the definition of speed as the ratio of distance covered to time consumed.

On the other hand, the frequency formulation is quite obviously a rule of correspondence and hence an epistemic definition of probability. Its extensive application results from the circumstance that we find in our experience numerous situations, so-called "statistical" sequences of events, which seem to be characterized in no other way than that the relative frequency of occurrence of some type of event, like the appearance of a 2 in throwing dice, happens to be nearly constant. Why not proceed, therefore, as in all similar instances of invariable occurrence? Why not give the constant quantity a name and standardize by operational directives how it shall be measured? This is what the frequency definition achieves.

Each formulation is barren by itself. To get something interesting out of the stagnant logical state of affairs we must activate probability as a physical quantity, which means that we must couple the two definitions and watch their interplay. Tentatively accepting Laplace's rule and taking 1/36 for the theoretical prediction as to the probability of the 2, we utilize the rule of correspondence supplied by the frequentist and *verify* the theory of Laplace. If the relative frequency turns out to be 1/36, we have shown the equivalence of the two definitions and demonstrated the correctness of the theory formulated by their conjunction. The equivalence is a matter for empirical decision; hence all attempts to reduce one to the other by logical or other means is foredoomed to failure.

Though the Laplacian and the frequency formulation of probability have been very much in the limelight, they are not the only representatives of our two modes of definition. Constitutive definitions are in fact as numerous as theories involving probabilities. In the next chapter several of them, in particular one due to Gibbs, will be more carefully studied. The quantum theory contains a very interesting example which relates probabilities with state functions of physical systems. All these are satisfactory representations of the constitutive mode, certainly as satisfactory as Laplace's formula. If they are usually ignored or considered subordinate, this can be only because philosophers have had their interest focused more strongly on games of chance than on modern physics.

One rather simple and fairly well-known constitutive example will be selected for comment. According to Maxwell, the probability that a molecule of a gas shall have a speed v (per unit range of speeds) is equal to

$$\text{Const} \times e^{-v^2/kT} v^2$$

The quantity T here is the temperature of the gas, and k is Boltzmann's constant. Perhaps this is not so simple as Laplace's prescription, nor is it applicable in the same fields. But where it *is* applicable, it achieves the same thing, that is, to relate the probability to other physical quantities. In Laplace's case the other physical quantities might be the number of faces and the symmetry of a die; here they are the temperature and Boltzmann's constant.

Epistemic definitions are not so numerous, and the frequency definition has a rank that is more nearly unique. But it does not reign in solitary splendor. In the first place, the careful student of the subject can discern variations in the meaning of relative frequencies sufficient to amount in fact to a variety of definitions. There is the subtle distinction between relative frequencies and their limits as the number of observations increases. But these things will be touched on later. Here it is well to exhibit an epistemic rule of quite a different sort, which often serves in lieu of a frequency definition.

To verify Maxwell's law of the distribution of velocities, which was mentioned above, the physicist does not determine the speed of a molecule in a series of tests—this would be an exceedingly laborious task. Rather he sends a stream of molecules through an open sector in a rotating disk and determines the number that go through a similar displaced sector in another rotating disk, some distance from the first but revolving on the same axis. The probability in question is taken to be proportional to the density of molecules caught on a plate. All this, when fully examined, amounts to nothing other than a new, ingenious, and elaborate epistemic definition of probability which is applicable to a certain domain of experience.

Of course, there is a relation between Laplace's rule and Maxwell's formula for the distribution of velocities in a gas, and there is a relation between the method of velocity selection just discussed and other operational definitions. Whatever refers to or is embedded in the field of constructs must be related because of the requirements of Chap. 5. Nevertheless, the procedures in question are to be counted as distinct formulas for the ascertainment of probabilities, and hence as different definitions. The one is not merely another version of the others.

Finally, we note an epistemic definition which, though rarely formulated, enjoys perhaps the widest use in practice. The actuarial practitioner is likely to think of probability as an index such that when it is correctly assigned to an event against which he insures his clients, he will neither lose nor make money.

13.4. Laplace's Rule and Its Shortcomings

Since Laplace's formula is sometimes taken to be *the* proper definition of all probabilities, it seems well to sketch its range of application and, what is more important, its limitations. An air of obviousness surrounds the simple process of counting cases, and it is easy to fall prey to the belief that this represents the whole content of the probability idea. The temptation is strengthened by the fact that there is always something to count, from the faces of a die to the number of alternative propositions. It seems as if the formula works everywhere, in games of chance, in physics, and in logic.

On looking more closely, however, one sees a need for qualifications. It arises from the word *equipossible*, which appears in Laplace's rule. When this term is well defined—and only under that condition—the rule can be applied. Whether it works successfully on being applied is still another question.

Clearly defined equipossibles are encountered predominantly in three kinds of experience. There is first the large field of games, where attention centers upon unambiguous alternatives. On throwing a die no doubt is left as to the number cast, and no reasonable person would argue that, for a symmetrical, unloaded die, the 2- and the 4-face are anything but equally possible—whatever that term may mean. The possibility of a die resting on a corner, though present, is always disregarded as negligible. But as games become more complicated, the meaning of equipossible grows less certain, as will be seen in a later discussion.

The subject of heredity is another which invites the application of Laplace's rule, and for the same reason. The observable properties involved are easily discerned and usually offer no cause for differentiation on the basis of unequal possibilities. Whether a pea is round or wrinkled, its flower axial or terminal, its stem tall or dwarfed can be established with fair certainty, and one of these alternatives carries, intuitively at least, no greater likelihood than another. The presence of dominant and recessive characters obscures this simplicity, but when it is recognized, it makes the appropriateness of counting traits all the more convincing. In the early science of genetics, then, Laplace's rule is a most natural theory. Its astounding success upon formulation of Mendel's laws did much to establish it in the minds of many biologists as the supreme or the only meaning of probability. Modern observations, however, together with the chromosome theory of genetics, have made the significance of the rule more doubtful by adding numerous qualifications as to the equipossibles.

Finally, science uses the rule to advantage wherever the events are *numbers*. This fact accounts for the prevalence of the Laplacian attitude in physics and chemistry, for situations in which the outcome of a number is expected are very frequent. Typical examples are the number of balls of a specified kind to be drawn from a box in a given number of drawings (which is the prototype of so many problems in science), the number of successes in n independent trials of any operation, the number of alpha particles emitted by a source per second, fluctuations in the number of gas molecules in a given volume. Here again, the plausible feeling that one number is as good as any other, intrinsically, recommends Laplace's rule.

But thus far we have been very hasty in our summary, and far too partial to Laplace. Although it may superficially seem otherwise, the meaning of equipossibles is in fact *never* clear and never regulated by the rule in question. Its fixation always amounts to a special postulate which converts what appears as a mere definition of probability into a theory about nature. Here appears one of the trade-marks of all constitutive definitions, characteristically attached to Laplace's rule and unambiguously identifying it. We now give a few examples illustrating these points.

It has been said that, in the case of a die, there can be no doubt about equipossibles. But this is true only with respect to an unloaded die, and if we were to look further, it could be seen to be true for *physical* reasons. The same initial obviousness marks the tossing of a coin: doubtless there are only two possible events, heads and tails, and there is nothing that would assign to one of these more weight than to the other. Some logicians, indeed Laplace himself, were vaguely ill at ease concerning the source of this knowledge about equal weights and tried to legislate ignorance into information by adopting a "principle of indifference" which is taken to certify equality of weights when there is no knowledge to the contrary. With this in mind, we consider tossing two coins instead of one. There are now three events, the conjunction of two heads, of two tails, and of head and tail. Here Laplace's rule might misguide the unwary to accept the value 1/3 for the probability of tossing two heads. Of course the thoughtful reader knows this to be wrong; the value is 1/4. And this leads the Laplacian to the afterthought that of course there are four equally possible cases: two heads, head and tail, tail and head, and two tails.

Now it is wrong to assume that this is any more obvious than the former way of counting. The fact is that we *must* count in this way to attain agreement with observation. Many paradoxes in the theory of probability have their origin in a failure to inspect carefully the assumptions concerning equipossibles, a failure which comes from the erroneous belief that Laplace's rule is equipped with facilities for establishing equipossible cases. The more complex the problem, the more serious becomes this defect. In the foregoing section we asserted that the probability of finding all aces in the hands of one bridge player, after thorough shuffling and fair dealing, of course, was 1/64. To explain how it is computed we should have to state that every possible distribution of the four aces among the four players was considered as equally probable. Experience bears this out. It does not substantiate an hypothesis which, on the face of it, seems just as plausible, namely, the theory that every possible number of aces in a given hand is equally probable. This assumption would give a probability of 1/4 for the event in question, which is manifestly wrong. A principle of indifference is of no help under such circumstances.

When taken literally this principle, coupled with Laplace's rule, leads to grotesque predictions, which, being well known, need only be mentioned here. My prospects for surviving this day are 1/2; for there are only two alternatives, survival and death, and, since I know nothing, the principle of indifference leads me to this expectation. In the same way, every person unknown to me is under a 50 per cent suspicion of being a liar or of being a murderer.—It should be admitted that this treatment is unfair to the Laplacian, who will rightly reject our protestation of ignorance in these examples. For we do know from experience that survival and death in one day are not equally likely. But the point here made concerns the inability of Laplace's rule to provide such knowledge.

In sum, then, the a priori formulation of probability is very common and very useful in the deductive sciences. It is always coupled, however, with an extraneous stipulation regarding events which are taken as equally probable.

13.5. The Frequency Definition and Its Shortcomings

The frequency doctrine and its terminology have penetrated physical science to an extent which makes it desirable for us to consider a few of its details. This will also equip us better for an appreciation of matters that will concern us in the next two chapters. As already indicated, the frequency theory deems itself in possession of, or postulates, a long series of observations: tests, actions, events, and the like. Thus it assumes to have at hand a large number of throws of a die, drawings of balls from a box, physical observations, tests of the quality of a manufactured product, etc. We shall follow the terminology of von Mises, the most successful modern exponent of the frequency view, in calling the long series a "Kollektiv," a term synonymous with the "probability aggregate" occasionally found in the English literature. In a sense, the latter term is preferable since it adverts to something more specific than an ordinary aggregate, an ordinary collection of tests. A Kollektiv is in fact a very special collection of events, namely, one that is sufficiently random to justify our considering it from a probability point of view. For example, the decimal expansion of $1/7$ is an aggregate of the integers from 0 to 9, but not a probability aggregate or Kollektiv, whereas the decimal expansion of π does form a Kollektiv. This distinction is merely to be noted here; we shall have occasion for further brief comment on it below.

A single event, like the throw of a die or the drawing of a card, will be called an *element* of the Kollektiv, and the elements shall be regarded as bearing numbers corresponding to the order in which they occur in their Kollektiv. By deliberately rearranging the elements in a different order one can destroy the random character of the aggregate. Each element is characterized by one of a set of mutually exclusive *properties*, like being a 6, or being an ace, or lying within a specified numerical range. The number of elements comprising a Kollektiv is always very large; the number of properties may be small (two in tossing a coin) or denumerably infinite (as in guessing the number of failures before a winning throw; cf. the St. Petersburg paradox discussed, for example, in von Mises [1]) or continuously infinite (as in measuring the length of an object). For simplicity we confine our remarks to cases in which the number of properties is finite.

Every property occurs with a definite *relative frequency* in a given Kollektiv. By relative frequency is meant the ratio of the number of elements possessing the property to the total number of elements. But one need not define this term with respect to the whole probability aggregate; one may let it refer to the first n elements only. The relative frequency of the ith property, considered for the first n elements of a Kollektiv, may be written in an obvious notation as $W_i = n_i/n$. Now to the mathematician it is very interesting to study the behavior of W_i, regarded as a

[1] *Op. cit.*

function of n. In particular he would like to know if its trend is similar to that of certain well-known sequences which define mathematical functions.

To clarify these matters we have plotted in Fig. 13.1*a* the actual numbers W_i observed in a sequence of coin tosses as n

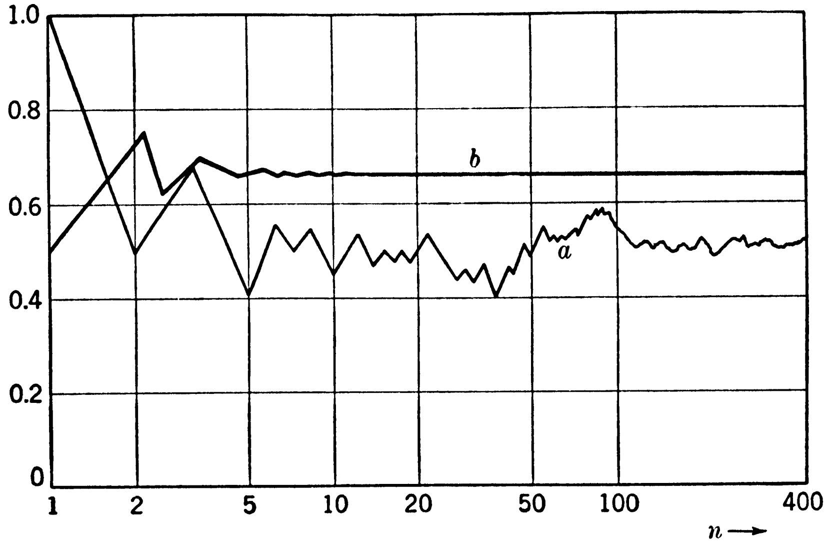

Figure 13.1

increases. Here i stands for the property, head. For comparison, Fig. 13.1*b* shows the trend of the series $\sum_{n=0}^{\infty}\left(-\frac{1}{2}\right)^n$, whose limit is 2/3. Figure 13.1*a* is taken from Cramér.[1] The second sequence approaches the value 2/3 in a very regular and certain way, while W_i seeks its limit more erratically. There is no doubt about the fact that W_i, as n grows larger, has more and more of its values distributed in the neighborhood of 1/2, but there is always the possibility of an arbitrarily large deviation from 1/2 for some stray values of n. This is a feature of all probability aggregates, and one which gives rise to certain difficulties in stating precisely what is meant by the limit of W_i as n approaches

[1] H. Cramér, "Mathematical Methods of Statistics," Princeton University Press, Princeton, N.J., 1946.

infinity. In spite of this, von Mises and others have defined the *probability* of the ith property as $\lim_{n \to \infty} \frac{n_i}{n}$; or $w_i = \lim_{n \to \infty} W_i$. If this is done, the word *limit* cannot be understood in its usual mathematical sense but must be modified in accordance with the more modern theory of point sets. To put things simply, the *orthodox* limit approached by our series is one which brooks no finite departures from the limiting value if n is large enough; the *statistical* limit tolerates finite departures, but the numbers n at which they occur "form a set of zero measure," *i.e.*, are negligible in comparison with those for which the limit is attained.[1]

In practice, Kollektivs do not possess an infinity of elements, and the working scientist is always forced to substitute relative frequencies for probabilities. To him the distinction between frequencies and their limits is a rather academic one; he acknowledges it, however, in a most practical way by refusing to apply the probability calculus when the number of elements is too small (when it does not represent a "fair sample" of a larger aggregate).

It is hardly necessary to emphasize the very great difference in meaning between the Laplacian ratio and W_i. In the example of a die, the former is the fractional number of faces, the latter the fractional number of events of a specified kind when the die is thrown. Probability theory gives no reason why one ratio should equal the other. That they do is just as remarkable, and no more mysterious, than that the formula for a falling body fits the observed facts. Note also that Laplacian probability can never be tested *as a probability*. All we are able to verify empirically is that the die has indeed six faces, one of which carries a 5, for instance. This itself is not without fruitful implications of a formal sort, for it allows one to conclude among other things the presence of twelve faces and two 5's on two dice. But never do we get from there to the other side of the fence, where events (throws, in this instance) actually happen.

The frequency concept has the opposite kind of limitation: it is rationally barren. Being an operational approach it enables us to measure but not to predict probabilities, except through

[1] One philosophically interesting consequence of this modified limit idea is that a probability of value 1 no longer means certainty.

an inductive generalization toward other similar instances. Thus, knowing the relative frequency of casting a 5 to be 1/6, I can forecast expectations regarding other throws with this die but shall not be able to say that the probability of throwing two 5's with two dice is 1/36. To be able to do this I should have to jump over the fence and back again.

This is not always realized even in so elaborate a science as physics. In many problems of the kinetic theory the physicist is interested in the probability of finding a specified kind of molecule in a certain region of space. Empirically, he means the number n_i of observations on the space in which such molecules are found to be present, divided by the total number of observations. His Kollektiv consists of a temporal sequence of observations. When reasoning about the matter he is likely to turn Laplacian and to compute his probability as the ratio of the number of specified *molecules* to the total number of *molecules* present. The utter disparity of these two concepts and the need for joining them by rules of correspondence often escape notice.

The frequency definition of probability has certain shortcomings of its own, some of which have already been mentioned. These imperfections do not spoil its meaning or interfere with its usefulness any more than the nonexistence of a perfect balance spoils the epistemic meaning of weight. One drawback is often seen in the need for introducing a new idea of a mathematical limit. Needless to say, the true mathematician regards this as a fortunate circumstance responsible for significant progress in his discipline. Another disadvantage—and this does in our opinion constitute a serious blemish—lies in the difficulty of saying when an aggregate is sufficiently random to pass as a Kollektiv. The early attempt of von Mises to define randomness seems to have failed. Others are likely to fail for a very simple reason: Any aggregate which is consciously formed, *e.g.*, a series of numbers written down by a working person, cannot be random. To obtain a Kollektiv one must blindly trust to nature, toss a penny, draw cards, or rely on the thermal noise of an electrical amplifier system. In a very peculiar sense, a probability aggregate is "nonconstructible." This is embarrassing to a definition of probability which claims to be operational, for there is apparently no human operation which serves to ensure randomness in nature's presumed Kollektivs.—In practice, however, there are acceptable methods for making reasonably sure of this point.[1]

13.6. The Synthetic View

Throughout the present chapter we have hinted strongly at the compatibility of the frequency theory and the Laplacian theory despite their basic differences; hence the present announcement of their marriage will take no one by surprise. The frequency theory supplies an epistemic definition of probability, the Laplacian formula a constitutive one. Their interplay is evident in every application we have considered.

In games of chance there must be and there always is the conative affirmation that combinatorial laws (Laplace) and frequency observations shall specify the *same* probability. The temptation to overlook this point arises from the empirical circumstance that the two definitions do in fact so often agree.

Heredity involves a comparable juxtaposition of the two ideas, perhaps in a more patent way. For in speaking of *laws* of heredity genetics signalizes the empirical element which lies at the union of two differently conceived probabilities: one defined in terms of combinations of characters (or gametes), the other in terms of frequencies of occurrence of observable traits.

In the third large class of applications of Laplace's rule, namely, to number situations, the same is true. When a box contains two balls, one black and one white, the probability of drawing the black ball is "obviously" 1/2. But this is not a good example because its simplicity hides all significant detail. Suppose, therefore, that the box contains one black and three white balls and I want to know the probability that, when one ball is drawn in each of six successive drawings (the ball being replaced each time and the box well shaken), two of the drawn balls shall be black. What we mean on the empirical side is this: I perform a set of

[1] It might well be added here that the mathematics of probability are often at variance with psychological reactions to the occurrence of rare events. An interesting light is shed on psychological aspects of probability in an entertaining paper by Warren Weaver [*Scientific Monthly*, 67:390 (1948)], where a "surprise index" is defined in terms of relative frequencies.

six drawings, say 100 times; then I count the number of times two out of the six balls were black and divide it by 100. Thus is obtained the measured value of the probability in question, and there is no other way: the frequency definition of probability is in the present instance (and in most others) the only epistemic definition.

And how is this probability computed? Following Bernoulli (who followed Laplace) we count the number of ways in which one ball can be drawn from four boxes, then we combine similar boxes, and so forth, coming out with the numerical value 1,215/4,096. The manner of computation has nothing to do with the measurement. *We formulate a physical theory* when we assign this value as the expected value of the frequency ratio.

From the frequency definition alone one does not derive the laws of the probability calculus. Frequencies by themselves can never be construed to imply the two basic theorems of the calculus: (1) The probability for the simultaneous occurrence of two independent events is the product of their probabilities; and (2) the probability that either one or the other shall occur is their sum. It was the historic mission of Laplace's theory to establish logically, to prove, these laws and many others derivable from them. If constitutive definitions were absent from this field, the probability theorist would be in the position of the geometer who is equipped with rulers and compasses but is unable to use the theorems of geometry.

We have already mentioned the availability of constitutive theories other than Laplace's. To be satisfactory, such theories must permit the derivation of the two basic laws above or must adjoin themselves to theories that do. One of them has been very much in the ascendancy of late and has reached a state of perfection far higher than the combinatorial scheme of Laplace. It is the probability notion based on Borel sets. A good exposition of it can be found in Cramér's book [1] already cited, a book in which a synthetic view like the present one is clearly expressed.

The technical aspects of set theory need not arrest us here. It is sufficient to say that this abstract science has developed certain quantities called set functions which behave in much the same

[1] Cramér, *op. cit.*

way as Laplacian probabilities. Moreover, they do not break down or misfire in combinatorial fashion when the range of properties becomes continuous, and hence they are on the whole more helpful. Cramér looks upon them as the mathematical counterparts of relative frequencies, just as he takes the whole of probability theory to be a "mathematical model" of certain common experiences. If the frequentist, *i.e.*, the person who rejects all but the frequency formulation of probability, were to carry his methods over into geometry, he would have to define a point as the limit of a chalk spot on a blackboard, a line as the limit of a chalk streak, and he would be obliged to demonstrate Euclid's theorems experimentally, Cramér notes with emphasis. The need of supplementation of such epistemic notions by constitutive ones is evident throughout his book.

In the next chapter we shall deal with probability theories which are also of the constitutive variety but are more limited in their ranges of application. They are never in conflict with Laplace's theory of combinations or with the theory of sets; but usually they supply more detailed rules for calculating "elementary" or basic probabilities. After that, they incorporate themselves without conflict in one of these standard forms of scientific theory, and their specificity is often forgotten.

Summary

Upon discussing the meaning of probability in science and in the newer discipline of inductive logic, it is concluded that they are not identical and that a careful distinction must be maintained between scientific probability and the idea "degree of confirmation" of a logical proposition, which seems to be the basic concept of inductive logic. In this book attention is restricted to the former.

As a result of this restriction, probability becomes a *measurable quantity*, quite on a par with others such as position, energy, valency, and atomic number. Like all physical quantities, probability partakes of the dual mode of definition set forth in Chap. 12. It is shown that the two principal classical approaches to the probability problem, Laplace's theory and the frequency theory,

are complementary and are both necessary if probability reasoning is to be scientific. The first provides a constitutive, the second an epistemic definition, and every application of probabilities to science must involve aspects of both.

Laplace's definition is not the only constitutive one, nor does the frequency theory provide the only epistemic definition of probability. In fact many elaborate physical formalisms, *e.g.*, Maxwell's theory of gases and Gibbs' statistical mechanics, achieve for certain physical situations precisely what Laplace's formula was designed to accomplish for games of chance. Other epistemic definitions that rank with the frequency theory in breadth of application are difficult to find, but some are available.

Each definition, when considered in isolation, has serious shortcomings which are discussed in Secs. 13.4 and 13.5. But when the definitions are allowed to interplay as they do when in scientific use, the defects are seen to annul each other.

In the last section attention is drawn to one of the newer forms of constitutive theory which is rapidly replacing Laplace's, a theory based on the properties of Borel sets.

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CHAPTER 14

Statistical Mechanics

14.1. The Problem of Explanation in Statistical Mechanics

As we have seen in Chap. 11, the systems of thermodynamics are ordinary objects: solids, liquids and gases with definite boundaries. The observables of interest are somewhat more remote from direct perception than the visual properties on which mechanics concentrates attention. Temperature, pressure, and entropy lack the intuitive immediacy of positions and velocities. They are bound to Nature by more extended and more complex correspondences and lead to concepts that are more abstract. All this makes the problem of explanation in thermodynamics rather unique and interesting and gives it some features which form a bridge with quantum mechanics.

In thermodynamics proper, observables are connected by what are sometimes called *empirical* relations, with the term empirical understood in a very limited and specific way. The "laws of motion" in this science are primitive equations combining the observables themselves. In a certain sense, the laws do not say "why" bodies behave thermodynamically as they do. While this connotes no defect of the methods of thermodynamics as a science, it nevertheless raises a question as to the possibility of other modes of explanation, of theories that "go behind" the phenomenologic structure of thermodynamics and its minimal assumptions. The question arises primarily in view of the postulate of extensibility (Chap. 5), which predisposes science to seek ways whereby the explanatory repertoire of one theory can be made to serve others as well. Now, clearly, the bodies of thermodynamics are also systems of mechanics, and it is indicated that one should inquire whether the laws of mechanics can produce, or at least simulate, the equations of thermodynamics.

But, as in all transferences of a theory into a domain other than its native own, one meets here with an initial obstacle. Though the systems are the same, the observables of thermodynamics are quite different from those of mechanics. By no stretch of the imagination can a mass point or a rigid body be said to have a temperature or an entropy or to exert a pressure. A merger of the two theories therefore requires first of all a reinterpretation of the observables of one in terms of the observables of the other. In effect this lengthens appreciably the chains of correspondence which supply the epistemic definitions for the quantities in question. This is seen very clearly in the example of temperature, where the usual operational definition, which refers to thermometers, must be augmented by some analogue on the statistical side (*e.g.*, kinetic energy of molecules) to remain meaningful. Or, to look at the matter in another way, this procedure provides new constitutive definitions for thermodynamic quantities. For the lengthening of the chain takes place entirely in the space of constructs, as already noted in the example of temperature. The same formal extension takes place when we associate pressure (for which an operational definition in terms of manometers is already at hand) with the mean force of molecular impacts per unit of area. It is simpler, perhaps, if we think of these additions to the rules of correspondence as new constitutive definitions. In this way, then, we find the good old thermometer reading now converting itself into a "modulus of distribution in phase" (Gibbs) or into the mean kinetic energy of the molecules composing the system.

The mechanical reinterpretation of thermodynamic observables and with it the reduction of all thermodynamic equations are performed in statistical mechanics. Neither reinterpretation of observables nor reduction of equations is possible by means of the proper tools of mechanics alone; additional constructs, not germane to mechanics and thermodynamics, are needed to attain these ends. The addenda are of a statistical sort and involve the notion of probability in an important way. Statistical mechanics is therefore a discipline in its own right, related to mechanics but operating with certain extra notions peculiar to itself. Just why and where probability enters is a question we now wish to consider.

For definiteness, we select one thermodynamic quantity, the pressure, and examine what is involved in its reinterpretation. The system may be taken to be a gas enclosed in a vessel. As a thermodynamic observable the pressure of the gas is the force exerted by the gas upon unit area of its walls. On applying the concepts of mechanics without supplementation—assuming of course what is always taken for granted, namely, that the gas consists of many molecules—this pressure becomes the amount of momentum lost per unit of time by the molecules striking a unit area. So far nothing new has been introduced, and if this simple transition completed the reinterpretation, statistical mechanics would be part of ordinary mechanics.

A difficulty appears, however, when it is noted that the thermodynamic pressure is constant, whereas its mechanical counterpart as just construed varies erratically in time and indeed from point to point on the surface. It would be difficult to select any value as representative of the measured thermodynamic pressure. To put the matter in terms previously explained: According to one definition, the observable is *possessed* by the physical system; according to the other, it is *latent*. Now latent observables have nothing intrinsically objectionable about them, as we have already indicated and as will be further discussed; but they are accepted by science only under compulsion and as a last resort. In this instance experiment clearly shows the pressure to be a quantity of the relatively stable, possessed variety, but our present theory does not permit it to be so understood.

The task then is to convert the latent into a possessed observable, and this is accomplished by an appeal to statistics. To get rid of the rapid fluctuations in the rate at which molecules deliver momentum to the walls of the vessel one must resort to some averaging process. Perhaps one should take the mean value of the dynamical quantity, momentum per second, over a sufficient period of time at the point where the pressure is to be measured; perhaps one should take the average of this quantity at any one given time over the entire wall of the container; perhaps one should follow the motion of a selected molecule in its course back and forth across the container, take due note of its collisions with other molecules on the way, compute the average momentum delivered to the walls in a single impact, and multiply by the number of impacts per second. All these opportunities are available. But the principles of mechanics are quite indifferent as to which of the choices is best.

What is required is the introduction of some Kollektiv, the assignment of probabilities to its properties and the subsequent calculation of the mean value of our dynamical quantity. The events composing the Kollektiv may be successive impacts of one given molecule or of some specified group of molecules; or they may be the simultaneous collisions of many molecules with the wall at some given time. Mechanics does not dictate what the probability aggregate shall be, and science may choose one which works, which leads to agreement with sensory thermodynamic experience.

Now it happens that several of them work! Such generosity of fate is rare in most realms of physical science, but in statistical dynamics, it seems, we encounter an embarrassing profusion of successful alternative constructs of explanation. Hence there are several brands of statistical mechanics, two of which will be set forth in the next two sections. What problems this ambiguity raises for "reality," which is as yet undefined, will be examined in the following chapter.

But prior to our detailed exposition we insert a word of warning, and a reference far ahead. Statistical mechanics succeeds in converting latent into possessed observables by introducing suitable probability aggregates. No one should conclude, however, that this trick works everywhere else in physics. In Chap. 16 certain observables will turn out to be latent and *not* convertible in this fashion or in any other known way. Some scientists, looking at the multiple success of statistical mechanics, have felt that a probability aggregate *ought* to exist by which the latency of all observables can be removed. But the search for it in quantum mechanics has been in vain, and a complete understanding of that discipline has at times been prevented by a belief that "scattering observables" are always indicative of a missing link, to be found in hidden properties regulated by a statistical aggregate. The theories of statistical mechanics present no *universal* cure for the vagaries of latent observables.

Above, the passage from thermodynamics to statistical mechanics has been traced for one quantity, the pressure P. The account could be repeated in similar form for other variables, notably the temperature T, the internal energy U, and the entropy S. In each instance the need for reference to a probability aggregate can be made evident. But the relation between the thermodynamic quantity and its statistical counterpart is usually much less obvious than in the case of P which we are considering. An identification of "mean rate of momentum transfer" with pressure is suggested by the laws of mechanics even if the kind of mean is left unspecified. An identification of T with the mean kinetic energy of the molecules (expressed in proper units) is not so suggested, and yet it must be made to assure success in one form of statistics (Boltzmann's). Similarly remote is the connection between entropy and "the mean value of the logarithm of extension in phase" (Gibbs).[1] On the other hand, U is related very plausibly to its statistical partner, the total mechanical energy of the system. In view of these facts scientists have become accustomed to calling the constructs which relate, on the statistical side, to the thermodynamic quantities P, T, U, S, and so forth, their *statistical analogues*, acknowledging thus the genetic difference of the latter concepts. What this means in terms of the methodology developed in this book is clear: The thermodynamic formulations of P, T, U and S are the epistemic definitions of these quantities; the statistical analogues are their constitutive definitions (not the only ones, however).

One can now easily see the outline of the problem of explanation in statistical thermodynamics. It is to develop mathematical relations between the analogues which match those between the thermodynamic quantities themselves. When this is done, the job of our present discipline is finished. Statistical mechanics is not called upon to furnish mechanical reasons why, for instance, the entropy should appear as the negative mean value of the logarithm of an extension in phase. It need not "explain" these longish rules of correspondence. Nor, of course, could they be recognized by looking at Nature. *What appears to be a simple juxtaposi-*

[1] The philosophic reader need not worry about the exact meaning of these terms; no specific inferences will be drawn from them.

tion of sense impression and external object in the naïve act of recognizing a thing and therefore hardly calls for anything so elaborate as a rule of correspondence, turns into something highly complex, almost arbitrary, and in need of close examination in a science like statistical mechanics. This, we hope, will exempt us from the charge of having made the methodology of science needlessly elaborate by the introduction of correspondences. Other theories which cannot be understood without them are to be reviewed in due course.

14.2. The Theory of Gibbs [1]

A thermodynamic body, regarded as a mechanical system, has a *dynamic* state (cf. Chap. 9) which can be specified in terms of a certain number of coordinates and an equal number of momenta. If we care to think of the system as composed of M molecules, this number is $3M$, since each molecule's position is described by its x, y, and z component. One need not, however, commit oneself as to the composition of the body; it is possible to proceed more generally by assuming that it is a dynamic system with a certain number of degrees of freedom. This, it will be remembered, is the minimum number of coordinates requisite for the description of the mechanical configuration of the body and will be called n. For the special case of a gas consisting of M molecules, $n = 3M$. The complete *state* (not merely its configuration or generalized "position") is then representable by $2n$ numbers, n coordinates and n momenta.

Now $2n$ is an extremely large number, easily of the order 10^{24}. Nevertheless one is to think of all coordinates and momenta as known, tabulated perhaps in an enormous book with 10^{24} different entries. (If this book had 500 entries per page, it would be about 10 light-years thick.) As the state of the body changes, *e.g.*, as its molecules move, some or all of the entries must be changed.

Since this job of bookkeeping is impossible at any rate, one might as well form a more elegant conception of the changing state. To illustrate it, we shall lose sight for a moment of the

[1] The reader may omit this section and the following without destroying the continuity of our argument.

swarm of particles composing the gas and consider the simplest kind of mechanical system: a mass point, perhaps a single molecule, moving along the x-axis. It has but one degree of freedom, and its complete state is specifiable by means of two numbers, x and p. Instead of entering them in a book, assume them to be placed on a plane graph with x as abscissa and p as ordinate. The pair of numbers is then represented by a dot on a plane or, in more technical language, by a point in a space of two dimensions. As the molecule moves about, its representative point describes a curve in the plane.

The perspicuousness of this procedure is lost when it is extended to less simple systems. A molecule moving in a *plane*, which is a space of *two* dimensions, requires x, y, p_x, p_y, that is, *four* numbers for its complete description, and these correspond to a point in four dimensions. Undaunted by its failure to be visualizable, Gibbs retained the construct of many-dimensional spaces and expanded it to suit his needs. Many-dimensional spaces had, in fact, been used in dynamical theories long before.

A system with n degrees of freedom and hence with $2n$ variables of state is represented by a point in a space of $2n$ dimensions, and changes in the state of the system are indicated by motions of the representative point along a curve in the $2n$-dimensional space. Such fancied motion in hyperspaces is not only a pretty conception; it also furnishes a useful formalism for accurate thought and an aid in analysis, for although one may not properly grasp it by intuition, this space has very simple mathematical properties. It permits, for instance, an easy generalization of basic geometric theorems such as Pythagoras' and Gauss' theorem of the divergence. For the scientist, its use is not a resignation to the mysterious, since its properties are very clearly known. This many-dimensional space is called the *phase space* of the thermodynamic system.

The dynamic career of the system is thus represented by a curve in phase space, a curve whose properties are interesting indeed. One knows for example that it can never cross itself, and unless the curve is periodic, it ultimately pervades the entire volume of its space. All this and much more has been proved from the principles of dynamics. Since every dynamic observable (energy, total momentum, angular momentum, etc.) of the system is a function of the $2n$ variables composing the dimensions of phase space, a definite value of each observable is associated with every point of phase space. Hence the path of our moving representative point marks a specific succession of values for every observable. Seeing *where* in phase space the point is at this moment, we know *what* is the energy of the body, its momentum, and so forth. And we also know that these values fluctuate rapidly from instant to instant and do not tell us much about thermodynamic observables (*e.g.*, pressure).

At this juncture Gibbs introduces a strange Kollektiv for the purpose of taking mean values. He constructs an *ensemble* of systems; that is, he imagines a great number of thermodynamic systems, all similar to the given one. If that be a vessel filled with gas, he imagines millions of similar vessels all filled with the same quantity of the same gas. The dynamic states of these systems will in general not be identical. Each member of the ensemble will have its fate represented by a point moving in phase space, and the whole ensemble, when viewed in that space, will appear like a cloud of dust with each constituent dust particle pursuing its own path. The density of this cloud of dust will differ from place to place, and it may change in time at any given place. Yet, as may be shown, every particle obeys certain social constraints despite its nomadic habits: it never collides with any other particle of dust, and it visits every part of its world.

Among all possible states of motion of the cloud one is particularly interesting: that in which the density at every point of phase space remains constant in time. This, says Gibbs, corresponds to *equilibrium* of the thermodynamic system (or the ensemble, if you please). And since thermodynamic laws apply to equilibrium conditions only, attention may be limited to this state of motion. Although the terminology is a bit loose, we shall refer to the constant-density condition of the cloud as the *canonical distribution* of the representative points corresponding to the ensemble. In its canonical distribution the cloud is not motionless; the points move in a manner which leaves the density of all points unchanged. Our atmosphere is a concrete example of this state of motion in three dimensions provided that we disregard winds: the atmospheric molecules move and still cause the density at a given height to be constant.

The beauty of Gibbs' theory lies in this: If we average the *rate of momentum loss* per unit wall area over the entire steady dust cloud (our canonical distribution), we obtain the correct value for the *pressure* of the actual system; if we average the *energy* over the canonical distribution, we find the correct value for the *internal energy* of the actual system. And similar success is achieved for all other thermodynamic variables. Surprisingly enough, *the canonical distribution prescribes the probabilities* (densities of cloud at different places in phase space) *which the various possible values of an observable must have in order to yield the correct mean value.* With the aid of the canonical distribution, the latent observables can be turned into possessed, steady quantities.

In this fashion *statistical analogues* are produced for all thermodynamic variables. The temperature corresponds to a certain parameter appearing in the mathematical expression for the canonical distribution. The word *corresponds* is used here in the accurate sense of our former rules of correspondence, to wit: The construct temperature is constitutively defined, wholly without reference to measurements, by Gibbs' theory, where it appears as the quantity θ in the expression for the canonical distribution, $P = e^{\frac{\psi - H}{\theta}}$. The rule of correspondence, or the epistemic definition, says that θ is related to the readings of thermometers, and so forth. In a similar way, all other thermodynamic quantities have analogues which are mean values of properly chosen dynamic variables, and the averages are carried out with the aid of the canonical distribution and with the ensemble which it implies.[1]

It would not be surprising if, during the preceding development, which moved rapidly from one unusual notion to another, the reader had lost sight of the main methodological issues. They revolved about the task of finding a certain mean value for rapidly fluctuating observables. But to find a mean one must know the weights, that is, the probabilities, of individual events. Dy-

[1] For mathematical details, the reader is referred to J. W. Gibbs, "Collected Works," Vol. 2, Part 1, Elementary Principles in Statistical Mechanics. For a briefer exposition see Lindsay and Margenau, p. 218, John Wiley & Sons, Inc., New York, 1936.

namics did not tell us these. Hence it became necessary to postulate a probability aggregate in which the values of dynamic observables are the elements, and Gibbs' theory was that postulate. It said, in effect: The probability of a value r belonging to a dynamic observable is proportional to the density D of the canonical cloud at the place in phase space where the observable has the value r. Knowing D as a function of r is the only requisite for computing the mean. Gibbs' theory is a valid set of rules for obtaining D, that is, for finding the probabilities.

It is not customary to classify this form of statistical mechanics among theories of probability. Yet that is where it properly belongs. As a scientific device it should be put beside Laplace's rule. This rule shows how to construct the probability of throwing heads with a penny, a 5 with a die, and so forth. It amounts to a constitutive definition which experience substantiates in terms of frequencies. Gibbs' statistical mechanics and every other form of statistical mechanics are successful doctrines enabling the physicist to construct the probabilities needed to account for thermodynamics. Like Laplace's rule, Gibbs' theory amounts to a set of constitutive definitions which are tested and confirmed by measurements.

Certain questions can hardly be avoided when the contents of this section are surveyed. What right has the scientist to postulate so curious an ensemble as that of Gibbs, to invent innumerable replicas of the one given system? How can such an extravagance describe the real world? We dismiss these questions here but return to them in the following chapter.

14.3. Other Types of Statistical Mechanics

Statistical mechanics is one of the rare disciplines of physical science in which several rival theories stand side by side and remain companionable. The one just described is elegant but has a number of notable faults. It pays very little attention to the molecular structure of thermodynamic systems and thus avoids contact with facts known in other fields; it has a peculiar air of aloofness. Also, by introducing probabilities via ensembles, it achieves success but not illumination, for it withholds from consideration all details of what happens physically. And lastly, it has been found difficult to bring Gibbs' statistics into harmony with the theory of quanta. This, to be sure, can be done, but in the process of achieving harmony one discovers that ensemble, phase space, and canonical distribution lose their charm; the dust particles develop the annoying habit of jumping discontinuously from place to place, and they avoid certain regions of phase space altogether. Physicists and chemists have therefore been interested in alternative forms of statistics.

Boltzmann was among the early originators of one such method, often named after him, and recently developed to greater perfection by the two British physicists Darwin and Fowler. While there are some differences between the original and the final form, we shall describe essentially the latter and use on the whole the terminology of the last-named investigators. The theory in question recognizes from the beginning the molecular structure of matter and bases its entire procedure upon this recognition. It thus also manages to satisfy the demands of the quantum theory.

Rather than consider the number of *degrees of freedom* of the thermodynamic body, as was done in the preceding section, we now give thought to the molecules composing that body. Suppose we number them from 1 to N. The strain placed on our imagination by this suggestion is as great as that produced by the ensemble; but the facility of "seeing" the moving molecules makes the strain less unpleasant to bear. Each molecule can take on any value of an observable; its speed, for instance, can be 1.5 cm/sec, 1,000 cm/sec, etc.; its energy can be 1 erg, or 10^{-16} erg, or any other value. For definiteness, let us confine our interest to one specific observable, the energy.

Now it is not equally probable on mechanical grounds that a single molecule whose motion is entirely unspecified will possess an energy of 1 erg and of 10^{-16} erg, the former value being in fact far more probable, as one may prove by the principles of dynamics.[1] That is to say, there are more states of motion in which

[1] To be precise one should say: The probability of a molecule having an energy between E and $E + \Delta E$ is greater the greater E, provided that ΔE is constant. Note that this would not be true if extraneous conditions, such as constancy of the average energy, were imposed.

a single molecule enclosed in a container can have an energy of 1 erg than 10^{-16} erg; no important problems of statistics are involved at this stage. To avoid unnecessary complications we are going to consider the energy scale divided into small segments of different lengths, the lengths being so adjusted as to make it equally probable, mechanically, for a molecule to possess the energy of one segment as another. These segments, too, are labeled by numbers.

Statistical assumptions enter as we transfer attention from a single molecule to the assemblage of N molecules, the thermodynamic body. To see this, it is useful to distinguish two kinds of states with reference to the assemblage.

1. The first is called a *microscopic* state. It is a detailed assignment of individual molecules to energy segments, expressed by a statement of this form: molecules number 5, 691, 1959 fall into segment number 1 (*i.e.*, have energy within that range); molecules 11, 750 fall into segment 2, and so forth, until all molecules are assigned to their proper segments. When a microscopic state is known, the total energy of the assemblage can be computed by adding up the energies of the individual molecules.

2. A *macroscopic* state of the assemblage is defined in the following way: There are three molecules in segment 1, two molecules in segment 2, and so forth, until the *number* of occupants of each segment is specified. Which individual molecules reside in segment 1, which in segment 2, etc., is now unimportant. One needs much less information to describe a macroscopic state than a microscopic one. Nevertheless the total energy of the assemblage is fixed and computable when the macroscopic state is known, for it does not matter which particular molecules have a given energy so long as their number is given. The point to be noticed is that a single macroscopic state "contains," as it were, or is compatible with, a considerable number of microscopic ones.

If in our example molecule 5 were transferred from energy segment 1 to 2 while at the same time molecule 750 were transferred from 2 to 1, a new microscopic state would result, but the macroscopic state and hence the total energy of the system remain unchanged. In this way it is seen that the number of microscopic states contained in a macroscopic one is equal to the number of permutations of molecules between different energy segments.[1]

So far as the thermodynamic body is concerned, our interest is limited to macroscopic states. An observable property has the same value for all microscopic states (sometimes called configurations) which make up a macroscopic one. Hence in order to find the mean value of an observable it is sufficient if we calculate its average over all the latter states. Our problem, then, seems to be solved.

It is solved except for one detail. What are we to take for the weights, the probabilities of the different macroscopic states? This question did not arise in the theory of Gibbs;[2] it must here be settled by a special postulate which characterizes the present form of statistics. The postulate asserts that *the probability of a macroscopic state is proportional to the number of microscopic states which it contains.*

Again, the assumption seems reasonable but is not dictated by the laws of mechanics. Its virtue is apparent in its consequences, for it leads to agreement between the computed mean values and the measured quantities of thermodynamics. The statistical analogues are different in this theory from those of Gibbs, but they satisfy the correct thermodynamic relations.

It is hardly necessary to point out once more the primary function of this form of statistical mechanics as a probability theory. Like Laplace's rule, it shows how to construct the probabilities which reveal themselves as valid in physical observations.

14.4. Statistics and Ignorance

Statistical mechanics is a link between thermodynamics and mechanics; it succeeds *almost* in reducing one to the other. The reason for its partial failure is in the need it has for introducing probabilities, quantities unknown among the concepts of mechanics. True, dynamical principles tolerate the use of probabilities in

[1] The reader who is conversant with the algebra of combinations will recognize that this number is $N!/N_1!N_2!\ldots$, provided that N_i is the number of molecules in the ith segment.

[2] At least not in this form. Gibbs circumvented it by postulating that probabilities shall be proportional to the densities of his canonical cloud.

certain special problems, where the initial state of a mechanical system is not completely known. But even there they offer no help in generating probabilities; these must first be introduced by considerations of a nondynamical sort, for the competence of the laws of dynamics is limited to the *transformation* of one set of probabilities into another. An example will make this clear.

A new comet appears in the sky, and it is visible for a short time only. Measurements have not been sufficiently accurate to fix its position with normal certainty, but from a few scattered observations it is possible to assign probabilities to its various positions at the time of observation. Obviously, the question when the object will return to be seen on earth cannot be answered with precision. It is possible, however, to compute the *probability* that it will return at any given time if, and only if, the assignment of initial probabilities, which has nothing to do with the laws of motion, is taken for granted. In a similar way the *most probable* moment of return can be calculated. One sees how the laws of mechanics act with perfect precision and how they transform the probabilities corresponding to an aggregate of initial conditions into another set of probabilities relating to the desired information. The example sets forth a characteristic generally possessed by the principles of mechanics: probability is a foreign element in mechanics; it does not evolve naturally in the application of these principles and must be injected into them from without if it is to function in mechanical description. Hence the failure of statistical mechanics in effecting a complete reduction of thermodynamics to mechanics.

The central role played by probability in the subject under study calls for comment on two questions. First, what effect has the use of probabilities in statistical mechanics upon the certainty of thermodynamic laws? And second, does the need for introducing them arise from ignorance of physical conditions, as in the example of the comet, and will more complete knowledge permit their elimination?

1. The first question has already been touched upon in Chap. 11; we are now able to amplify and confirm the statements there made. Both types of statistics chosen here for presentation lead to the same conclusion; for this reason we may limit our remarks

to the Boltzmann form. Its basic assumption makes the probability of a macroscopic state proportional to the number of microscopic states composing it. Accordingly, a state in which the energy of the thermodynamic system is distributed with fair uniformity among all its molecules has a far greater probability than the state in which some molecules have all the energy and some have none. However, because the latter condition has a finite probability—though an exceedingly small one—we should expect it to be realized perhaps once in a very long time. That would be the event in which part of the water in a kettle freezes while the rest boils off.

Whether one takes this expectation seriously depends on whether he regards statistical mechanics as an *explanation* of thermodynamics, in the sense that it is logically prior, or not. For practical purposes the decision does not matter, and consistency is possible either way. Because the statistical point of view provides an extension of the molecular theory, its acceptance as logically prior to thermodynamics is favored by the methodological principles and is correspondingly predominant despite its price: encumberment of the laws of thermodynamics by probabilities. The answer to our first question is therefore clear. If statistical mechanics is taken to be logically anterior to thermodynamics, if it is not merely a useful device for producing laws which are formally similar to those of thermodynamics, then the latter do not operate with certainty.

Before attacking question 2 it is well to consider the consequences of this view with regard to one of the principles of thermodynamics, the famous second Law. When interpreted in accordance with the foregoing developments the second Law simply asserts the passage of the system to more and more probable molecular arrangements as time goes on, an unfolding of the fullest possibilities among microscopic states. This leads to the well-known results—equalization of temperature and pressure, the space-filling tendency of gases, diffusion—and it also raises the prospect of a *heat death* for the universe. This hypothetical future state will be realized according to the laws of thermodynamics when all molecules have reached their most probable state, and the temperature of all creation will be uniform; heat can then no longer flow, nor can anything but random transfer of matter take place. Complete stagnancy will envelop the world, and life must cease.

But statistical mechanics, while permitting this process to be easily conceived, also qualifies the prospect in important ways. The rare occurrence, in which part of the water freezes and the rest evaporates, was a reversal of the probable trend, and every such reversal counteracts the heat death, though it cannot prevent it. It is not out of the question, however, that after a very long time the condition of stagnancy, once realized, will terminate itself by spontaneous regeneration of motion, much in the manner in which the relative frequency in Fig. 13.1 after having practically reached its limit, kicks off for a finite departure. The idea of a universe which serially rejuvenates itself is therefore not a scientific absurdity.

Even more potent arguments are set against the prospect of an ultimate heat death by the enormous size of our macrocosm. If it is infinite in content, or if creation of matter-energy takes place according to unknown laws in the far reaches of space, all our speculations are off. Caution is suggested by the mysterious messengers, the cosmic rays, which come to us laden with energy from undisclosed astronomical origins, each a tiny contradiction of the law of averages. In earlier decades men[1] found solace in the thought that organic processes might reverse the probable trend. That speculation is wrong, but nobody seems to need consolation today.

2. We turn to the second question: Why is statistics forced to deal with probabilities? From the methodological angle it has already been answered; latent observables were to be converted into steady ones. But why did we not take averages over the actual motions of the molecules as the spirit of dynamics and the example of the comet so directly suggest? The answer is simple indeed; we could not handle the job. Hence it is impossible to say with finality whether probabilities are or are not essential in explaining thermodynamics. This makes it appear as if Gibbsian ensembles and microscopic states were screens for ignorance. Since it is impossible to determine the positions and momenta of

[1] See G. Hirth, "Die Ektropie der Keimsysteme," 1900.

all molecules at any time, one must assume some initial distribution (this is the purpose of the canonical ensemble) which produces the observed thermodynamic behavior. There is no ban, however, on raising the question as to the initial condition of all molecules in a fundamental sense; for an assignment of positions and momenta to mass points as heavy as molecules does not seriously violate the laws of nature, the difficulty being only a practical one. And one would naturally like to ascribe the success of the canonical ensemble and of the theory of microscopic states to the circumstance that, in fact, molecular assemblages never assume arrangements contradictory to the probability postulates, or at any rate to the milder circumstance that all possible configurations lead ultimately to states described by canonical ensembles.

It happens that this last assumption is not completely true. It is wrong because the laws of mechanics do permit states which never result in the kind of equilibrium contemplated in statistical mechanics. These are the periodic motions. A concerted vibration of all molecules in a container between top and bottom is an example. This, to be sure, is unlikely, but if once realized it will violate the statistical assumptions underlying thermodynamics.

But in another sense the hypothesis that all configurations lead in the end to conditions described by canonical ensembles is true. Exceptional motions are possible, but they are dynamically so unstable that the slightest disturbance will destroy them. They need not be considered at all in a statistical sense, and in fact one may show that they form a set of zero measure among the set of all possible motions. This is satisfactory and creates confidence in the postulates of statistics. A complete reduction of them to mechanical bases (ergodic hypothesis) has not been achieved. We cannot quite say that, if we know the exact positions and velocities of all molecules of a gas at all times, the laws of thermodynamics could be derived without an appeal to nondynamical factors. The situation may not be quite like that of the comet whose motion could have been predicted had its initial state been known.

Present researches (Birkhoff, von Newmann, Koopmann, Wiener) may shed light on this crucial problem. But meanwhile the philosopher of science is left with an unresolved issue in the face of this challenge which the probabilities of statistical mechanics offer for ultimate reduction. For if these probabilities are primary, if they are the scientist's last resort, they must in some sense form part of physical reality.

From another point of view the problem has lost its urgency. This is because another discipline, namely, quantum mechanics, has now been developed in which probabilities are known to be irreducible. Hence, whatever the outcome of the researches in statistical mechanics, probabilities must be incorporated in the structure of reality.

In closing, a word ought to be said about the usefulness of statistics, lest the impression arise that physicists working in this field are concerned only about academic matters of consistency and meaning. Much work is being done at present on highly practical problems, and the subject has become a valuable adjunct to thermodynamics. While thermodynamics relies upon measuring its variables, statistical mechanics can calculate its analogues from molecular properties and then predict the thermodynamic behavior of a substance even before experimentation. This practical utility of statistical mechanics has nowadays made the theoretical physicist a desirable member of chemical research groups.

Summary

In this somewhat technical chapter the theory of statistical mechanics is incorporated into the methodology developed in this book. Considerable attention is given to the questions as to what is meant by the "reduction" of thermodynamics to statistical mechanics, and in what manner statistical mechanics is able to "explain" the laws of thermodynamics.

The classical approach of Gibbs to this science is developed and contrasted briefly with that of Darwin and Fowler. One problem of great philosophic importance is whether probability ingresses into physics by reason of mere ignorance or perhaps convenience on the part of the investigator, or whether it is ineradicably stamped on science by axioms of statistical mechanics. It is shown that both these views are at present defensible.

Finally, some comments are made concerning the prospective "heat death" of the universe and its imminence.

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CHAPTER 15

Reality: A First Outline

15.1. Nonphysical Realities

We have come a long way from our original task, the quest for physical reality. At first the road seemed clear. To avoid all preconceptions, in particular to escape the traps skillfully set by ontologists all over the terrain we had to travel, we decided to let scientific method be our guide. But now it appears as though scientific method had taken us far off the main road, through all the back alleys of physics, and succeeded admirably in obscuring even the initial glimpse we had of our goal.

Such is the price we have to pay for the original decision. We could have flown to our destination, borrowing one of the balloons so plentifully available around the gasworks of ontology. Given a favorable wind we might have got there quickly. But we should not have known for sure the exact location of reality with respect to other parts of human experience and the territory which surrounds it; nor could we be wholly certain that our landing place was the one we sought.

The epistemological approach we have followed is not without peculiar hazards of its own. Being the slow advance of an earthbound party it may lose itself in some pretty valley far removed from reality; or the searchers, after long and tiring pursuits, may come home and report that their presumed goal does not exist, that it is a mirage, only visible from the upper atmosphere. This last possibility is in fact the greatest danger to which the epistemologist exposes himself. He is constantly tempted to reject all because of the difficulty of establishing any part of reality. This, according to Poincaré, is the other easy way out: "Douter de tout et tout croîre, ce sont deux solutions également faciles."

It is not our plan to save what can be salvaged from former broken-down attempts to establish scientific reality. We respect the honest conclusion of many scientists today who aver that scientific method cannot touch ultimate reality. They mean the kind of reality which looms on the ontologist's horizon. Because such reality seems to be different from their own, and because philosophers have often preempted the use of the term for their special purposes, scientists have become *unduly* reticent about the problem. Yet to them the term means something perfectly clear and definite. For who would deny that physical reality must be ascribed to stars and stones and atoms?

The cause of many epistemological fiascoes has been the time-rooted tendency to implant into reality a notion of the absolutely permanent. It is unchanging reality, a hopelessly static thing, to which the eager mind so easily falls prey. Static reality is the result of most investigations that remain wholly within the precincts of logic and apply logical rules to preformed objects of experience. The claim to self-sufficiency in establishing reality, thus made by logic, is like Archimedes' boast anent the importance of his newly discovered principle of statics, the law of the lever: "Give me where I may stand, and I will move the universe." Logic seems to say: "Give me the objects of experience, and I will define reality." Both claims are true. But Archimedes' difficulty of finding a fulcrum is a serious one; and equally serious is the matter of stating *what* are the objects of experience. Do they include, for example, the imperceptible entities of contemporary theory?

It happens that science defines a dynamic kind of reality, one that grows and changes as our understanding grows and changes. We shall not admit defeat at the outset because traditional philosophy finds this surprising and displeasing. Rather, we shall take the success of science as strong evidence in its favor.

The concepts of science themselves are subjected to the requirements of permanence in the strongest measure they will bear, since in accordance with the analysis of Chap. 5 permanence is one of the metaphysical resolutions with which science voluntarily castigates itself. But it has wisdom enough to remain alive under the castigation. In this sense, then, whatever it defines as real carries the imprint of the durable, of the principle of being.

Hence reality, as we shall see, while violating unreasonable demands of eternal persistence, incorporates the maximum of what is truly permanent in experience.

One other widespread misunderstanding should be mentioned at this point: the belief that reality is the *cause* of our experience. What this means in detail is difficult to see because of the ambiguity of the word *cause*, but it is clear that those who hold the view are unwilling to give hearing to a formulation of reality which does not explain the whole of human experience. They will be disappointed in what we have to say. To us, reality is not the cause but a specifiable part of experience. A thorough and balanced study of science exacts from its practitioners and devotees one important concession: they must accept the fact that science does not resolve the ultimate mystery of experience, if there be a mystery, which some doubt. It takes experience for granted and does not attempt to answer Heidegger's "Was nichtet das Nichts?"

Here we seem to be admitting that there are *other kinds of reality* than the physical. Yet it is unwise to leave the matter with the prejudice which such an unqualified admission invites. Acknowledgment is intended only of the evident fact that there are kinds of *experience* other than those normally standardized into scientific knowledge. As yet, they have not been united into an organized pattern comparable with the structure of physical reality, and it would be pardonable for the scientist to suggest that the name *reality* be at present denied to them. When they are thus organized, they may present the features of science or they may not. The whole arsenal of human ingenuity is available for their treatment, and it ill behooves us to prescribe a single method of procedure for all investigations. What the scientist can do, however, in concert with serious scholars in other fields using different approaches, is to examine more assiduously the applicability of his own methods to nonscientific problems.[1] Far from venturing a prognosis of the outcome of such endeavors, this

[1] An outline of the application of scientific method to ethics may be found in two articles by the author [*Am. Jour. of Physics*, 15:218 (1947); *Scientific Monthly*, 69:290 (1949)]. See also "Conflicts of Power in Modern Culture," edited by L. Bryson, L. Finkelstein and R. M. MacIver (Conference on Science, Philosophy and Religion), New York, 1947, p. 13.

book confines itself to marking the positive meaning of *physical* reality, at the present time, whether it be a limited concept or not. We regard this task as a necessary first step toward the larger goal.

The survey of physical science conducted thus far, which is incomplete because it excludes the quantum theory, makes possible nevertheless a preliminary statement of the answer to our problem. Hence it is desirable that we come to definite terms at this point in our story. In its relation to the preceding survey, however, the answer will seem needlessly complex, and the suspicion will be strong that the present account has been devious in order to appear erudite. The injection of rules of correspondence into the epistemological scheme in particular, designed to pull together sense impressions and their constructional counterparts, may seem artificial and confusing, and such fine distinctions as that between possessed and latent observables will doubtless incur disapproval because of their inutility. Their importance will be made clear later on. Our hope at present is to show that the answer to be given is not inconsistent with these refinements or, if the reader please, these artificialities.

15.2. Reality Defined

The elements of experience significant for science have been classified as (1) immediate sense data or impressions, whose totality was called Nature; (2) rules of correspondence; (3) constructs. To be valid, or to be verifacts, constructs have to meet two elaborate tests: (1) Singly and in combination, they must subject themselves to certain metaphysical requirements. (2) They must obey the stringent demands of empirical verification.

A simple but not a satisfying answer to the question of reality is available if we reserve the quality, real, for all parts of Nature. The inadequacies of such a view have already been exposed. In the first place it would embrace far more than what we could appropriately claim for the domain of physical reality since it includes the rudiments of aesthetics, ethics, and religion. Moreover, among the elements which are superficially to be labeled physical, Nature displays some (hallucinations, mirages, and so forth) which are rejected on methodological grounds. But if this indicates the need for *selecting from* Nature a certain part of its contents, further inspection shows that avenue to be blocked also. For we must clearly admit to the class of reals many things which are not immediately perceived.

These difficulties are joined by others of a more formal but equally disastrous kind. If sense data alone were recruits for reality, its domain would be ill-defined. As has been emphasized, a clear boundary of the sensory is hard to establish, the transition from data to constructs being a gradual one. Worse than that, sensory experience is of short duration, and it would be inadequate to ascribe reality to nothing but momentary surges of perception. No serious doctrine of *esse est percipi* has ever been proposed within so limited a context. What is left to the advocate of the real as the sensory is the possibility of including a certain selection from all sense perceptions, past, present, and future, of all persons and presumably of all sentient beings—but this possibility is most unattractive to anyone with due respect for clarity of statement.

Yet we cannot dismiss Nature from consideration, for certainly the impression of a falling stone is to be judged a real one by anyone who perceives it. The scientist's experiments, his meter readings, his operations—all claim title to reality. What is evident from such considerations is that the field of sense data, while coinciding partly with the real domain, does so more or less by pretension; it is unable to demonstrate its relevance by its own indigenous character. For this reason, and for others already emphasized, we are driven beyond the confines of Nature in our quest for physical reality.

Whether rules of correspondence form part of it is easily recognized by reference to ordinary language, which, after all, has just rights in settling the matter. For we are equally concerned with what reality does in fact mean in lucid scientific discourse and with what it ought to mean in cases where language is ambiguous. Rules of correspondence, so far as they are recognized, are not spoken of as part of reality. Rather, they are regarded as "mental devices," as habits of thought, or, in cases where they are sufficiently elaborate to have attracted special notice, as operational definitions. They are factors in establishing reality, as we shall see, but are not to be considered as real themselves.

Among constructs, then, the problem seems to center. If external objects are constructs, they must hold the key to reality. The tree is real because it is the rational terminus of certain rules of correspondence having their origin in sense impressions and because it satisfies the demands of consistency which common sense imposes. Atoms are real because they are rational termini of other less obvious rules of correspondence having their origin in more refined sense impressions and because as constructs they satisfy the demands made by the metaphysical principles of science into which practice and care have transformed the inarticulate maxims of common sense. Reality is conferred jointly by the process of fitting new parts into an already existing structure of ordered conceptions and by the process of empirical validation. Valid constructs, verifacts in short, are the elements of reality. Constructs are not valid because they refer to something real; on the contrary, they denote something real because they have been found valid. Hence the towering importance of epistemology.

Of course there is nothing in the act of constructing which makes its product per se a more promising candidate for the real. Whoever prefers to believe he *finds* concepts, or abstract entities, or indeed external objects within his given world may still accept our criterion of reality. Only he must think it a little strange that some of the things he "finds" are real, while others are not.

The criterion may on reflection give rise to some embarrassing questions, embarrassing at least at first sight. To certify that a construct denotes something real, there must be at hand a rational context into which it is or can be integrated. In other words, a confirmed theory must be available since otherwise one cannot be sure that the metaphysical requirements are met. To be sure, the term *theory* is to be taken in a suitable sense as including the certain, even if unconscious, associations and expectations we form about simple objects in our daily lives. But it must not be extended to the uncertain conjectures called guesses and hypotheses. One of the embarrassing questions alluded to arises at this point: Are the entities which figure in a hypothesis *not* real, and do they *become* real when the hypothesis, upon sufficient test, has been elevated to the rank of a theory?

Unless one ponders over this question at some length, it appears to present a clear issue and to require an unqualified answer of yes or no. Hence it would seem to force a parting of the ways. It is perhaps natural for the scientist to take the affirmative stand, to assert that entities indeed begin their life, start being real, when they are discovered. He might argue that other views involve mysticism and throw away every solid ingredient of reality which science offers. To be sure, he may permit a difference between reality and *knowledge* of reality as a tolerable finesse because there is, after all, a distinction between someone's having devised a verifact and my being aware of it. But to take reality out of experience, anybody's experience, means to deprive it of substance. The only possible way, he might continue, by which the scientist who wants reality antedated to its discovery can save his standing as a reasonable person is to follow Berkeley, to assume the additional presence of a divine experience for the purpose of stabilizing his disconnected reality.

There is much truth in this argument.

On the other hand, most scientists and philosophers shrink from these conclusions. They feel that reality cannot be born with man's knowledge, that discovery and confirmation are wholly incidental to existence, whose essence resides above the vicissitudes of experience in impressive, perhaps Platonic splendor.

There is a good deal of truth in that view, also.

The question we are trying to answer is not so simple as it appears and bears some resemblance to the renowned: Have you stopped beating your wife? Let us see what it means in a concrete case. In 1932 Chadwick discovered the neutron.[1] After that date a physicist's answer to the question, Do neutrons exist, would have been affirmative. Before 1932, the answer would not have

[1] J. Chadwick, *Proc. Roy. Soc.*, A136:692 (1932). For a review of this discovery and other matters related to experimental nuclear physics see J. D. Stranathan, "The Particles of Modern Physics," The Blakiston Company, Philadelphia, 1942.

been unanimous, for the discovery was preceded by a twilight period of about twelve years, during which the existence of neutrons seemed possible but could not be confirmed despite numerous experimental attempts. The *hypothesis* of a neutron, viewed as a close union of a proton and an electron (which it later turned out *not* to be), was originally suggested by Rutherford in 1920.

So, between 1920 and 1932, people would have considered the reality of neutrons possible, or perhaps likely, but uncertain.

Before 1920 the question would have elicited two kinds of answers. The matter-of-fact scientist would have said, "No"; the wary Milquetoastian, "I don't know." But the latter would have given the same answer to a query about the existence of unicorns.

Now we come to the more crucial question, to be answered in the light of present knowledge: Did neutrons exist (this is meant to be entirely synonymous with "were they real") prior to 1932? And by far the majority of scientists would answer yes. This may be taken to be the answer even if the date were changed to 1920.

But clearly, the idea was constructed in 1920, validated in 1932. And how can a construct be real before it is constructed? This apparent paradox has a perfectly acceptable resolution provided that we do not forget the results of our methodological analysis. If constructs, to be valid, had to satisfy only sensory or datal requirements, the paradox would crush all arguments. We note, however, the importance of metaphysical principles, one of which requires permanence of constructs, uniqueness and permanence of rules of correspondence connecting them with present, future, *and past* sensory experience. Thus the very act which establishes the verifact implies uniformity of its action throughout time. So far as we know it implies it erroneously in most instances, for most constructs are abrogated or modified sooner or later—but the acceptance of a construct as valid, even if temporary, must nevertheless by virtue of its metaphysical concatenation bring with it an imputation of its permanence and hence project its reality into the past. When this is recognized, it is no longer paradoxical to say: *After a construct is validated, it must be said to have been real before it was formed.*

However, this does not place constructs wholly above the contingencies of experience. There is a sense in which Rutherford's neutron was never real, for some of the properties which he attributed to it were not confirmed. In the same way, a logical stickler might deny the reality of the neutron as it is presently conceived, for it is almost certain that some of the properties assigned to it are subject to future revision. Are we to say, then, that the neutron as such is real—for one might reasonably entertain a belief in its continued function as part of physical theory—but its properties are not? As we have seen, properties are constructs, too! At this point, rather than draw more and more elaborate distinctions, I am perfectly willing to admit that reality does change as discovery proceeds. I can see nothing basically wrong with a real world which undergoes modifications along with the flux of experience. To be sure, the metaphysical context always forces us to view the past as consistently haunted by the constructs now held real, but, to be honest with ourselves, we cannot enforce that static structure rigidly upon the future. Admittedly, this deals eternal reality, the majestically reposing *εἶναι*, a crushing blow.

It is easy to succumb to the temptation of distinguishing at the outset between the permanence of physical entities and the permanence of theories about them, saying for example that the entities themselves are not affected by the vicissitudes of theories. Such an assertion amounts to granting the breakdown of the epistemological approach to reality. To support this distinction, writers at times emphasize the relative independence prevailing between theory and the more general qualities of all constituents of the physical world. For example, they would hold that the neutron as such, if it is real, will never be abandoned as part of reality although its accepted mass, its internal structure, its magnetic moment may undergo changes as theories change. They would then regard the neutron as permanent, its properties as inadequately known and regulated by the theory of the day. The trouble with such an attempt lies in its historic failure. Entities introduced by science are often disavowed *in toto*. During their reign they cannot be distinguished from elements which are real in a more durable sense.

In our view, a complete severance of entities from theories is impossible, and an acknowledgment of the life of reals apart from the life of theories which maintain them real is an error. A difference arises only because methodological principles, in their role as stabilizers of experience, make it natural for us to postulate and believe in permanence whenever reality is established. Such permanence, though putative, is the best we have, and science shows it to be good enough for all its purposes. I consider it far superior to stagnant permanence and deplore the attitude of scientists who seek the latter.

Romanticists and idealists often avoid the seemingly unorthodox thesis of a real world in progress, a reality undergoing changes induced by discovery. They succeed in this by substituting a belief for certainties: the belief in a convergence of all evolving verifacts upon a limit of *final* verifacts, which limit they then identify with some sort of ultimate reality. Indeed most working scientists avow and cherish this belief. But they would hardly stake their all upon its truth—which is unprovable. The fact is, as has already been said, that science is possible without such an assumption. And now we note in addition the possibility of defining physical reality without it.

However, there is no need for dogmatism at this point: it is perfectly proper to introduce two kinds of reality. One might be called *physical* reality and identified with what has been described. The other might be termed *ultimate* reality; it is the hopeful kind, definable only in retrospect when (and if) science has attained a sufficient degree of convergence. Ultimate reality is not itself a scientific construct; nevertheless it plays an important role in animating research, for the scientist is a whole man as well, and he lives by ideals. Whatever the psychological appeal of such *ultima eventura*, this book does not deal with them.

In the last section a vague terminology was used. Verifacts were said to "refer to" and to "denote" something real. It can now be admitted frankly that this was done to avoid shocking the reader. After the preceding explanation we hasten to correct that usage, lest it give rise to some spectral reality behind valid constructs. These verifacts, as part of experience, are reality—but they are not all of reality.

15.3. The Reality of Data; Other Selves

Earlier in this chapter we observed an important connection between immediate perception, our Nature, and reality. Only it remained problematic how a selection of real data could be accomplished, since Nature offered no help. The selection can be made, and relevance to reality can be established, by appeals to verifacts. I "really" see a tree if the object conveyed through this vision conforms to the requirements on valid constructs. The perception of a mirage is illusory, unreal, because it conveys no confirmable entity. This is the primary sense in which the term *real* is applied to data.

There are several others, whose character it is to qualify sensory impressions which have taken place in the past. One can ask whether a person had a real hallucination at a certain time, whether Joan of Arc really heard the voices, whether Cook really saw the North Pole. The first of these three questions appears quite different from the others, and particularly from the last, because it ascribes reality to an act of perception which was itself unreal in the former sense. However, all three have this in common: they inquire about *occurrences* without direct regard for their significance to physical reality. They illustrate a historian's use of the word *reality*.

But this use still conforms with our definition. Past sense impressions of the sort here treated become occurrences which constitute a conjectured temporal course of an objective universe. Thus they are constructs and need to be verified as such. The methods of confirmation are, we believe, identical with those outlined in this book, although the emphasis seems distributed differently among the different requirements. The process of empirical verification still has its circuital form, with one occurrence suggesting another and finally driving the inquiry into the realm of the immediate, which in the present case may well be an ancient text or a phrase in a document. It is not our purpose, nor does it lie within the author's competence, to examine extensively the meaning of historical reality. But it is safe to say that, when historical facts, past occurrences, are viewed as posits to be confirmed, established history becomes a real sequence of objective events and shares many of the attributes of physical reality.

After this digression we return to summarize the status of data. Two meanings of the term *real*, as applied to sense impressions, must be distinguished and will be accepted. The first assigns to perceptions the quality of having to do with the real world, of being bona fide and nonillusory. This quality arises from relations of the particular impression with verifacts and with other impressions. The second meaning concerns sensory acts as occurrences and asserts that they really happened. Here the act itself loses the vividness and coercion which the immediate possesses in one's own experience and becomes a *construct* in need of being validated.

The situation is somewhat different with respect to sense data which I, myself, remember. They authenticate themselves as being real in the second sense and therefore require validation only in the first. The difference, however, is a gradual one: dimly remembered sense impressions partake of the nature of constructs and require verification of both varieties—we have already seen how, along the stretch of memory, the immediate takes on progressively the character of the rational.

In the following our interest will be confined mainly to reality of sense data in its first meaning, their being nonillusory. The other, actual incidence, is always taken for granted by the scientist when he uses data in his empirical procedures.

We are guilty of having talked about past sense impressions of others without indicating what the experience of others means. The epistemology filling the preceding chapters acknowledges only "experience," which presumably means my own first-person experience; reality is constructed from the elements of that experience and is still part of it when fully formed. Does that not anchor us in solipsism? To say that objects around us are real is to claim for them the character of verifacts; it is at once the maximum measure of actualness, of authenticity which can be assigned to them. This protocol of reality, thus understood, is the ultimate epistemological commitment. Other persons exist for me in precisely the same exhaustive and yet unmysterious sense. When they are said to be (physically) real, they are presented in this character as verifacts within one's own range of knowledge; their experiences, their "selves" are given and confirmed as are all other cognitive certainties. In this way the experience of others, with their manifestations as sentient beings now elevated to the status of reality, becomes an important part of my own experience.

15.4. The Real World

According to the foregoing analysis the real world comprises all valid constructs and that part of Nature which stands (or stood, in the wider sense which includes historical reality) in epistemic correlation with them. These are the elements which have (or had) physical existence. A brief survey of them will appropriately close this chapter.

The things recognized in our daily lives, both inanimate and living, clearly belong to the real world because our criteria were formed with special reference to them. In their further elaboration, the criteria continue to specify what is real in the invisible universe of science. Here, however, the guidance of common sense must sometimes be relinquished in favor of the verdict of abstract principles, though that verdict may seem strange.

As to the reality of entities postulated within the molecular and atomic domain no question can arise despite their indirect accessibility to perception. But it is equally clear that they need not possess the qualities suggested for them by macroscopic experience. Hence it is not paradoxical to conclude that atoms have no color, electrons no definite position, and so forth. The same considerations permit the assignment of nonintuitable qualities to objects in the large without destroying their reality. The physical universe, for example, may be conceivable only in four dimensions and may yet exist as an acceptable and real physical entity.

But what about the properties these physical objects carry, properties which measurement reveals as varying from instant to instant? If they are recognized as valid constructs—and we believe this premise to have been established in the preceding chapters for all physical observables—reality cannot be withheld from them. There is an appearance of inconsistency in this conclusion, since quantities are not permanent and seem to violate one of the metaphysical principles imposed. But that appearance vanishes upon a moment's thought: As observables, quantities are affixed to systems with relative invariability, although their *values* change. The position of a moving body may change continuously without forcing us to deny the enduring manifestation of the construct, position, at all times.

The next question at issue asks about the reality of the changing numerical *values* of physical observables. A piece of matter is real; its temperature, being a permanent observable, has also been accepted as real. Now we say the temperature is at the moment 50°F. Are the 50° real? Here we are pressing the scientist to commit himself to a judgment which he is rarely called upon to make. In so far as the value, 50°F, can be established with certainty and violates no maxims of reality except permanence,[1] he would be willing to relax the demands of that principle and admit the value as a momentary item of reality. This would most probably be the physicists' consensus, expressed in a statement like this: The real world contains a block of metal, which is now at a temperature of 50°F. But if someone wished to deny this particular value the rank of reality, the scientist would hardly raise a strong objection. There are instances, after all, where a clear verdict need not be rendered, where the issue of reality becomes uninteresting.

In Chap. 7, a strong preference has been expressed for the interpretation of space as a *relational* entity. It should now be emphasized that this admission reflects no prejudice upon the *reality* of space. Spatial constructs such as point, line, and surface, subsist in multiple and stable correspondences with data of immediate experience, converting data into integral parts of our total experience and thus rendering them understandable. For

[1] It should be remembered that the principle of permanence, like all other requirements for verifacts, does not operate in an absolute sense. We have tried to be careful in providing sufficient flexibility for all metaphysical principles to have them function by compromise and in unison, rather than in a manner which would exclude the claims of one in favor of the rigid demands of another. Seekers of static reality may not like this concession, or perhaps they will revel in it as a triumph for their side; but we believe that science in honesty must make it.

that reason they are verifacts and have equal claim to reality with the reified objects we encounter. From points, lines, and surfaces, space can be constructed, and this new entity, though abstract, retains the functional alignment with Nature which has been posited as the character of reality. Physical reality is not synonymous with concreteness, as that term is ordinarily understood.

The elements of geometry are often called idealizations of the crude immediacies presented by Nature. This is not to be denied. But there is a sense in which every passage from the *P* field to constructs is an idealization, a formalization under the metaphysical requirements which is made necessary by the inherent uncertainties of Nature. In our view, idealization is not antithetic to realization. For, clearly, if one adopts the term *idealize* to describe the special instance of a rule of correspondence which defines the construct "point," one should also grant that *reification* is a form, albeit perhaps a simpler form, of the idealizing process.

We here find ourselves in apparent contradiction with a judgment prevalent among scientists, namely, that perfectly straight lines, perfect triangles, and so forth "do not exist in nature." To restore harmony we need only examine carefully what this statement implies, for it does not deny reality to the elements of geometry; it merely asserts that *bodies* do not exist which exhibit perfectly straight lines or perfect triangles, which is true and not contrary to our conclusion. The geometric elements themselves are nevertheless real as constituents of abstract space, which is a physical reality. While no real body exhibits a straight line, it is still true that a real straight line extends between any two points of space (and that space consists of real points). A line in space is a real but not a material line.

A number is clearly an observable of a class of objects; it is real in so far as Nature presents countable immediacies which lead, by a rule of correspondence involving the operation of counting (epistemic definition), to the construct designated by a specific number, *N*. Hence numbers are real if existential counterparts for them are available. This is certainly true for all the smaller integers. But at the present time we must probably suppose that the total number of discernible objects in the universe is finite, the number of permanent elementary particles being of the order 10^{80}. To be sure these can be ordered into sets, vastly greater in number but again forming a finite class. A number greater than that of the members of this class would defy the criterion of reality we have employed.

But in fact we are not bound by this simple procedure. The rules of correspondence need not be as direct as was here assumed. For we have already generated real space, with a network of geometric elements as numerous as may be desired, and numbers may be associated with these previously established verifacts by constitutive definitions. These constitutive definitions may yield nothing more than peninsular constructs of type C' (Fig. 5.1), numbers which are related to the raw material of the P field only by the rules that gave them meaning. From these, reality must be withheld. Whether they occur can hardly be settled in the pages of this book, but is an interesting question in the philosophy of mathematics. Infinity may be one of them. My own inclination, however, is to doubt it. For it seems that the scientist, in his efforts to understand the presentations of Nature, particularly when rationalizing the quality called continuity, needs all the numbers the ideal procedures of the mathematician can generate—including infinity and the irrationals.

Let it not be supposed, however, in view of this large concession, that methodological principles have lost their hold on the factual situation and that they predict rational realities without restraint. Construction alone will not suffice to engender reals; imagined creatures are constructs but are unreal because they lack counterparts in Nature, whereas numbers possess them.

Such discernment of the methodological principles is evident again in the effective manner with which they distinguish between real and unreal abstracta. If the general theory of relativity is correct, Euclidean space does not possess reality, although of course it may still be a *useful* approximate model for the description of experience. Usefulness, the pragmatist's criterion, is not alone significant for establishing reality. Indeed many mathematical constructs are first formulated without any concern for their reality. Even while unequipped with epistemic correlations, they may serve the useful purpose of synthesizing many abstract relations. And often, as the history of science progresses, they take on reality upon discovery of suitable linkages with sensory experience. To cite an example, matrices have undergone this change with the development of quantum mechanics.

Similar comments could be made about groups, algebraic fields and many other "ideas" of mathematics. A group is not a mere idea but a significant part of reality because interpretation of Nature requires it: Among the properties of a crystal is that of being a group. Mathematical functions present much the same aspect. The function $\frac{1}{2}mv^2$ (not only the symbol or the name of it) stands in correspondence with certain P experiences, the rule of correspondence being the operational definition of kinetic energy. On the other hand, the function itself, as a fertile construct, violates none of the formal demands to be put on verifacts. In our sense, then, being of the form $\frac{1}{2}mv^2$ is one of the real attributes of kinetic energy. But again we admit that this is a strange way of talking and that we are rapidly approaching an area where concern over the issue of reality is wanting among scientists.

To this area belongs certainly the question as to the reality of *universals*. Electrons, wavelengths, the states of physical systems, as individuals are verifacts. But what about the term electron without reference to particulars? I believe that concept to be of the insular type and would exclude it from the world of reals. Yet I would grant that the epistemology developed in this book needs only slight extensions to make the reverse judgment preferable.

The Gibbsian ensemble, discussed in the last chapter, occupies a peculiar place in the realm of physical being. It is a construct in the inventive sense of the word, a palpable fiction from the point of view of realism; for it seriously offends common sense by artificially creating a multiplicity of systems when there is obviously only one. Hence one would like to regard it as an artifact. However, consistency leaves no choice in our decision. At first sight the ensemble violates no principle of the methodology of science, is not barren, and permits clear rules of correspondence with thermodynamic observations. Therefore we seem obliged to admit it as a constituent of physical reality, its grotesqueness notwithstanding. On all these counts we are asked, in fact, to associate with a given pail of water a postulated infinity of pails of water in order to be able to explain the one pail's thermodynamic behavior. The reality of this ensemble is based on the same kind of evidence as the reality of the molecules of which another theory alleges the water to consist. Here we have the crucial fact: one cannot reject the Gibbsian ensemble as unreal and retain Gibbs' statistical mechanics as correct.

Fortunately the Gibbsian theory is inadequate, and the reader's comfort in the matter of reality is saved. For there is one principle which the notion of ensembles does violate: the principle of simplicity. Though vague in its operation, simplicity nevertheless decides in favor of the molecular theory and the form of statistics introduced by Boltzmann and refined by others. This form of theory accounts equally well for the facts of thermodynamics but accounts in addition for many other observable facts to which Gibbs' statistics are not applicable. When the molecular theory is accepted, the limited success of the Gibbsian method of calculation can in fact be demonstrated and shown to be due to an elegant camouflage of molecular assumptions, and the ensemble is thus shown to be an artifact. Its reality (but not its usefulness) is thus destroyed. Respect for historical accuracy forces us to add that Gibbs was completely aware of all this; to him the ensemble was nothing more than a successful algorithm.

We have enumerated many items which are real and some that are not. Perhaps it should also be stated that relations between constructs, such as purely logical relations of implication or contradiction, or quantitative relations such as equality, being greater than, and so forth, should not be regarded as verifacts themselves. One must conceive of them as being neutral with respect to the question of reality. The same is true of the regulative principles of Chap. 5; while their bearing upon reality is very important, they do not lay claim to being real themselves. They might be said to act as verifactors rather than as verifacts.

One may well feel that the foregoing analysis, when carried through with greater thoroughness than the problems of physical science seem to require, will lead to a satisfactory resolution of the controversy between nominalism and realism. But in the author's opinion the outcome will not be wholly in favor of one or the other contested doctrine.

Summary

Valid constructs, *i.e.*, verifacts, are the elements of physical reality. This view raises several questions, which are discussed in the present chapter. One concerns the status of verifacts at a time prior to their construction (or discovery, if a more traditional terminology is preferred) and perhaps after their rejection—for few, if any, scientific constructs are valid forever. The paradox which seems to be involved in almost every answer to this question is resolved by noting that the acceptance of a construct as valid requires, in view of the developments of Chap. 5, an *imputation of permanence* as a methodological act: after a construct is validated, it must be treated as having been real before it was formed.

Another question concerns the reality of other selves. If the definition of reality here suggested is adopted, one can no longer be a solipsist. For it is then immediately clear that other selves, the experiences of others, have the rank of verifacts in my own experience and therefore enjoy a status wholly commensurate with stars and stones and atoms.

The reality of data, *i.e.*, Nature, requires special consideration, for a sensory perception can be real in two different ways. The distinction is briefly discussed in the present chapter. Further attention will be given to it in Chap. 21, where the idea of historical reality is more carefully worked out.

Section 15.4 surveys the contents of the real world and deals with the questions of the reality of abstracta such as space, number, group. It briefly raises the problem of universals.

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CHAPTER 16

The Breakdown of Physical Models

16.1. The Crisis of Classical Mechanics

The revolution in the physicist's conception of his universe which occurred during the last three decades is comparable to the upheaval that took place in the sixteenth and seventeenth centuries, both in scientific fertility and in philosophic significance. In the last analysis the older movement, which culminated in the works of Francis Bacon, Tartaglia, Galileo, and Newton, is to be viewed as an emancipation of physical doctrine from the presuppositions of scholastic ontology, a first frank avowal by natural science of epistemology as the highway to its goal.

The recent transformation, despite its radical character, is not so easy to describe. An aura of strangeness still surrounds the novel ideas it has generated, and an air of incredulity beclouds the the judgment of the astonished investigator who ponders over its final meaning. Proper terminology for an appraisal of the philosophic consequences of new theories is not yet at hand. In the writings of scientists, two noncommittal and to some extent misleading phrases are frequently relied upon to convey the flavor of quantum physics; one marks it as a transition from descriptive to *symbolic* understanding of the physical world; the other (Bohr's interpretation) assigns to the new developments the character of a "complementary" [1] transcription of physical processes. The precise meaning of these assertions will be analyzed in the present and the following chapters. Our conclusion will be that modern theory, far from abandoning epistemology as a guide to understanding, has reaffirmed its faith in that discipline and has decided

[1] The earlier German word *korrespondenzmässig* is perhaps more appropriate. Roughly, it adverts to a description of atomic processes in terms other than ordinary spatiotemporal, but in a certain sense equivalent to ordinary description.

to trust it to the last, even beyond the precepts of common sense.

A review of some factual matters which signalize the breakdown of mechanistic hypotheses in the field of atomic physics is indispensable in this attempt. We ask the reader's indulgence, then, for the brief account of history that follows.

The story begins approximately at the turn of the century, when the dazzling successes of a universal application of mechanical reasoning had blinded men's awareness to all alternative modes of understanding. Electrodynamics, based on the use of models, had penetrated the field of optics and was able to give a good account of heat phenomena as well. Suddenly this science was stopped before two major obstacles which, after many attempts, it found itself unable to surmount.

The first was placed in its way by spectroscopy, a budding branch of science which was surer than all others of its experimental ground. Anybody who looked at a spectrogram could see that spectral lines emitted by atoms were "sharp"; they had very definite frequencies or wavelengths. What could electrodynamics do with this observation? The atom was known to consist of a positively charged nucleus surrounded by negative electrons. If the system was to be dynamically stable, the electrons had to move about the attracting nucleus in periodic orbits, and in so moving they underwent accelerations. For there can be no periodic motion without acceleration. Here, however, lay the difficulty: according to the principles of electrodynamics an accelerated charge lost energy by radiating. In consequence, the electron should spiral inward toward the nucleus, change its frequency of revolution, and, after some time, collide with the nucleus. This picture therefore leads one to expect a number of facts which contradict experience. First, the atom should radiate at all times; in fact it radiates only when it is excited (thermally, by ion bombardment, etc.). Second, its radiation should be of continuously varying frequency; in fact the frequency is constant. Third, it should collapse when the process of radiation is completed—and this can be shown to require but a small fraction of a second; in fact the world has remained stable for a considerable period of time. Obviously something had gone wrong with electrodynamics.

The second obstacle rose in a field closely related to spectroscopy and was presented by the mysterious behavior of incandescent solids. When a solid is heated, its small constituent parts vibrate about positions of equilibrium. The fact that the body emits light invites an application of electrodynamics, which can account for the occurrence of light emission on the assumption that the small constituents are electrically charged. Nothing would have been granted more readily, for there is abundant corroborative evidence for this assumption. But this modicum of initial success is completely marred by the further details of the picture. Having once entered the scene, electrodynamics does a complete job of prescribing what should happen; in particular, it specifies the intensities with which the various wavelengths must be emitted. And it does so in complete violation of the observed facts.

It will perhaps be recalled how the first form of quantum theory arose in the midst of this embarrassment. Planck, having exhausted all means permitted by traditional electrodynamics, discovered how one heretic but nevertheless simple hypothesis restored agreement. If he assumed the vibrating constituents of solid bodies to be in *quantized* states of motion, if a given oscillator could possess not any amount of energy sanctioned by customary reasoning but only an integral multiple of one fundamental "quantum" ($h\nu$), then theory and observation came to terms in beautiful agreement. But the whole substance of classical physics was traded in exchange for this success.

To Bohr the price did not seem high, for he immediately perceived the great potentialities of the quantum in other directions. To him it became clear at once that it offered the solution of the former problem, that posed by the sharpness of spectral lines. If the oscillating charge composing a solid body is constrained to move in *quantized* fashion, similar integral rules can be imposed on the electrons of an atom. The electron, then, does *not* radiate when it resides in a quantized state, for if it did, quantization could not be maintained. Hence radiation occurs only when the electron passes from one quantum state to another. Quantum states are nonradiating states and hence are stationary, and all is well so long as a stationary state is maintained. The stability of the universe is thus saved. What happens as the electron passes from one stationary state to another is a further story whose plot is not determined by the quantum hypothesis alone.

But here Bohr was guided by the facts he wished to explain. Since the jump of an electron from one orbit to another involves absorption or emission of energy, and atoms absorb and emit energy in the form of light, a jump must correspond to the absorption or emission of a spectral line. Now according to spectroscopic observation the spectral line emitted during the passage had a definite frequency, while theory said that of course the energy released was definite in amount and equal to the difference between two quantized energies. What is more natural than to suppose the one to be proportional to the other? To suppose that the energy difference between two quantized orbits, ΔE, shall equal a constant times ν, the light frequency emitted, is thus quite plausible. But to find perfect agreement with experience when this constant was given precisely the value h, previously postulated by Planck, was most astonishing indeed.

The details of the theory which resulted from Bohr's brilliant conjecture need not detain us long. They are usually outlined in the form of two postulates. The first is a mathematical rule for finding the quantized energies of an electron. It will be necessary to explain it briefly because the idea of *quantum numbers*, which dominates much of the atomic scene, enters the stage through this postulate.

In advanced dynamics, a quantity called *action* plays an important role. Although its exact meaning need not concern us, let it be stated that it is a variable whose value increases as the motion goes on; in fact, in a periodic motion the action increases by an equal amount during every period. In this respect action resembles the time variable itself. But there is one complicating feature; while the dynamic system has only one time, it possesses as many different action variables as it has degrees of freedom. Thus we may think of one action associated with every coordinate of the moving system, and each action increases in time. If the electron moves freely in three dimensions, it has three degrees of freedom and hence three action variables, which we shall call A_1, A_2, and A_3. Bohr's first postulate requires that the increase in every A

which takes place during one period of the *stationary*, or quantized, motion shall be an integral multiple of Planck's constant h. In symbols,

Bohr's postulate I:

$$\Delta A_1 = n_1 h \qquad \Delta A_2 = n_2 h \qquad \Delta A_3 = n_3 h$$

To this, we now add the second postulate, which is none other than the relation between ΔE and ν already stated:

Bohr's postulate II:

$$\Delta E = h\nu$$

Postulate I selects all possible quantized motions; they are found by substituting for n_1, n_2, n_3 all integers from zero to infinity. These integers are called *quantum numbers*. Postulate II makes contact with observations by allowing the frequencies ν to be computed when ΔE is known, and ΔE in turn is calculated with the use of well-known laws of mechanics.

Application of the Bohr theory to the hydrogen atom resulted in spectacular agreement with observations all along the line. Accuracy in spectroscopic measurements was notably high, and now theory was suddenly able to predict correctly the wavelengths of spectral lines to as many decimal places as measurement could yield. Spectroscopy, until then a tantalizing game with numbers, completely revealed its secrets. The acclaim of the new theory was universal, and it was natural for physicists to believe that the deepest of their mysteries had been solved. Hence it is understandable that, in the excitement over its success, men overlooked a malformation in the theory's architecture; for Bohr's atom sat like a baroque tower upon the Gothic base of classical electrodynamics.

The "postulates" of the new theory were perhaps too specific to lay proper claim to that name; by deftly suspending the validity of classical physics in one instance and using it in others they convey the appearance of being superb heuristic devices which fail to furnish basic understanding. Yet, whatever the methodological faults of Bohr's early atomic theory may be, it properly emphasized the generality of quantized motions and gave wider application to a mysterious new constant, Planck's h. That theory was the first step across a maze of challenging paradoxes, and it was also the last attempt to understand the workings of the microcosm in terms of models employing the familiar motions of visual mechanics.

The factual inadequacy of this atomic picture became apparent soon after its discovery. Even its greatest success, its explanation of the spectrum of hydrogen, was marred by a minor failure. The quantum number n_2, associated with the angular motion of the electron, refused to take on the value 0 as it should have, while the others did take this value. That is to say, to obtain the correct results for the energy of the atom it was necessary to assign to n_1 and n_3 the sequence 0, 1, 2 ... but to n_2 the sequence 1, 2, 3 .... Further evidence, furnished by magnetic studies (Landé g formula) indicated that $n_2 - 1$, and not n_2, was the important quantum number describing the angular motion. But if Bohr's theory requires different rules for different degrees of freedom, the suspicion of being *ad hoc* which attached to it from the beginning becomes a threat even to its usefulness.

However, things deteriorated further. The disease which befell the quantum number n_2 was contagious. When an application of the theory to other physical systems, notably the rotating and the vibrating molecule, was eventually made, other quantum numbers appeared. And observations on the spacing of lines in the spectrum bands emitted by molecules indicated clearly that only *half-integral* quantum numbers could render an account of the facts. Moreover, in many cases where theory demanded an integral quantum n, observations insisted on $\sqrt{n(n+1)}$. Can it be true, then, that quantum numbers are sometimes integral, sometimes half integral, and sometimes even irrational?

Perhaps the most damaging of the theory's failures was its inability to account for the spectra of atoms having more than one electron. This failure is also the most revealing, for it indicates a false focus of attention, an adherence to those features of the atomic problem which cannot in the end be essential. Perhaps this is best seen in the attempts which were made about 1915 to apply Bohr's postulates to the two electrons in the helium atom. In doing so, specific assumptions needed to be made about the relative position and velocity of the two electrons. All possible assumptions were tried: the two moving particles were tentatively located in a single fixed plane, then in space; their motions were described with all conceivable phase relations between them; they were made to move in the same and in opposite directions of revolution—but the correct energy of the helium atom could not be obtained. At this point physicists began to ask themselves whether a theory must deal with so much minor detail before it can give a simple major answer. Are the constructs with which the Bohr theory operates—the sense of revolution of the electrons, the relative phase of their motion, their being in a single plane—germane to the problem at hand? Could experiment ever guide us to knowledge concerning them?

Such increasing encumberment of theoretical notions with minute detail gradually came to be looked upon as an indictment of the methodology which they involved. Finally when, in 1925, Goudsmit and Uhlenbeck discovered what is called the electron spin, scientific imagination was stretched to the breaking point. To account for certain spectroscopic observation it seemed necessary to suppose that the electron revolves about its diameter and thereby produces a magnetic moment; but, in doing so, the good old spherical (Lorentz) electron rotated so fast that a point on its circumference moved with a speed 300 times as great as that of light. Here theory became incredibly specific, and, what is worse, it appeared to violate the secure facts of relativity.

The theoretical structure that followed the ailing theory of Bohr, it seemed, would have to be less specific in the demands it made upon prior knowledge of the structural details of atoms: it would have to be more modest in its claims.

16.2. Dualism between Waves and Particles

A realization that the difficulties with physical theory were of an extraordinary sort, with their roots extending way down to the basic stratum of philosophy, became clear when the atomic dilemma was seen in its relation to an even more perplexing paradox concerning the nature of elementary particles. This paradox developed principally with respect to *light waves* and *electrons*, to which we here confine our discussion, but affects in an equal measure all other elementary constituents of nature: protons, neutrons, mesons, and neutrinos. The facts of the situation are best presented under the four headings I*a*, *b*; II*a*, *b* with numerous subitems, as follows (our summary will be succinct, since the material itself is elementary and well known):

I*a*. *Evidence for Assuming that Light Is a Wave*

1. The *propagation* of light is described by Huygens' principle; this principle has been demonstrated mathematically by Huygens, Young, and Kirchhoff and can be so demonstrated only on the premise that light is a wave motion, mechanical or electromagnetic. Newton, who contradicted Huygens on very plausible grounds and held a view which imparted both corpuscular and undulatory properties to light, did not achieve victory over Huygens' proof.

2. *Interference* of light is wholly incomprehensible on any model but a wave model, for two particles cannot destroy each other, whereas one wave can cancel the effects of another if the crests of one coincide with the troughs of the other.

3. *Diffraction*, a form of interference, leads to the same conclusions.

4. Light may be *polarized*, *i.e.*, have different properties in different directions transverse to the line of propagation. The most natural explanation of this long-known fact is to assume that light is a *transverse* wave, in which the vibration is at right angles to the direction of propagation. All experiments agree with this interpretation. To be sure, polarization does not totally contradict the hypothesis of light *corpuscles* since the latter may be assumed to be unsymmetric and thus to establish a preferred direction transverse to that of propagation. But all available evidence and the artificial character of this assumption are very much against it.

5. Light has never been observed to possess an *intrinsic mass*. It is known to have energy and momentum when in transit and therefore a relativistic mass; but no one has ever stopped light and weighed it. This is no doubt the leading consideration which

made the corpuscular hypothesis unpalatable to the early investigators. Even today it goes against one's grain to conceive of a wave *with* mass or a particle *without* it.

b. Evidence for Assuming that Light Is Corpuscular

Light corpuscles are called photons, and we shall use this name whenever it is convenient. The early arguments of Newton in favor of the photon hypothesis (see the Queries in his book on "Opticks"), while ingenious and interesting, were based on fragmentary knowledge and will not be reviewed here.

1. Planck's discovery of *quantized energies* suggested for the first time since Newton that light might not be a wave. A wave carries its energy continuously throughout its spatial extension, and the easiest way to account for quanta of energy is to postulate chunks or darts of light, to assume discrete photons. That these chunks are particles is not directly indicated by Planck's idea but is peculiarly compatible with it.

2. The *photoelectric effect* receives a most natural explanation in the framework of the photon hypothesis, as Einstein was the first to perceive. When light falls upon a metal surface, electrons are found to jump out from the surface. These electrons, which normally reside fairly placidly within a metal, abstract energy from the impinging light as it is being absorbed by the metal, and when they have accumulated sufficient energy, they are able to tear themselves loose from their metallic confinement. By other methods it is possible to measure the amount of energy needed for an electron thus to free itself, and it is not difficult then to compute the time in which a light wave of known intensity would convey the needed energy to an electron. Suppose that time to be one second, say. Clearly, if light is a wave, and if this conception is adequate, we must shine the light upon the surface for at least one second before the first electron can make its photoelectric leap and after that we expect a swarm of followers. This, however, is not observed. On the contrary, the first electron comes forth almost immediately without waiting the decent period of one second; it shows no regard for conservation of energy or any of the amenities of the wave picture here involved. Nor do the others; they jump whenever they please but always in such a manner as

to maintain an equality between the *average* energy of the electrons and the energy abstracted from the wave.

Doubtless the simplest way to make plausible this frivolous behavior of the electrons is to deprive light of continuity. If light itself consisted of darts in photon fashion, and if these darts were distributed at random through the region of what we normally call the wave, the electrons of the metal when struck by darts must emerge in chaotic succession. This qualitative idea takes on precision and compelling force when it is coupled with Planck's discovery, in other words, when an energy $h\nu$ is assigned to every photon of frequency ν. Then, as Einstein showed, even the quantitative features of the photoelectric effect yield to satisfactory explanation. At this stage we are asked to think of light as a random swarm of darts (photons), each equivalent to a package of energy containing $h\nu$ ergs of energy. What a photon "looks like," or how a dart manages to have a frequency ν are questions on which we remain respectfully silent.

3. The reverse of the photoelectric effect occurs when *X rays* are produced. In an X-ray tube, electrons fall upon a metal surface and release photons (in this case X rays, which are photons of large ν). Again, matters are satisfactory when each photon is provided with an energy $h\nu$, an energy which is in this case abstracted from the impinging electrons. Hence the production of X rays points to the same conclusion as does the photoelectric effect, namely, that photons are bundles of energy moving through space in random fashion.

4. If the accumulation of evidence thus far is already beginning to strangle the wave, the *Compton effect* deals it a final blow. One of the surest things we know about a wave is that it cannot change its frequency once it has come into being. It will change its velocity of propagation and its wavelength as it passes from one medium into another, its direction will be altered on reflection and on refraction, but the quantity ν is indelibly marked upon it as part of its own true nature. Hence it was truly astonishing when Compton discovered in 1923 that scattering of X rays by material objects altered their frequency. This grotesque violation of the principles of wave motion found automatic sanction when viewed as an interaction of *particles*. Compton showed with beautiful

simplicity that if a *corpuscle* of energy $h\nu$ (and corresponding momentum $h\nu/c$) collides with a material particle, it retains after collision smaller energy $h\nu'$ which agrees exactly with the altered frequency ν' observed in the scattering experiments. Thus light not only appears as a swarm of particles in so far as its energy quantization is concerned—indeed it actually behaves like particles in collision with other particles of matter.

5. Less definite but equally impressive is the evidence drawn from the action of Geiger counters. These instruments, so widely needed in our atomic age, indicate the passage of charged particles. Essentially a counter is a vessel whose interior is under high electric stress, a cylinder maintaining an electric field near its breakdown value. If the potential across it is raised, an electrical discharge takes place and is registered by amplifiers connected with the instrument. Such a disturbance in the potential occurs when a charged *particle* passes through the vessel, as one can easily see. The remarkable thing is that it also happens when a high-energy *photon* (*e.g.*, gamma ray) passes. But it is difficult to see how a wave can trip the counter, or indeed how it can have the sharp directional properties, the dartlike character which make it go through one vessel and not through another close by.

II*a*. *Evidence for Assuming that Electrons Are Corpuscles*

1. In the form of cathode rays, electrons were recognized from the very beginning as bearing negative *electric charge*. That fact was manifest in the deflection suffered by beta rays when they were sent through electric and magnetic fields. Hence no doubt was left as to their corpuscular nature since a manner in which a wave could carry charge is impossible to conceive.

2. Equally decisive and established by the same experiments was the conclusion that electrons carry *mass*. Wiechert and Kaufmann determined the ratio of charge to mass of an electron with sufficient accuracy to show its difference from the ions in electrolysis, and Thomson made the first quantitative measurement of this ratio by methods which most students remember from their freshman physics course. It seems unthinkable that a wave can have both charge and mass.

3. The *velocities* of the electrons were found to have values depending on the potential difference across the cathode-ray tube. The point at issue here is the occurrence of a variety of velocities in the same medium, the vacuum of the tube. Electromagnetic waves, which are the only waves known to be transmitted by a vacuum, travel with a single unique velocity. This observation was astonishing and was regarded as further corroboration for the corpuscular nature of electrons.

4. Millikan's celebrated experiment allowed an independent and accurate measurement of the *electron charge*. He was able to catch an electron on a drop of oil and to determine the electric force which would hold it still, that is, would keep the drop from falling. At least as interesting as the ingenuity of the experimental setup and its well-known practical success for science is the rarely noted circumstance that here, apparently for the first time, electrons were made to stand motionless, which obviously no self-respecting wave would do.

5. To adduce further evidence may seem superfluous. But certainly the Wilson cloud chamber cannot be dismissed without mention. Like other charged particles, electrons leave a streak of droplets as visible evidence of their path, and these droplets are easily photographed and studied. It is difficult to conceive how a wave, uncollimated and free to spread in space, could make narrow tracks such as those observed in a cloud chamber.

b. Evidence for Assuming that Electrons Are Waves

1. In 1924 Louis de Broglie wrote an epoch-making doctor's dissertation. Shutting his eyes to the intuitive content of classical physics, he inquired what would be the formal consequences of combining the theory of relativity with Planck's quantum hypothesis. His reasoning is so simple that we feel tempted to set it down in outline. According to Bohr and Planck, he argued, the energy E of a physical system is related to a frequency ν by the relation

$$E = h\nu \tag{16.1}$$

Let us therefore assign to an electron a vibratory "phenomenon," of frequency ν. We do not ask how this can be visualized or how it can be mechanically maintained.

An observer who moves past the phenomenon with a velocity v will see it, not as an up-and-down vibration, but as a progressive *wave*. The theory of relativity, invoked at this point, easily shows by simple steps, which are omitted here, that this wave moves forward with a velocity

$$V = \frac{c^2}{v} \tag{16.2}$$

and not, as might be naïvely expected, with the observer's velocity v. The constant c is again the velocity of light. Now we make use of Einstein's mass-energy relation

$$E = mc^2 \tag{16.3}$$

On combining Eqs. (16.1) and (16.3) one finds

$$h\nu = mc^2 \tag{16.4}$$

But according to (16.2), $c^2 = vV$, and hence, from (16.4),

$$h\nu = mvV \qquad \text{or} \qquad \frac{V}{\nu} = \frac{h}{mv}$$

Now the reader will recall that the ratio of the wave velocity V to the frequency ν of a wave is the wavelength λ. Hence de Broglie's famous formula,

$$\lambda = \frac{h}{mv} \tag{16.5}$$

When summarized, the preceding argument amounts to this: If Einstein and Planck are both formally right in their contentions, an electron—in fact any particle of mass m—has associated with it a wavelength whose value is given by Eq. (16.5). This is a strange conclusion. Does it mean that electrons, like other physical systems exhibiting wavelengths, can be made to interfere, to be diffracted, and to be polarized?

2. In 1927 Davisson and Germer of the Bell Telephone Company discovered by accident that an electron beam, reflected from a nickel crystal, showed a diffraction pattern very much like those produced by X rays. From the pattern the wavelength of the electrons could be computed, and *it agreed with formula* (16.5).

3. In 1928 G. P. Thomson, the son of the man who first "proved" the corpuscular nature of electrons, passed swift cathode

rays through foils of metal and observed the formation of diffraction rings similar to those produced when X rays are sent through crystalline powders. Again, the wavelength calculated from the rings provided a check on de Broglie's formula.

4. A year later Rupp in Germany *diffracted* electrons by means of optical gratings and proved the same formula valid by the standard method employed in spectroscopy, using ruled gratings. At this stage, the undulatory nature of electrons was established beyond doubt, and utilization of the new knowledge was begun in the design of electron microscopes.

5. Meanwhile, a host of experiments have demonstrated the possibility of *polarizing* electrons. In fact, the electron spin discussed in the previous section turned out to be nothing other than the direction of polarization of the electron waves.

Indeed the electron possesses *all* the normal attributes of waves.

16.3. The Fundamental Nature of Elementary Particles and the Haziness of the Immediately Given

The scientist steeped in traditional theory will now demand to know *what is* the true nature of the photon, *what is* the true nature of the electron. Are these elementary constituents of the physical world a mixture of particles and waves or are they neither? The mixture theory is in common vogue; to give it currency and verbal sanction Eddington invented the name "wavicle" as a presumably appropriate descriptive term for the electron. This adds a pleasing twist to our inquiry and coaxes the uncritical to believe in the mixture theory as a solution of our problem. On further thought, however, one is forced to say that such word play, far from representing a solution, even destroys the means for getting one. For it tries to persuade us that there is no problem; it asks the philosopher to play tiddlywinks when he should be minding his business. His natural instinct that there can be no fundamental ambiguity in the realm of last things is sound, and it is to be hoped that it will not be surrendered to the physicists' easy dualistic chatter.[1]

[1] Eddington himself, of course, was quite above reproach in his philosophic treatment of the "wavicle" problem.

To clarify the problem we note to begin with that the obvious properties of waves and particles are *incompatible*; adding them together as though they were merely *different* does not make sense. It is proper to say that a certain animal is a horse and a beast of burden, but not a horse and a cow. Furthermore, wave and particle aspects *do not exhaust the character of an electron*, although they have played dominant roles in the physical literature. Certainly "chargicle" and "wave charge" are no less appropriate terms for the electron. But there is one very basic kind of entity, different from particle, wave, and charge, which of late has not been examined for the extent to which it can reproduce the electron's behavior; it is the vortex. Certain interesting properties of charged masses (cohesion, attraction) can indeed by explained satisfactorily by assuming them to be vortices in a perfect fluid, but current fashion has passed by this intriguing analogue. For that reason and for no other the names "whorlicle" and "whorly wave" have not been suggested.

Electrons and photons are *neither* particles *nor* waves. They are no more one or the other than they are hot or cold, red or blue. And in saying this one should point out with particular emphasis that this is not an admission of ignorance. We mean to claim it as a positive fact that an electron is neither particle nor wave, and we deny that we don't know what it is. As will be seen below, the physicist has very accurate knowledge of an electron's nature; interestingly enough it is a kind of knowledge not translatable into terms like *wave*, *particle*, *red*, *blue*, *hot*, and *cold*. Why, after all, should it be thus translatable? If a visible object failed to have visible qualities, there would be cause for amazement. If an intrinsically invisible construct does not display them, consistency and orderliness seem to be furthered rather than impaired.

There is another, perhaps more interesting angle from which these facts may be viewed. Earlier (Chap. 4) we spoke of the haziness of the immediately given and showed likewise that raw perception, when properly examined, always repudiates certainty of detail. Constructional procedures alone assure stability and reflect a measure of stability upon the plane of perception. The lesson we have now learned is that the part of Nature corresponding to the constructs, electron, photon, happens to be uncertain, intrinsically uncertain, with respect to the usual sensory qualities which these terms once conveyed.

The physicist, in order to grasp reality, has had cause to heed the council: Thou shalt not make unto thee any graven image....

When this renunciation is made, there results a new attitude, freer than the old and yet circumscribed by the same unaltered methodological principles, equally rooted in confirmation, an attitude which brings fresh vigor to the solution of persistent problems. As an example of its wholesomeness we note here but one consequence of the new approach, namely, the promise it gives for a solution of the problem of *causation*, which, as we shall see later, had become cobwebbed before the attacks of mechanistic reasoning.

Suppose we *had* to think of an electron as a particle. We should then be forced to impute to it a definite shape and a definite size, precisely as was done in prequantum days. It is true that experience has never revealed these properties and hence that the theory which requires them inflates itself with inessentials. But this, one might say, is unfortunate though not destructive to the discovery of causes. Our attention is here upon a problem of far greater magnitude, however, namely upon the very feasibility of causal description. An electron of finite size *must* have internal structure which cannot forever conceal itself from the scientist's search. Such questions for instance as those concerning the forces which hold the electron's charge together, which prevent its repulsive elements from exploding, will always raise their heads. Thus the physicist, whether he wishes it or not, is ultimately committed by the mechanistically detailed framework of his conceptions to regard the particle as an infinite aggregate of points, each of which has its normal complement of six variables of state. The behavior of the electron is then determined by the motions of the infinite aggregate, and unless these motions have a special, adventitious simplicity (as they do in a rigid body, for example), every opportunity for describing the particle *completely* is sacrificed beyond retrieve, for an infinity of causal factors is untractable and self-defeating. Mechanical reasoning is always threatened by this causal catastrophe, and for that reason among others, mechanics as an ultimate mode of understanding is predestined to be inadequate.

The new theory avoids the debacle by making it unnecessary for the questions of the size, shape, and particle-wave nature of electrons to be asked, by recognizing from the beginning one cardinal fact: the uncertainty of the immediately given. In this manner it saves causality in the microcosm, and it also shows why mechanical description is valid to an extremely high degree of approximation in the large.

In sketching the need for a new and more liberal attitude we have been slightly incoherent. The only conclusion warranted by our premise so far is that elementary systems, like electrons and photons, do not have the particularized aspects of corpuscles, waves, and vortices. Coupled with this, however, is an even more drastic aspect of modern physics of which indications have already been given earlier in this book; this is the failure of such basic qualities as position in space and time to be universally meaningful. One simple example supporting this more radical thesis has been given in Sec. 3.4; in the next section we describe at greater length another of the convincing instances which force us to deny unique position to electrons.

16.4. Where Is the Electron?

The classical physicist, by an inappropriate instrumental chase, forced his quarry, the electron, to take refuge in a dual appearance: it presented itself as a particle because it had charge and mass, as a wave because it had no definite position. To understand the meaning of this last assertion it is well for us to consider the three possible experiments described below. The first is very familiar; the other two are simplified versions of processes which go on when electrons are diffracted by crystals and in microscopes.

1. Imagine a steel screen S, with a circular hole, and a gun at some distance from it trained upon the center of the hole H (Fig. 16.1). Behind the screen is a target T on which the bullet hits are registered. The hole H, of course, is large enough to permit the bullets to pass, and it will be assumed that its size can be varied. Uncontrollable influences (wind, vibrations) will cause some of the bullets to miss the hole when the gun is fired, but those which do go through in a long series of shots will form a pattern on the target screen T. Most of the bullets will lodge near the

center of the pattern, and there is a gradual decrease in the density of hits toward the periphery of the pattern, where they stop abruptly. The pattern is depicted in Fig. 16.2*a*.

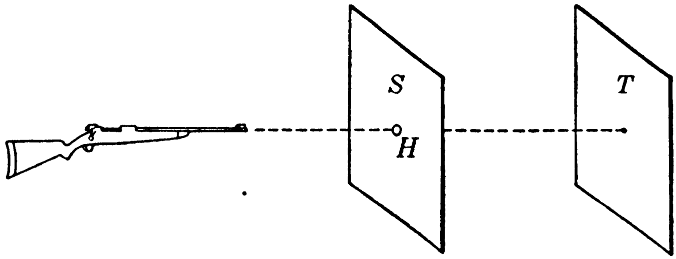
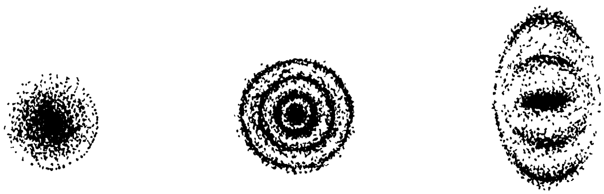


Figure 16.1

When the diameter of the hole is made smaller, the pattern is reduced in size, but the density at all points remains unchanged while the edge is rimmed off.

If another hole is made in the screen *S* immediately below the first hole and the series of shots is repeated, the pattern of Fig. 16.2*a* is *unaffected*, but a second similar pattern (with a different



(*a*) (*b*) (*c*)

Figure 16.2

density distribution, to be sure) will appear below it. All these observations receive satisfactory explanations when the bullets are thought of as particles which either do or do not go through the single hole or which, in the presence of two holes, either hit the screens or pass through one of the two holes to *T*. Here we encounter no difficulties of conception.

2. For the next experiment, we replace the gun by an electron gun, a device which accelerates electrons as an exploding cartridge accelerates a bullet. Two screens are used again, the first having a circular hole and the second serving as a target; but the sizes and spacings of all these objects are appropriately reduced.

When the gun fires away, a pattern of electron hits is observed at T, but a pattern of an altogether different sort. Instead of the monotone decrease of intensity toward the edge, one now sees the series of circular rings qualitatively depicted in Fig. 16.2*b*. Hits extend well into the region from which the edge of the hole excluded all electrons. And the density of the pattern does not end abruptly but decreases gradually to zero.

When the diameter of the hole is made smaller, the whole pattern *expands*, changing the density at all its points. This can hardly be explained by picturing the bullets as particles unless one has them bending around the edge of the hole and unless one assumes in addition that the degree of bending is greater the smaller the hole. To be sure the behavior described is typical for waves. But we now wish to take the attitude to which our previous consideration has committed us, for nothing is gained and much is lost by foolishly alleging that at one end of the experiment (in the accelerating gun) the electron is a particle and at the other end it is a wave. Let us then remain consistent and see what happens to the "particle" which left the gun. Clearly, its path has been deflected around the edge of the hole in S. Or did it have no *path*?

3. Let us repeat the second experiment with two holes in the first screen instead of one. The pattern now observed is not a juxtaposition or a superposition of two patterns like Fig. 16.2*b*, but an entirely new pattern shown in Fig. 16.2*c*. This result, perhaps innocent at first sight, is fraught with sinister meaning. An electron going through the upper hole must have modified its behavior because of the presence of the lower hole! That is the least we are forced to conclude in following out the mechanical interpretation of happenings in time and space. Already one feels uneasy and hopes he will not be called upon to explain the presumed interaction between the electron and the supernumerary hole through which it did not pass. But the trouble does not end at this point.

A comparison of the pattern 16.2*b* or 16.2*c* with 16.2*a* immediately reveals that *b* and *c* can under no condition be explained by random errors. For if that explanation were correct, a decrease in the size of the hole should reduce the size of the pattern, whereas the opposite is true. Patterns *b* and *c*, therefore, must result from some sort of connivance between the entities producing the patterns. To ascribe them to a physical interaction between the entities and the hole will not do because a single electron scores only a single hit; the pattern, however, is the result of many successive impacts. Now a connivance between the electrons which make the pattern can reasonably be imputed only so long as they have a chance to collaborate, that is, so long as they approach the hole or holes together, or at any rate in sufficiently rapid succession to have an opportunity for conniving.

To test this point we may fire the gun at considerable intervals of time, so that the fate of one electronic bullet can safely be assumed to be independent of the others. And behold, after many impacts of individual bullets on the screen, the same pattern results! If each electron did take cognizance of the hole which it avoided, how did it know through which hole its predecessors and its successors were to go or had gone? How did it achieve that correlation with the action of its precursors and successors which is manifest in the regular pattern on the target screen? At this point, rather than invoke a mysterious influence through time, one is constrained to admit that *each electron passed through both holes at once*, or that *each had some inherent disposition for forming the pattern which the whole aggregate only was able to realize.* Electron waves were invented by the model addict to avoid facing this issue.

Whatever the entity *is*, at the instant of its passing the first screen it did not have a determinate position. And we are not saying that its exact position was not or could not be observed: it had none.

At this stage we are ready to present the basic structure of quantum mechanics, the theory which joins these strange pieces of evidence in a deep organic union. Its tenets are abstract but, we believe, not difficult to comprehend when we remember that it shuns all models. Only the customary straining for mechanical models makes modern physics difficult and obscure. In our treatment little emphasis is placed on the interesting mathematics [1] in which quantum theory is clothed; our desire is to single out logical and epistemological axioms and relations for careful examination.

Summary

Chapter 16 starts with a brief summary of the recent history of atomic physics. It then proceeds to develop with care the renowned dualism (particle-wave nature) afflicting all elementary particles. First, we review the factual evidence for each one of the following four propositions:

I*a*. Light is a wave.
I*b*. Light consists of corpuscles.
II*a*. Electrons are corpuscles.
II*b*. Electrons are waves.

The meaning of the assertion that some of the properties of electrons and photons *cannot be known* is then analyzed, and we conclude that there is in fact no dualism, that electrons and photons *have no exact position* in the mechanistic, intuitable [2] sense of the word. Position has become a latent observable.

To illuminate these conclusions in all their detail the last section presents an analysis of the problem of electron diffraction which, when interpreted in any manner except that above, leads to the absurd predicament that a single particle, in a single passage from one point to another, may take two different paths.

Selective Readings

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[1] The mathematical structure of quantum mechanics is described in some of the books cited in the bibliography of Chap. 17. For a succinct and reasonably simple version see Margenau and Murphy, "The Mathematics of Physics and Chemistry," Chap. 11, D. Van Nostrand Company, Inc., New York, 1943.

[2] The word *intuitable* here (and elsewhere in this book) means conceivable or representable in terms of mechanical models, like the German word *anschaulich*.

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CHAPTER 17

Basic Ideas of Quantum Mechanics

17.1. Orientation

The terminology in this branch of physics is still strongly conditioned by its recent origin and carries an awkwardness at times resembling confusion. This makes a few introductory comments for the purpose of general orientation seem appropriate.

There is no consensus even with respect to the name of the subject we are about to treat. Sometimes it is called *matrix mechanics*, sometimes *wave mechanics*, and now more commonly *quantum mechanics*. Each of these names has a good historical warrant, and it is illuminating to recall briefly how they came to be adopted.

In 1924 Heisenberg discovered a calculus which allowed some of the properties of atomic systems to be calculated with striking success, and because this calculus involved *matrices* as major mathematical tools, the first name arose.

A little more than one year later, Schrödinger published three amazing papers in which he derived the same results by what appeared to be a totally different method of calculation. He solved an equation very much like the *wave equation* of classical physics and thus produced a theory which formed an extension (though with radical modifications) of the ideas of de Broglie. This complex of theories was called *wave mechanics*. It is well to note at once that the solutions of Schrödinger's equation are wavelike only for the simplest physical systems, taking on in general a rather complicated form which bears little resemblance to ordinary waves in space. The name "wave mechanics" is therefore not very descriptive of what goes on in the subject it designates, and the steadfast adherence to it on the part of some physicists has, perhaps, injected into an already difficult situation a misleading bias which hinders deeper understanding. We hope this will become apparent as our account goes on.

With the discoveries of Heisenberg and Schrödinger there were available two tools, matrix mechanics and wave mechanics, which performed in different ways the same function and achieved the same ends. This curious duplicity, which stood in violation of basic metaphysical principles, disappeared in a remarkable flash of illumination which allowed the whole new territory to be seen at once. For it was discovered that the matrix and the wave calculus were *isomorphic*, were two different forms of the same fundamental set of constructs, and the discovery of this equivalence exposed that basic structure to view. Isomorphic calculi are not uncommon in mathematics. One well-known isomorphism exists between two forms of geometry, ordinary and analytic. The first operates with points, lines, and angles, the second with coordinate axes and numbers; but the theorems of geometry can be demonstrated in either form.

A theory general enough to combine the methods of Heisenberg and of Schrödinger, a theory so constructed that the latter two appear as special cases under it, was advanced by Dirac and called the *q*- (quantum) number calculus. Somewhat lacking in mathematical rigor, Dirac's method was placed on a technically sounder analytic basis by von Neumann, who proved that the elements of the matrix calculus and those of wave mechanics are special forms of "operators in Hilbert space." This very general theory now carries the name of *quantum mechanics*, a name more suitable than any of the others because it is impartial and it exacts no tribute from intuition. We adopt this name.

But it is not our desire to present the subject in its most general form, as an algebra in Hilbert space. Rather, we select the special formulation which physicists have found most useful and which they employ most widely in their work. It is the theory of Schrödinger, extended to include the important contributions by Born and Jordan, contributions which made possible the statistical interpretation shortly to be discussed. The reader should understand, however, that the entire development contained in this and the following chapters could also have been written in a wholly different language involving matrices, and also in another involving points of Hilbert space, in place of our "state functions."

17.2. Operators, Eigenfunctions, and Eigenvalues

Quantum mechanics makes use of a few new ideas which have not as yet found their way into elementary mathematics courses and will therefore be discussed. Though unfamiliar and therefore perhaps awe-inspiring, they are no more difficult to comprehend than the simpler notions of the differential calculus. The reader will recall what a mathematical *function* is: The equation $y = f(x)$, to be read "y is a function of x," assigns to every value of the independent variable x a value of the dependent variable y. That equation is often written $y = y(x)$. Both independent and dependent variables may take on other names; thus $\psi(x)$ says ψ, the dependent variable, is a function of the independent one called x; $u(t)$ says u is a function of t, and so forth. Before any of these symbols has concrete meaning, the functional form of y, ψ, or u must be given, that is, it must be specified that y is a sine function, for example, or that ψ is the square. In that case $y = y(x)$ reads $y = \sin x$, and $\psi = \psi(x)$ becomes $\psi = x^2$. On numerous occasions it may be obvious from the context, or it may be unnecessary for other reasons, to specify that y is a function of x or that u is a function of t, and the symbols y and u in place of the explicit $y(x)$ and $u(t)$ may then be written.

As the next step we introduce a more unusual notion, that of an *operator*. A function, $y(x)$, can be subjected to many mathematical operations which have the effect of changing it into another function, let us say into $w(x)$. For instance, $y(x)$ can be multiplied by 2, or by x; it can be divided by some number n or some other function $f(x)$; it can be differentiated or integrated with respect to x. These operations we have just described in words are ordinarily indicated by familiar symbols, and we write them, successively, as follows: $w = 2y$, $w = xy$; $w = y/n$, $w = y/f(x)$, $w = dy/dx$, $w = \int_0^x y\,dx$. Now it is advantageous to introduce one special symbol for the class of all such operations as convert a function $y(x)$ into some other function $w(x)$. That symbol is Q, and we abbreviate the foregoing equations to the single one $w(x) = Qy(x)$. The symbol Q (and the construct for which it stands) are called an "operator."

A very important kind of operator is the differential operator d/dx. If Q is identified with it, the equation $w = Qy$ takes the form $w = dy/dx$. Another differential operator is $Q = d^2/dx^2$, the symbol denoting the second derivative. A very general differential operator is a linear combination of all derivatives,

$$Q = a_0 + a_1 \frac{d}{dx} + a_2 \frac{d^2}{dx^2} + \cdots$$

in which the quantities $a_0, a_1, a_2, \ldots$ are functions of x. In the following, our attention will be confined to differential operators of the first (d/dx) and second (d^2/dx^2) order, that is, to symbols like the foregoing Q but with a_3, a_4, and all higher a's equated to zero. There will also be occasion to use the simple operator, multiplication by x, which will be indicated by $x\cdot$ when it must be explicitly written.

It is clear that operators are not functions themselves; indeed an operator symbol such as Q is like a transitive verb without an object. The object is supplied by the function upon which the operator acts. This does not mean, of course, that the operator Q alone is devoid of *meaning*: the operator "to see" has meaning, though less specific meaning than "to see John."

Let Q be some given operator, $\varphi(x)$ some given function. The new function $Q\varphi$ is then also defined. Now it is interesting to compare $Q\varphi$ with φ. In general $Q\varphi$ and φ will be entirely different functions, but it is conceivable that they have similar properties. It might even happen that the two are identical, as would be the case, for instance, if φ were $\sin x$ and Q were $-d^2/dx^2$. The mathematician has long been fascinated by situations in which φ and $Q\varphi$ are *proportional* to each other. When this is true, $Q\varphi$ is some number (let us say q) [1] times φ, and the equation

$$Q\varphi = q\varphi \tag{17.1}$$

represents this state of affairs. But the problem is not usually to

[1] This symbol q does not represent a quantum number.

find an operator Q which will satisfy this equation *for a given function* φ. The typical situation in quantum mechanics is *to find a* φ *that will satisfy Eq.* (17.1) *when* Q *is specified.*

Thus, assume an operator Q to be given, but let nothing be said about the function $\varphi(x)$. Is it possible to choose a function φ so that Eq. (17.1) will be satisfied? For some operators, notably the differential operators already mentioned and for the operator $x\cdot$, the answer is affirmative. Methods whereby such functions can be found are well known and need not trouble us here. That function $\varphi(x)$ which satisfies Eq. (17.1) for a given Q, in other words, that function $\varphi(x)$ which under the operation Q converts itself into a constant multiple of its former self, will for definiteness be called $\psi(x)$. Thus, among all possible functions $\varphi(x)$ to which Q might be applied, there are some special ones, here named $\psi(x)$, which satisfy

$$Q\psi(x) = q\psi(x) \tag{17.2}$$

What the constant q turns out to be is in general not under our control.

The function ψ is called an *eigenfunction* of the operator Q, and the number q is called the *eigenvalue* of Q. Examples of eigenvalue equations and their solutions will be found in the next sections.[1] It is to be emphasized that the only symbol which is known or preassigned in Eq. (17.2) is the operator Q. Knowing it one can usually construct: (*a*) a set of q's; (*b*) a set of ψ's. There is nothing strange in the fact that one mathematical quantity

[1] To take the mystery away from Eq. (17.2) we consider an example at once. If Q is the differential operator of second order d^2/dx^2, that equation reads

$$\frac{d^2\psi}{dx^2} = q\psi$$

It is an ordinary differential equation whose solution is well known to be

$$\psi = c_1e^{\sqrt{q}x} + c_2e^{-\sqrt{q}x}$$

This represents an eigenfunction of d^2/dx^2. We note that q may have any value we wish to give it. (As will be seen later, this is not always true.) Hence we may say that the eigenvalues of this operator are *unlimited* or, as will also be made clear subsequently, *unquantized.* Note, however, that the eigenfunction ψ depends on q parametrically. To be more definite, therefore, we should indicate this dependence explicitly and write $\psi_q(x)$ in place of ψ.

is able to create, as it were, two sets of others. We shall express this state of affairs by saying: An operator *generates* a set of eigenvalues and a set of eigenfunctions. Every eigenfunction is associated with, or belongs to, some specific eigenvalue.

17.3. Axiom 1. Observables and Operators

We have seen in Chaps. 9 to 11 how every exact scientific discipline proceeds: it defines its systems, selects observables for these systems, and combines a significant set of these observables into states. The laws of the discipline regulate or restrict the choice of states. In classical mechanics all this is relatively simple, for its systems are of the picturable type, and its observables are of the ordinary, possessed variety. The position of a mass is a *quantity*, uniquely possessed at every instant, and can be represented suitably by a number. Since it changes, the number must be allowed to change: The observable, *e.g.*, position, becomes a function of the time or of other variables which change in time. Clearly, then, the mathematical *function* is the construct which more or less obviously corresponds to an ordinary (possessed) observable. That is why, in classical mechanics, the *function* $x(t)$ corresponds to position, the *function* $m \cdot v(t)$ to the momentum, the *function* $\frac{1}{2}mv^2$ to the kinetic energy of a particle, and so on. Each of these *has* a specific value at a given time.

The systems of quantum mechanics (electrons, protons, photons, atoms, nuclei), while customarily thought of as sharing the intuitable attributes of mechanics in the large, do not actually possess them, as we now know. It is true, such properties as position, momentum, energy, are observables with respect to these systems, but they are not *uniquely* observable in a single act of looking. They are what we have previously called latent observables, observables having relevance to the system, to be sure, but not revealing themselves as directly and as consistently as possessed observables do. Recall the electron which, when classical methods were used to describe its motion, committed the atrocity of going through both holes at once! It made no sense to speak of its position in a single passage. If for that reason we should *completely* abandon the idea of the electron's being in space, we should ignore something very valuable, namely, the fact that there was after all an observable pattern on the screen. In this way the electron, while retaining in a looser sense its spatial existence, manifests the latent character of the observable, position. That observable is now something which can have different values in different observations, but still has a sufficient internal stability to produce a unique Kollektiv of values in a sequence of observations. A latent observable scatters [1] when repeatedly observed, but it has a determinable probability distribution. Quantum mechanics is different from classical physics because it is forced to deal with latent observables.

Functions, we have seen, are satisfactory for the treatment of ordinary quantities. What mathematical construct is suited to serve in the treatment of latent observables? Since every observation yields a number, the object of our search must also produce numbers; but it need not itself be that serial arrangement of numbers which we call a function. Now we have seen that an operator, which is not a function, generates numbers called eigenvalues. Formally, then, the suitability of operators for representing latent observables is apparent. Quantum mechanics has seized upon it and has developed this opportunity into a tremendous success.

In the following, we abandon sequential reasoning and present the basis of that science in axiomatic [2] form, as the way for such an account has now been cleared.

The first axiom states: *To every observable there corresponds an operator.*

It is a task of science to make a detailed assignment of operators to observables. This, of course, cannot be done with the use of the present axiom alone but requires for its achievement the entire theory that follows. Nevertheless it is well to indicate at once, in anticipation of further developments, what some of these operators are. They will seem strange at this point and doubtless will induce

[1] We shall see later that there are special conditions (eigenstates), under which even a latent observable produces the same value in repeated observations. This, of course, is no more surprising than that probabilities can take on the limiting value 1.

[2] The word *axiom* is here used in a rather general sense as meaning any theoretical proposition of physical or mathematical content which cannot be derived from other propositions. Its use may at times be displeasing to the expert logician.

a strong desire on the part of the reader to know where they come from. But we shall merely say here that they are initially postulated in the same way as functions are postulated in the older branches of physics, and are then verified in much the same manner. For whence, after all, do we have the knowledge that kinetic energy equals $\frac{1}{2}mv^2$ if not from postulation and subsequent verification?

Three observables will be of particular interest. They are listed in Table 17.1, together with the operators which correspond to them and the symbols which will be used as abbreviations for the operators. The last column contains the familiar classical functions used for the observables in ordinary mechanics.

Table 17.1

Observable	Operator	Symbol	Customary function
Position............	$x\cdot$	X	x
Momentum........	$-ik\frac{d}{dx}$	P	$m\frac{dx}{dt}$
Kinetic energy......	$-\frac{k^2}{2m}\frac{d^2}{dx^2}$	H	$\frac{1}{2}m\left(\frac{dx}{dt}\right)^2$

The constant k appearing in the momentum and energy operators is Planck's constant h divided by 2π; m is the mass of the moving system. The theory which is being developed is applicable to all entities which are ordinarily thought of as particles (electrons, protons, moving atoms, molecules, and stones), but we shall for the present focus our attention upon *electrons*. In what follows, m will therefore denote the mass of an electron. The appearance of i, which is $\sqrt{-1}$, is perhaps unexpected here but will cause no difficulties. In fact the presence of i is a comfort to the mathematician, for without it the momentum operator would not have the formal properties which make it useful. (It would not be "hermitean" and would not always possess real eigenvalues.)

[At this point we face the question: Are all observables in the microcosm of the latent type? The answer reveals a strange asymmetry in the present quantum theory and suggests a defect which future developments are likely to remove. For the answer is that there are *some* observables, including those listed above, which are latent and *others* which are not, although no good reason can be given for the distinction. Charge and mass of an electron belong to the latter class; they are often regarded, not as proper observables but as parameters appearing in the equations of quantum mechanics. This manner of speech does not alter the philosophical situation: charge and mass of an electron are observables in the same sense as energy and spin, and the division is unsatisfactory. I believe it probable that a future quantum theory will treat all observables as normally latent ones.]

17.4. Axiom 2. States and Functions

In classical mechanics, a state is a collection of values for a sufficient number of observables, *e.g.*, position and momentum when the system is a particle or mass point. Since in the new theory these observables do not "have" values, a state must be redefined.

This can be done in two equivalent ways. The first represents a minimum departure from classical methods but is a little difficult to grasp. Since we shall not adopt it as our route, it need only be mentioned here for the sake of completeness. It happens that, when a system is in a given condition, there are always *some* observables which *do* yield consistent values in repeated measurements and therefore, in that particular state of the system, are not latent, though all others are. Henceforth we shall speak of such normally latent observables, for which however a special state exhibits unique values, as *having sharp values* or simply as being sharp. Now a state can be defined by naming all observables which are sharp and by then listing their values. This is the first method referred to. As was said, it is entirely equivalent to a second one which is more powerful in application and, although more abstract, nevertheless easier to understand because it is closely aligned with the basic philosophic attitude of quantum mechanics.

The second method is based on axiom 2, which asserts:

To every state of a physical system there corresponds a function, called a state function, $\varphi(x)$.

If this axiom is to be meaningful, one must be able to predict with its use the statistical results of observations on all possible observables exhibited by the system, and predict them from a knowledge of $\varphi(x)$. In other words, $\varphi(x)$ must hold the key to everything which can possibly be known empirically, that is, by performing measurements, about the system in this state. The following axioms will show that this is true. Axiom 2 establishes a *rule of correspondence* between a partial state and φ but does not complete the chain of meaning, since it leaves $\varphi(x)$ without significance at this point by failing to indicate how φ shall be confirmed. This gap also will have to be filled.

Equally vague at this stage is the symbol x, the independent variable appearing as argument of the state function. As we shall see, its interpretation does not really matter, but it is well to think of it as a positional coordinate in the usual sense and to think of φ as a curve constructed above the x-axis. In general, φ has more variables than one, in fact as many as the system when visualized classically has degrees of freedom. But we shall choose examples which allow us to get along with a single variable x.

The first two axioms establish a sort of formal medium, created for its prospects of success in describing the epistemological situation of atomic experience but devoid of material content. This is supplied by axioms 3 and 4.

17.5. Axiom 3. The Empirical Meaning of Eigenvalues

In classical mechanics it was assumed without question that all values of a physical observable are intrinsically possible. A mass particle could be found anywhere, with any momentum ($-\infty < p < \infty$) and hence with any energy. There was one minor restriction, to be sure: the kinetic energy could not take on negative values; but this was automatically ensured by its formula, which contains only positive quantities. Atomic systems, however, show the phenomenon of quantization; they exercise a peculiar selectivity with respect to the values which certain observables, notably the energy, can take on. Quantum mechanics accounts for this contingency in a beautiful and natural way by means of its third axiom, which asserts:

The only values which measurements of an observable can yield are the eigenvalues of its operator.

Let us see what this postulate implies with respect to the permitted, or measurable, values of the three observables listed in Table 17.1. There is a certain advantage in starting at the bottom of that list and in considering the energy observable first. If the symbol E is used to denote the eigenvalues of the operator H, the equation to be solved is $H\psi = E\psi$ or, explicitly,

$$-\frac{k^2}{2m}\frac{d^2\psi}{dx^2} = E\psi \tag{17.3}$$

Its solution is familiar from first-year calculus or physics; the equation is formally identical with that which in classical physics describes the motion of a simple harmonic oscillator,

$$-M\frac{d^2y}{dt^2} = Ky \tag{17.4}$$

$$K = \text{constant} > 0$$

Since Eq. (17.4) has the solution $y = c \sin(\sqrt{(K/M)}t + \delta)$, where c and δ are constants of integration, Eq. (17.3) must yield

$$\psi_E = c \sin\left(\frac{\sqrt{2mE}}{k}x + \delta\right) \tag{17.5}$$

Every function of this form is an eigenfunction of H, regardless of the value of E. We note that, for *every* E, a corresponding eigenfunction ψ can be constructed. Hence every value of E is an eigenvalue of H and therefore permitted; quantization is absent in this case. This result is in agreement with observation: a *free* electron (which is what our theory here describes) can possess any value of E.[1]

[1] There is of course the trivial condition that $E \geq 0$, in agreement with classical physics.

The result is wholly different, however, when the particle is not free. Assume it to be confined to a range of length l, so that $0 \leq x \leq l$. The quantum mechanical transcription of this condition is that the system possesses no state outside these limits for x, that is, ψ_E vanishes outside the range. Analytically, this can be brought about only by letting ψ_E be zero at $x = 0$ and at $x = l$. Under these circumstances Eq. (17.5) is still a solution of (17.3), but it does not satisfy our boundary conditions. To enforce them, special values need to be assigned to the parameters δ and E.

Clearly, if $\psi_E(0)$ is to vanish, δ must be put equal to zero. Second, if $\psi_E(l) = 0$, we must put $\sqrt{2mE}\, l/k = n\pi$ where n is an integer, for only sin $n\pi$ is zero for every n. But here we see the emergence of quantization, for when the last relation is solved for E, we have

$$E = n^2 \frac{\pi^2 k^2}{2ml^2} \tag{17.6}$$

Only discrete values of E, namely, $\pi^2k^2/2ml^2$, $4(\pi^2k^2/2ml^2)$, $9(\pi^2k^2/2ml^2)$, etc., are thus predicted and are actually found to occur. The successive values of E are called *energy levels*.

Why has quantization eluded the scientist until so recently? Equation (17.6) holds the answer. Only if $\pi^2k^2/2m$ is large enough, *i.e.*, if m is small enough, is the separation between successive energy levels appreciable. For ordinary masses, m is so large that quantization went undetected. But for atomic masses, above all for the electron with its m equal to 10^{-29} gm, the separation between levels became evident and important.

Having dealt with H, we shall now consider the operator P. Its eigenvalue equation reads

$$-ik\frac{d\psi}{dx} = p\psi \tag{17.7}$$

provided that we use the symbol p for the eigenvalues. Equation (17.7) is immediately integrable and has the solution [1]

$$\psi = ce^{ipx/k} \tag{17.8}$$

[1] By the symbol c we represent in this chapter a constant, but not the same constant in different equations.

The solution is proper for all real values of p, positive and negative, and we conclude that measurements on the momentum of a free particle can yield all possible values and disclose no quantization. Experiment confirms this fact. Again, with every eigenvalue p there is associated an eigenfunction, namely, that given by Eq. (17.8), which shall henceforth be labeled ψ_p to avoid confusion with eigenfunctions belonging to other operators.

Finally, we examine the operator X. It leads us into some unfamiliar but very simple algebra. The eigenvalues of X will be called a; they represent the position of the particle as measured relative to some origin. Table 1 shows that we must solve

$$x \cdot \psi = a\psi \tag{17.9}$$

a simple equation which seemingly has the solution $x = a$. But let us not commit the cardinal sin of algebra, division by zero: ψ may not be divided out from Eq. (17.9) when ψ is 0, and it may well be 0 for certain values of x. Proceeding more carefully, then, we write the equation in this form:

$$(x - a)\psi = 0$$

and interpret it as follows: One of the factors on the left must be zero. Hence either $x = a$ or $\psi = 0$. Thus we see that the function $\psi(x)$ is zero everywhere except at the point $x = a$, where it may have any value and still satisfy our equation. Rigorously, ψ is not a function at all but can be approximated closely enough by a function, drawn in Fig. 17.1, which is zero at all points except in the immediate neighborhood of a. At this point it takes on very large values. Dirac has introduced the name *delta function* for it, and its usual abbreviation is $\delta(x, a)$. We observe therefore that the eigenfunction of the operator X belonging to the eigenvalue a is

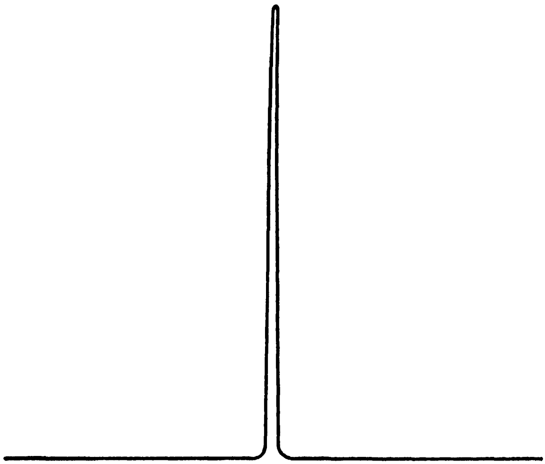
$$\psi_a = \delta(x, a) \tag{17.10}$$

Again, quantization is absent since all values of a are permitted by Eq. (17.9). Summarizing, we have found that for the unrestricted free particle[1] none of the three observables here con-

[1] We now understand, of course, that the word *particle* no longer has its narrow mechanistic meaning.

sidered has quantized values. This does not imply classical behavior in all other respects; in particular it does not alter the latent character of these observables.

In many other instances quantization occurs, as it did in the example of our electron restricted to be in a finite range l. Physically, instances of quantization are far more interesting than the



Dirac's δ-function, $\delta(x, a)$

Figure 17.1

simple cases treated here; they are generally regarded as characteristic of the new discipline and have supplied its name. Yet it will be seen that in a deeper sense quantization is merely a by-product of a new basic method for describing physical experience. The best-known physical systems exhibiting quantized energies are the simple harmonic oscillator, the rotator, all atoms, molecules, and crystals. Furthermore, all angular momenta have quantized values. If we omit these from consideration here it is because they add but little to the fundamental matters at issue, while the results above are needed in our studies ahead.

In deriving the *eigenvalues* for the energy, the momentum and the position of a free electron it was necessary to find the *eigenfunctions* as well. Their meaning is still obscure at this point since it is conveyed by the next axiom on our list. But to have all the present information readily at hand we shall collect it in Table 17.2.

Table 17.2

Observable	Eigenvalue	Eigenfunction
Position	a, unquantized	$\psi_a = \delta(x, a)$
Momentum	p, unquantized	$\psi_p = ce^{ipx/k}$
Energy	E, unquantized	$\psi_E = c \sin\left(\frac{\sqrt{2mE}}{k} x + \delta\right)$

17.6. Axiom 4. The Empirical Meaning of States and Eigenstates

Assume that we are given a system, *e.g.*, an electron, and that we know it to be in a state $\varphi(x)$. What can we conclude as to its behavior? First it is well to clarify the meaning of the term *behavior*, which is less obvious here than for systems presenting clean, possessed observables. To sum it up, *the behavior of an atomic system is the totality of its reactions to physical operations.* But physical operations designed to elicit quantitative reactions are called measurements. Hence the question just asked can also be phrased: What can measurements reveal concerning a system in the state $\varphi(x)$?

From the point of view now established an observable is not so much an attribute of the system as it is an entity determined by the physical operation to be performed. Energy, for example, is not something which the system carries but something called forth or, if one wishes to carry this view to the limit, emerging in the act of exposing the system to a designed set of circumstances. When this is understood, the latent nature of observables, their scattering of values on measurement, ceases to be paradoxical. The reader may now see more clearly why the modern physicist can no longer countenance simple realism and why the elaborate epistemology of this book is required.

Since measurement does not fix the value of an observable with certainty (if the measurement were repeated when the system is in exactly the same state, the same outcome is not to be expected), only a probability can be established. Now suppose that the operator corresponding to the observable about to be measured is Q and that its eigenvalues are q. We wish to know the probability $W(q)$ that, when the measurement is performed, the result be q, the system being in the state $\varphi(x)$. The fourth axiom provides this information; it says:

Let $\psi_q(x)$ be the eigenfunction belonging to the eigenvalue q of Q. Compute

$$b(q) = \int_{-\infty}^{\infty} \varphi(x)\psi_q(x)\,dx \tag{17.11}$$

Then the probability

$$W(q) = b^2(q) \tag{17.12}$$

This looks complicated indeed and seems to have the design of a cookbook recipe. Nevertheless it is a natural and very elegant rule when considered as a theorem in Hilbert space.[1]

Before applying the axiom a word must be said concerning $W(q)$. In what sense is it a probability? What is the aggregate, and what are its elements? It happens that quantum mechanics deals successfully with two kinds of aggregates. The first is formed from repeated measurements upon the *same* system, always similarly prepared before observation so that it can safely be assumed

[1] Equations (17.11) and (17.12) are not quite general, and we shall later need them in their general form. The detail to be added here should not distress the reader who looks upon complex functions with abhorrence, for it does not alter the logic of our development. The functions φ and ψ_q may indeed be complex. When that is the case, Eq. (17.11) must be replaced by

$$b(q) = \int \varphi^*(x)\psi_q(x)\,dx \tag{17.11'}$$

and Eq. (17.12) by

$$W(q) = b^*(q)b(q) \tag{17.12'}$$

A star denotes the complex-conjugate function. The mathematical reader sees that (17.12′) is necessary to make the probability *real*; (17.11′) is required for consistency.

to be in the same state $\varphi(x)$.[1] The second kind of aggregate consists of a large number of simultaneous observations on a collection of similar systems, all of which are in the same state. The latter type of aggregate is more commonly encountered because it is easier to produce, almost every observation on atomic systems involving numerous specimens. It should be emphasized, however, that the first kind of Kollektiv is equally proper and that the probability features are not introduced into quantum mechanics by the spatial interplay of many specimens, as they are in statistical mechanics. The diffraction of electrons by holes in a screen, which was discussed at the end of the last chapter, should be remembered at this point.

To render more definite the significance and the utility of axiom 4 we apply it to the three operators we have selected for study. Our system is a free particle in a state $\varphi(x)$. What is the probability $W(a)$ that we shall find it at the point $x = a$? To the observable, position, there corresponds the operator X, and according to Table 17.2 its eigenfunctions ψ_a are $\delta(x, a)$. Therefore

$$b(a) = \int_{-\infty}^{\infty} \varphi(x)\, \delta(x, a)\, dx$$

An evaluation of this integral is very easy because of the simple properties of the delta function. The integrand is zero everywhere except at $x = a$. At this point $\varphi(x)$ has the value $\varphi(a)$, and $\delta(x,a)$ has some constant value. In consequence, $b(a)$ is simply $c\varphi(a)$.[2] Using Eq. (17.12), we find for the probability

$$W(a) = \text{const}[\varphi(a)]^2 \tag{17.13}$$

and this is a most important result. For it says that *the state function $\varphi(x)$, after squaring, represents the probability of finding the particle at the point indicated by its argument.* Thus is provided

[1] Whether one can ever be sure in quantum mechanics that the same state has been prepared is a question frequently discussed. See J. von Neumann, "Mathematische Grundlagen der Quantenmechanik," Springer-Verlag, Berlin, 1932; A. E. Ruark, *Phys. Rev.*, 48:466 (1935); E. Schrödinger, *Proc. Camb. Phil. Soc.*, 31:55 (1935); H. Margenau, *Phil. Sci.*, 4:337 (1937), and other references given in the last citation.

[2] The quantity b is often called a "probability amplitude."

a useful picture for the meaning of the state function. When $\varphi(x)$ is given, we know at once that $[\varphi(x)]^2$ is proportional to the probability that the particle be "located"[1] at x. In some of the simpler treatments of the subject this interpretation of the state function is rendered explicit by a special axiom of its own. As the present derivation shows, it is but a subcase of axiom 4.

In considering momentum measurements, nothing is gained by proceeding with fullest generality. Let us therefore assume the

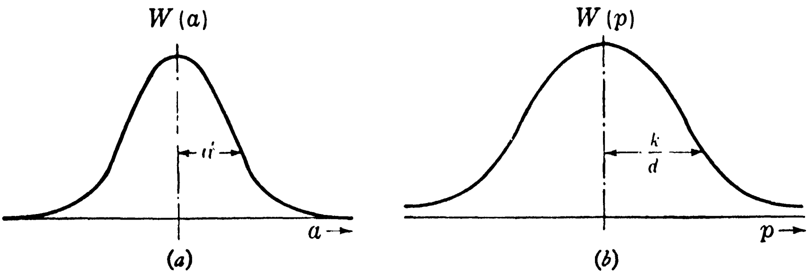


Figure 17.2

state $\varphi(x)$ to have a special form. The Gauss function happens to be easy to treat and the results obtained with its use are interesting to interpret, for they lead directly to the uncertainty principle. Hence we take

$$\varphi(x) = e^{-\frac{1}{2}(x/d)^2} \tag{17.14}$$

This state corresponds to a probability distribution $W(a) = ce^{-(a/d)^2}$, which is plotted in Fig. 17.2*a*. The quantity d is a measure of the width of the distribution (it represents one-half the width of the curve at one *e*th of its maximum ordinate) and has been drawn in the figure. As is seen, d is also a measure of the uncertainty in position which inheres in the state $\varphi(x)$. At the risk of annoying the reader by repetition we remark once more that the curve in Fig. 17.2*a* presents the statistical results of measurements on the position of a particle in state $\varphi(x)$ as given

[1] Here and elsewhere we use a term which, in its common interpretation, contradicts what has been said about the latency of the particle's position. Physicists continue thus to employ "classical" words in a new setting, preferring the danger of misinterpretation of what they say to the laborious language that would be needed for exact expression.

by (17.14): most of the measurements yield $a \cong 0$; the others are spread symmetrically about this value.

Now we ask about the results obtained or to be expected when the *momentum* of the particle is measured while the particle is in the same state φ, given by Eq. (17.14). On looking up the eigenfunction ψ_p in Table 17.2 and using it in Eq. (17.11), we calculate

$$b(p) = \int_{-\infty}^{\infty} e^{-\frac{1}{2}(x/d)^2} \cdot c e^{ipx/k} \, dx$$

In tables of integrals one finds the following formula:

$$\int_{-\infty}^{\infty} e^{-\lambda x^2 + i\mu x} \, dx = \sqrt{\frac{\pi}{\lambda}} \, e^{-(\mu^2/4\lambda)}$$

Thus, after identification of constants, we see that

$$b(p) = \text{const}\, e^{-\frac{1}{2}\left(\frac{d}{k} p\right)^2}$$

and by Eq. (17.12)

$$W(p) = c e^{-\left(\frac{d}{k} p\right)^2} \tag{17.15}$$

This formula represents the probability of measuring a specified momentum, p, and it is plotted in Fig. 17.2*b*, where the width of the distribution, k/d, is also indicated. Measured values are scattered symmetrically about the point $p = 0$, which is the most probable of them.

The fact that a Gaussian curve in a yields also a Gaussian curve in p is accidental but welcome, since it allows widths of curves, *i.e.*, uncertainties, to be expressed in a similar way in both distributions. In general, $W(p)$ does not have the same mathematical form as $W(a)$. Despite this fact the two are always correlated in a unique way which is exemplified by the equations just derived, and we shall have important cause to return to them for further examination (Sec. 18.1).

To complete the job at hand, the energy observable requires consideration. There is thus left the problem of calculating $W(E)$ in accordance with Eqs. (17.11) and (17.12) when $\varphi(x)$ is given by (17.14). However its solution will teach us nothing new; $W(E)$ has a bell-shaped form very similar to Fig. 17.2*b*. We therefore reserve its calculation as an exercise for the interested reader.

The empirical meaning of an arbitrary state, $\varphi(x)$, has now gradually unfolded itself, albeit in an abstract and perhaps an unfamiliar way. But what are the so-called *eigenstates* which entered the scene at every turn? Do they have some special physical significance, or are they merely tools for the calculation of probabilities needed in Eq. (17.11)? They will now be seen to have a very interesting significance, and this will expose itself if we apply axiom 4 to a state $\varphi(x)$ which is so chosen as to be an eigenstate.

Assume, then, that $\varphi(x) = \psi_{q'}(x)$, to take an example. It is necessary here to add a prime to the symbol q in order to indicate its reference to a special, single value q' to which the function $\psi_{q'}$ belongs. According to Eq. (17.11)

$$b(q) = \int_{-\infty}^{\infty} \psi_{q'}(x)\psi_q(x)\, dx$$

This integral cannot be evaluated by elementary means, but according to a fundamental theorem of analysis (which will not be proved here) it yields a strange but simple result: it is zero whenever q' is different from q; when $q' = q$ the integral has the value 1.

Since the square of 0 is 0 and the square of 1 is 1, $W(q)$ is identical with $b(q)$. Therefore

$$W(q) = \begin{cases} 0, \text{ if } q \neq q' \\ 1, \text{ if } q = q' \end{cases}$$

In words: If the system is in an eigenstate of the operator Q, namely, the eigenstate belonging to the eigenvalue q', then the probability of finding for q the particular value q' is unity, the probability of not finding that value upon observation is zero. An eigenstate is thus revealed as a state in which *one* observable does not scatter, a state in which a normally latent observable is *possessed* and acts like an ordinary quantity of classical physics. The eigenstate, as we shall see, is sharp with respect to that observable.[1]

[1] In general, when states are functions of more than one variable x, an eigenstate is sharp with respect to several observables.

The eigenstate $\psi_a(x)$ of Eq. (17.10) presents this character very clearly. The delta function depicted in Fig. 17.1 shows that the particle will certainly be found at $x = a$.

Perhaps it is well, at this point, to summarize the rather extensive information which has been drawn from axiom 4.

a. The square of the state function $\varphi(x)$ always represents the probability of finding [1] the particle at x when a position measurement is performed.

b. From the distribution-in-position [more accurately, from $\varphi(x)$] the probability distributions characterizing measured results on all other observables can be found by analytic procedures [Eqs. (17.11) and (17.12)].

c. An eigenstate of an operator is a state which entails no scattering of measurements performed on the observable belonging to that operator. One and the same value will always occur in repetitions of that type of measurement so long as an eigenstate persists.

17.7. Generalizations

So far as the logical structure of quantum mechanics is concerned the preceding developments were very general. Restrictions were made, however, in the applications of the four axioms, and it is proper to indicate now in final appraisal where generality has been sacrificed.

First, our analysis was carried through with reference to three observables only. Quantum mechanics knows and uses well-established operators for six different observables, though some of these operators have a rather unfamiliar form. The fundamental method of quantum mechanics as conveyed by the four preceding axioms has been carried into fields other than mechanics with some success. But the question as to the representation of certain observables, such as the electric- and magnetic-field strengths of electrodynamics, has not yet received a wholly satisfactory answer.

The converse question whether, for every conceivable mathematical operator there exists a physical observable, has at times been raised; but that question is hardly more interesting than its

[1] See footnote, p. 346.

classical counterpart in some such form as this: Does every conceivable mathematical function of the dynamical variables define a physical quantity? The best answer is that it does, but that the quantity may not be one of scientific interest. For instance, the combination of variables, $mx^{3/2}$, is certainly a meaningful physical quantity having definite values at definite times and, for that matter, having reality. However, the physicist has not dignified it by giving it a name, in obvious contradistinction to the quantity $\frac{1}{2}mv^2$, which he calls kinetic energy. The reason is the lack of significance of the quantity $mx^{3/2}$ in his established system of constructs.

The question as to the physical meaning of an arbitrarily constructed mathematical operator has a similar answer.

As to *systems*, their variety is great indeed. Our applications were limited to the simplest one, the free mass point, as exemplified by the free electron. The electron may, however, be bound to other particles by forces as it is in atoms and in molecules. In such cases the operators X and P remain unchanged; their measurable values remain unquantized. But the energy will consist of two parts, the kinetic energy previously denoted by H and an additional term for the potential energy. Also, if the particle has no restrictions placed upon its motion—has three degrees of freedom in classical parlance—H has a more complicated form in which the symbol d^2/dx^2 is replaced by ∇^2. These details, however, do not affect the axioms stated or any part of the logical structure presented here.

An important change in the formalism occurs with systems containing more than one "particle." [1] The state function then involves more than three space variables and cannot be mapped or visualized in ordinary space. But the many-dimensional spaces which are familiar from statistical mechanics lend themselves readily for use in the description of many-particle systems, and no difficulties are encountered. Here it is found that the four axioms developed in this chapter are incomplete and in need of supplementation by a requirement concerning the *symmetries*

[1] Physicists continue to use classical language because of its directness even if it is inaccurate. See footnote, p. 346.

(cf. Chap. 20) of the state function. These requirements are meaningless for single particles and are therefore excluded from our present study.

The most drastic defect of our account thus far lies in its confinement to states which do not change in time. We have assumed the state function to be $\varphi(x)$, not $\varphi(x, t)$; that is to say, we have considered states at some particular instant. In general the state function changes as time goes on.

If a similar restriction were made in classical mechanics, consideration would be limited to the subject of *statics*, and *dynamics* would be left out of account. Thus, in the strictest sense, the four preceding axioms define the field of *quantum statics* and do not consider *quantum dynamics*. To deal with this second, larger field, an "equation of motion" similar to Newton's second law must be introduced, and this is to be done in the following section. Axiom 5 enlarges quantum statics into the more embracive discipline of quantum mechanics by adding the subject of quantum dynamics. These comments suffice to outline terminology and to provide general orientation.

Before starting on the last leg of our present journey, we add a word of encouragement to the reader who sees before him another difficult and lengthy task. In classical physics, when the subject of statics is dealt with, most of mechanics is still ahead. In quantum mechanics the division of subject matter is totally different. Quantum statics is by far the larger and the more thoroughly developed part of the new mechanics. It is the subject of *stationary states*, and it happens that a stationary state in quantum theory corresponds, as a rule, to a dynamic state in classical physics; it corresponds in fact to a state in which the forces are not functions of the time. Every periodic motion of classical physics, properly studied in ordinary dynamics, has as its quantum counterpart a stationary state whose study belongs to quantum statics. The states we have considered in the preceding sections were stationary ones although their analogues in classical physics involve *moving* electrons. The last axiom to be studied therefore covers a smaller ground than might have been expected, and its exposition can be brief.

17.8. Axiom 5. Quantum Dynamics

In the present section it is understood that all preceding axioms remain intact. The meaning of states and operators is unchanged, but the energy operator H, defined for the free electron in Table 17.1, is of special importance here.

We ask this question: Suppose the state of the electron is now $\varphi(x)$; what will it be at some later time? The question forces us to include time (t) as a second independent variable among the arguments of φ. But Eqs. (17.11) and (17.12) are unaltered, the only effect upon them being that $b(q)$ and $W(q)$ are now functions also of the time. In particular, the probability of finding the electron at x, still given by $[\varphi]^2$, will change in time since φ is a function of the time. If we think of this probability distribution as some sort of condensation or a cloud in space, this cloud *diffuses* as t increases.

Axiom 5 regulates the change of φ in time. It says:

If φ is given at any one instant, its value at any other time is the solution of the equation

$$H\varphi = ik\frac{\partial\varphi}{\partial t} \tag{17.16}$$

Here H is meant to be the differential operator previously introduced and k again the constant $h/2\pi$. Equation (17.16), also discovered by Schrödinger, will be called the Schrödinger equation involving the time or, briefly, the *Schrödinger time equation.* It has already been mentioned in Sec. 17.10.

First let it be noted that its consequences are no different from those already discussed in all instances of quantum statics. The latter subject may now be defined as including all problems in which the operator H is static, *i.e.*, does not depend on t. When that is true, one need merely put

$$\varphi = \psi e^{-i(E/k)t} \tag{17.17}$$

in Eq. (17.16), with ψ independent of t, in order to have it reduce to Eq. (17.3). It is also readily seen that Eqs. (17.11) and (17.12), which must now be used in the forms (17.11′) and (17.12′), yield the same result for $W(q)$ whether we introduce ψ or $\psi e^{-i(E/k)t}$ for the state function $\varphi(x)$. Hence, so far as all observable results are concerned, Eq. (17.16) enforces no innovations when the energy operator H is constant in time.

But H does depend on t when the forces acting on the system are functions of the time (when, in classical parlance, the system is nonconservative). In physics this case is interesting and important; it leads straight to the modern theories of radiation. But we omit it from consideration because it adds little to those basic matters which are here at issue.

Let us return, then, for one final look at the situation previously studied, the case where H is constant in time. It is interesting to see whether an arbitrary function $\varphi(x)$, in which all dependence on t has been suppressed, is stationary in the sense that, once realized, it will remain unchanged in time.[1] Careful inspection of Eq. (17.16) shows this to be impossible, and the example which follows will present the facts with some interesting detail.

In Sec. 17.6, Eq. (17.14), we assumed for the sake of argument that $\varphi(x)$ had the form of a Gauss curve of width d. It is not difficult to see whether this Gaussian probability distribution remains unchanged in time. For that purpose we merely substitute $\varphi = e^{-\frac{1}{2}(x/d)^2}$ into Eq. (17.16) as the solution valid at $t = 0$ and compute by standard methods the solution for any t.[2] It turns out to be

$$\varphi(x, t) = \left(1 + \frac{ikt}{md^2}\right)^{-\frac{1}{2}} \exp \left[\frac{-x^2}{2\left(d^2 + \dfrac{ikt}{m}\right)}\right] \qquad (17.18)$$

if m is the mass of the particle.

The reader might find it instructive to plot the function $\varphi^*\varphi$ for different values of t to study how its width increases with time. He will see that the probability distribution retains the Gaussian form but broadens out in time, having a width $\sqrt{d^2 + (k^2t^2/m^2d^2)}$, which starts with d and goes to infinity. This

[1] To remain unchanged in time means to be of the form (17.17), as our former considerations indicate.

[2] The details of this example are given in Margenau and Murphy, "The Mathematics of Physics and Chemistry," p. 380, D. Van Nostrand, New York, 1943.

broadening of the distribution of positions in space is spoken of as the diffusion of a wave packet. Clearly, the rate of diffusion is small if md is large, large if that product is small. This conclusion will interest us later (Sec. 18.8) when we investigate classical physics as the limiting case of quantum mechanics; it will again concern us in connection with the problem of causality (Chap. 19).

Summary

Chapter 17 contains the essentials of the mathematical structure of quantum mechanics and cannot be easily summarized. An acquaintance with the differential and integral calculus is assumed, and all new concepts are explained in terms familiar to the student of these branches of mathematics.

In ordinary experience, described by the methods of classical physics, observables are of the possessed variety. The suitable representation of such observables is the mathematical *function.* It is therefore proper that such things as position, energy, momentum be regarded as (numerical) functions of the time. But this representation is wholly unsuited for latent observables, and a new method of procedure must be sought when they are to be treated. This is found in the representation of observables by *operators.*

A state in classical physics was a collection of observables, *i.e.*, of mathematical functions, and it must now be a collection of operators. Such a collection, however, can be shown to be equivalent to another mathematical representation, according to which a state is given by a function of space coordinates, $\varphi(x)$. This function, sometimes called a state function and sometimes a probability amplitude, sums up the entire meaning of a physical state. Knowing it does not enable the physicist to say what will be the value obtained in a single measurement performed upon an observable; it does, however, permit a prediction of a probability distribution with respect to measurements on all observables. This result is precisely conformable to the nature of latent observables; it does not have the character of an incomplete description rendered by a defective theory, as is sometimes supposed.

The new constructs are established in axioms 1 and 2 (Secs. 17.3 and 17.4). The rules of correspondence which link them to immediate experience are developed in axioms 3 and 4 (Secs. 17.5 and 17.6). The last two sections add details needed for greater generality.

The mathematical results of the present chapter will not be used as quantitative tools in the subsequent portions of the book, except in a few strategic places where precision is indispensable. They are needed for a complete understanding of the uncertainty principle.

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CHAPTER 18

Uncertainty and Measurements

18.1. The Uncertainty Principle

The custom has been to introduce students to the uncertainty principle by way of well-chosen empirical examples, and such is doubtless the proper heuristic procedure for making the principle's acquaintance and for learning to work with it. The novel discovery of uncertainty is indeed fraught with great pragmatic consequences, most of which display the unregenerate air of factualness and specificity that attaches to matters of observation and not to philosophic principles. Understandably, therefore, the physicist has at times behaved like the proud and somewhat embarrassed owner of a new and strange toy; he has taken delight in inviting his friends to admire it and in watching the philosopher's astonishment as he turned the switch and made it go. But the knowledge thus conveyed is not tantamount to understanding. Above all it tends to make the uncertainty principle appear like a proscription of certain experimental procedures or at best as a thesis of limited measurability, when in fact it is a basic innovation in our way of representing reality.

The inductive approach via selected experiments has historical importance because it is the psychological avenue which led Heisenberg to the discovery of the principle. Yet it may be said without disrespect for the facts of history that there comes a time when the necessity for organizing knowledge suggests an alternative approach, a logical approach from the side of basic axioms. Only when this is conjoined with the inductive story will the whole picture emerge, only then will the atmosphere of dogmatism which surrounds the principle be finally dispersed. For the philosopher knows that physicists have failed to be convincing, and have often even failed to make sense when explaining uncertainty. Physicists are prone to discuss a few experiments which show how accuracy in measuring position spoils accuracy in measuring momentum and then, under the pretense of drawing conclusions, announce with finality: Position and momentum are not simultaneously measurable, experiments of contrary implication are impossible, and so forth. It is our desire here to show precisely what these assertions mean, how they are rooted in the basic methodology of quantum mechanics, and how far they are from being references to incidental facts of experience. To this end we purposely invert the customary procedure and treat first the logical genesis of uncertainty. Afterward, we shall take occasion to discuss and interpret the usual examples.

The uncertainty principle is contained in the axioms of the last chapter. Or, if it is preferred, one may say that the axioms reflect uncertainty. But if the last version be adopted it is only fair to add that the axioms imply a great deal more than the principle despite its spectacular importance in the history of the subject. What uncertainty means has in fact already been told, though not emphasized, in Sec. 17.6 and in Fig. 17.2, which accompanied it. We shall now extract the important features of that example and discuss them in the light of the present topic. In Sec. 17.6 we have arbitrarily taken a state function of the form

$$\varphi(x) = e^{\frac{1}{2}(x/d)^2}$$

which corresponds to a probability-in-position of the particle given in Fig. 17.2*a*.

This curve has a width *d*, and it is important to remember clearly the meaning of this parameter. Figure 17.2*a* represents the probability, understood as relative frequency, of position measurements on the electron. The state of affairs is one which can be brought about by an electron gun firing electrons at a screen arranged in the manner of Fig. 16.1, but with the first screen missing. Screen *T* is now sensitized so as to become luminous, or to scintillate, at points where an electron impinges, the means for doing so (*e.g.*, coating with zinc sulfide) being well known to the makers of oscilloscopes and electron microscopes. The gun is equipped with diaphragms at different potentials so that each electron leaving it is "prepared" to have a state given by our φ, to wit: the number of impacts on the screen, when plotted along a horizontal line through the center of the impact pattern, is given by Fig. 17.2*a*. The pattern itself will look like Fig. 18.1. As was seen before, it does not matter whether the electrons arrive singly or in one large swarm, provided that the swarm is not dense enough to make the repulsion between the electrons appreciable. This proviso is usually fulfilled in practice. By tinkering with the electron gun the distribution sketched, and hence the state φ, can be realized.

Now if d is zero, all electrons will hit the screen at 0 and the uncertainty in an electron's position along the a-axis is zero. This

Figure 18.1

is not ruled out by the axioms of quantum mechanics: it corresponds to an eigenstate of X, the delta function of Fig. 17.1 with $a = 0$. The pattern of Fig. 18.1 will then concentrate into a vertical line at 0. On the other hand, if d is infinite, the uncertainty of position is infinite; the pattern will degenerate to a uniform haze all along the a-axis, and the function $\varphi(a)$ will become a constant. We shall take d to be a measure of the uncertainty of an electron's position along the a-axis and call it for brevity *uncertainty of position.*

Quantum mechanical theory predicts, as shown in the last chapter, that the probability-in-momentum of the electrons, prepared by the gun to be in the state φ just discussed, is given by Fig. 17.2*b*. Experiment shows this to be true, and while we have no intention of elaborating upon it—the truth of quantum mechanics is here taken for granted—it is nevertheless worth while briefly to indicate how such facts are established.

The fashionable instrument for use in discussions of this sort is a gamma-ray microscope. Such an instrument, however, is a plausible fiction and does not exist as a device suitable for the present purpose, and a better way to find the momentum is not to "measure" it at all. We shall merely move the screen T (cf.

Fig. 16.1) farther away from the gun and determine again the pattern of positions. If the pattern is unchanged, all electrons had the same (namely, zero) momentum along the a-axis;[1] if it is infinitely diffuse, the uncertainty in momentum (along the a-axis) was infinite. In general, by studying the pattern of positions at various distances from the gun the momentum distribution can be determined. It is given by Fig. 17.2*b*; the prediction of quantum mechanics is verified. We wish to emphasize that *the pattern of Fig.* 17.2*b results for the momentum whether or not the position of the electrons forming that pattern has previously been measured.* In our arrangement, the electrons forming the momentum pattern of Fig. 17.2*b* were not previously disturbed.

If the uncertainty of position is d, that of momentum is given by the corresponding quantity in Fig. 17.2*b*; it is k/d. Putting $\Delta x \equiv d$ and $\Delta p \equiv k/d$, the Δ here standing for uncertainty, we have

$$\Delta x \cdot \Delta p = k \tag{18.1}$$

and, as before, k is Planck's constant (usually denoted by h) divided by 2π. Equation (18.1) looks at first like some specific result brought about more or less accidentally by the particular form chosen for φ, by its having the character of a Gaussian function.

As a matter of fact, however, the result is independent of all particulars and appears conspicuously at the end of every calculation of this kind whatever the form of φ. This is shown in many books on quantum mechanics and need not be reproduced here. But caution is requisite in order to stabilize the significance of the Δ symbol, to make it mean the same thing for different φ distributions. For it is clearly to be expected that a change in the definitions of Δx and Δp will bring about a change in the detailed form of Eq. (18.1).

The quantity d in our calculation is the "half width" of the φ^2 curve at a point where its height is the fraction $1/e$ of its maximum value. Obviously, nothing prevents us from taking as a measure of the uncertainty of position some other suitable

[1] In Fig. 16.1 the a-axis is a vertical line on screen T drawn through the center of impacts.

parameter, as for example the half width of the φ^2 curve at half its maximum height. Had this been done. the constant k in Eq. (18.1) would be replaced by $(\ln 2)\cdot k$, a quantity differing slightly from k but still having the physical dimension and the order of magnitude of Planck's h. The freedom of choice in the meaning of uncertainty which becomes apparent here is often falsely injected into the uncertainty principle itself, and many elementary treatments, in trying to avoid specific commitment as to the definition of Δ, have created a gratuitous confusion in the very statement of the principle.

Perfect clarity results when one takes for Δx and for Δp those quantities which statisticians call *standard deviations.* They are more easily defined and in general more significant than the half widths employed in Eq. (18.1), and they retain their good meaning even when φ is more irregular than it is in our single-maximum example. The standard deviation for a curve of Gaussian type is $d/\sqrt{2}$. Hence, if Δx and Δp are standard deviations, the uncertainty principle for Gauss distributions reads:

$$\Delta x \cdot \Delta p = \frac{k}{2} \tag{18.2}$$

When the type of distribution is *not* specified, one may nevertheless prove, using nothing except the axioms, that

$$\Delta x \cdot \Delta p \geqq \frac{k}{2} \tag{18.3}$$

This is the most general formal statement of the principle under discussion, as applied to position and momentum. Equation (18.2) shows that the Gauss distribution is the one for which a given uncertainty in x entails the least uncertainty in p.

The inequality sign in Eq. (18.3) has occasionally created the erroneous impression that one never quite knows what the uncertainties are, even when the Δ's are well defined. Such ignorance is altogether spurious when the state is known, for the axiom tells us in detail how to calculate the distribution (and hence the standard deviation) of momentum measurements when the state function (and hence the standard deviation of positions) is given. For every $\varphi(x)$, Δx and Δp, and hence their product, are exactly known. It is well to bear this in mind, lest the principle in question suffer from an extra uncertainty different from that which it objectively asserts.

An inequality like Eq. (18.3) holds not only for the observables position and momentum, for which it was here illustrated, but for some others as well. Physicists recognize certain pairs of observables as correlated in a sense which we need not explain here; they call the individuals of such a pair "canonically conjugate." Examples are position and momentum, angle and angular momentum, time and energy. All conjugate pairs possess the common characteristic that the product of their physical dimensions has the dimension of action. *For all canonically conjugate pairs, inequality* (18.3), *is true*. Hence

$$\Delta\theta \cdot \Delta L \geqq \frac{k}{2} \tag{18.4}$$

$$\Delta t \cdot \Delta E \geqq \frac{k}{2} \tag{18.5}$$

provided that the symbols θ, L, t, and E denote angle, angular momentum, time, and energy. These results must be stated here without proof; they follow from the form of the operators corresponding to the observables involved. Later sections will offer examples which show more clearly the meaning of Eq. (18.5), but first we return to consider position and momentum a little further.

18.2. Limiting Cases of the Uncertainty Principle [1]

Let us assume that the position of the electrons coming from the gun discussed in Sec. 18.1 is sharp, which means that they all strike the screen at one point, say at zero, and that therefore the pattern of Fig. 18.1 is a vertical line at 0. Then $\Delta x = 0$. It is interesting to see what the axioms now say about Δp.

In mathematical form, the assumption just stated reads as follows:

$$\varphi(x) = \delta(x, 0)$$

This is to be inserted in Eq. (17.11) with the observable p replacing the indefinite q. But $\psi_p(x)$ can be obtained from Table 17.2, and

[1] This section may well be omitted by the nonmathematical reader.

Eq. (17.11) takes the form

$$b(p) = c\int_{-\infty}^{\infty} \delta(x, 0)e^{ipx/k}\,dx$$

Electrical engineers will recognize this integral at once as the Fourier transform of the delta function. However, we need no higher mathematics to obtain its value. The integrand is zero at all points except at $x = 0$, and therefore the integral is proportional to the value of $e^{ipx/k}$ at that point, which is 1. In other words, the integral is not a function of p—it is a constant with respect to p. Hence, according to Eq. (17.12), $W(p)$ is a constant also. The probability for measuring a given p is the same whatever p may be, and the uncertainty in p is infinite. Here we have the answer to our initial question; for the special case in which Δx is zero, the uncertainty principle forces Δp to be infinite.

Now we assume an eigenstate of the momentum observable to be present, and we inquire as to the distribution in x. If the momentum is known invariably to be p, then

$$\varphi(x) = \psi_p = ce^{ipx/k}$$

But we have already demonstrated, in accordance with Table 17.2, once and for all that the probability for measuring position $W(x) = [\varphi(x)]^2$ [see Eq. (17.12)]. In this instance, however, where φ is complex, the square must be replaced by $\varphi^*(x)\varphi(x)$. Since $e^{-(ipx/k)} \cdot e^{ipx/k} = 1$,

$$W(x) = \text{const}$$

The pattern of Fig. 18.1 is a uniform smear; the uncertainty in position is infinite because that in momentum is zero.

These are clearly the two extreme cases to which the uncertainty principle can be applied: in one the position was sharp and therefore the momentum infinitely diffuse; in the other the reverse was true. In general the situation is intermediate between these limiting instances.

18.3. Examples of Uncertainty

We have endeavored to show how the uncertainty principle arises and draws its significance from the deepest stratum of quantum mechanics, from the epistemological doctrine which relates the state of atomic systems to an *aggregate* of datal experiences and not to a single complex called one measurement. The principle exhibits the residual lawfulness, with this term now understood in the manner of classical mechanics, which still prevails between aggregates of observations. Except for this reference to Kollektivs its meaning could not be accurately stated.

Of course the principle also implies that single observations cannot be determinative of the kind of knowledge needed for prediction, not even quantum mechanical or statistical prediction. Uncertainty must always have a destructive effect upon the physical relevance of measurements performed on conjugate observables. In individual cases, this effect may take one of several diverse forms: interaction between the system under observation and the measuring device may render the conclusions to be drawn very inexact; even without measurement, a system may hide its determinate position under the aspect of being a wave; or a process may occur in which a system loses one form of precision while gaining another (this is known to physicists as "diffusion of wave packets"). Such effects are multiple and not easy to classify. Collectively, they are manifestations of what we have previously called the haziness of the immediately given. It is one cardinal virtue of the uncertainty principle that it draws them together into one significant and towering though abstract generalization.

We now present a few of the simpler instances which allow a detailed inspection of the ways in which uncertainty manages to muddle the classical situation. Many others may be found in the famous writings of Bohr [1] and in Heisenberg's [2] own careful summary. The limited selection here made finds justification in two circumstances; first, it is our desire to employ a minimum of technical physics; second, many of these examples are well publicized and easily accessible.

a. Suppose an electron is known to be somewhere on a line, within a range Δx. Since it is to be represented by a wave, this

[1] For example, N. Bohr, *Nature*, **121**:580 (1928).

[2] W. Heisenberg, "The Physical Principles of the Quantum Theory," University of Chicago Press, Chicago, 1930.

may be paraphrased by saying that the wave has finite displacements in the range Δx, zero displacements outside. But if a wave is to have zero displacements, it must suffer destructive interference, and hence it is necessary to build up the wave representing the electron under consideration out of *several* sinusoidal waves, for a wave cannot interfere with itself. In other words, we must

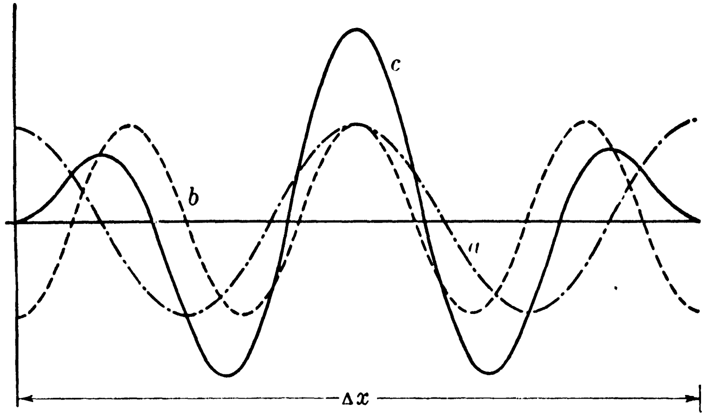

Figure 18.2

employ a system of waves, a wave packet, to represent the electron's state.

Of the various components of the packet it is required that they annul one another at the ends of the range Δx; what they do outside will for the moment be ignored. Now, clearly, to achieve annulment at least *two* sinusoidal waves are needed. If one of them has n (an integer) wavelengths in Δx, the other must have at least $n + 1$. In Fig. 18.2 this state of affairs has been drawn for $n = 2$. Wave a has two wavelengths, wave b has three wavelengths within Δx. Their resultant, c, is the heavy line which evidently possesses zero displacements at the end points of the range. The wavelengths of a and b are, respectively, $\lambda_a = \Delta x/n$, $\lambda_b = \Delta x/(n + 1)$.

Note that the reciprocals of the wavelengths differ by $1/\Delta x$, that is, $\Delta(1/\lambda) = 1/\Delta x$. But ours has been a minimal assumption: we could also have brought about interference at the end points by assigning to wave b, $n + 2$ wiggles, or $n + 3$ wiggles, etc., within Δx. Under these circumstances $\Delta(1/\lambda)$ would have been

$2/\Delta x$, $3/\Delta x$, and so forth. Hence we write for the sake of greater generality

$$\Delta\left(\frac{1}{\lambda}\right) \geq \frac{1}{\Delta x}$$

At this point, de Broglie's equation (cf. Sec. 16.2), $\lambda = h/p$, will be used. When the proper substitution is made for $1/\lambda$, the inequality above reads $\Delta(p/h) \geqq 1/\Delta x$. Since h is a constant, this at once becomes similar to the uncertainty relation:

$$\Delta x \cdot \Delta p \geq h$$

It differs from Eq. (18.3) by featuring h in place of $h/4\pi$, and this is occasioned by the different meaning of uncertainty here adopted. For the Δ's employed in the foregoing development signified not standard deviations but the whole range in which the observables could be found.

Our result does not ensure that the waves will interfere destructively outside the range Δx, since the wave displacement has been made to vanish only at the end points. To enforce interference everywhere else outside Δx, further waves have to be introduced which, however, will not prevent the interference of the two here chosen. They must be of smaller wavelength than b, and hence their reciprocal wavelengths differ from $1/\lambda_a$ by even greater amounts than we considered. All this strengthens the inequality derived but leaves its formal statement unaffected.

In the present example no measurements are involved; uncertainty arises from the wave aspect of the electron alone, and the wave aspect reflects what we have previously called the latent character of the observable, position. We prefer to leave it at that despite the tendency among physicists to trace all uncertainties to the agency of measuring devices. This can be done, of course, if one is not too delicate in his reasoning. For one can say that the electron would not act like a wave if its position were never measured; *ergo* it is the measurement which makes it behave like a wave and thereby causes the uncertainty.—Naturally, measurement is quite defenseless under this indictment, for it is the source of all we come to know about the electron. The issue raised here affects the final interpretation of every example included in this section. With this understanding we forego its reiteration until we are ready to settle it more satisfactorily. To do so clearly requires first of all a thorough examination of what a measurement means, and this will be postponed until the following section.

b. An illustration of the uncertainties in energy and time, represented by Eq. (18.5), is also readily available. It is based upon the Planck–Bohr–de Broglie relation between energy and frequency: Wherever there is energy E, there is presumed to be a vibration of frequency ν, such that

$$E = h\nu$$

When this is taken seriously, as it must be in atomic physics, it suggests that to measure E we must count vibrations. The error in E, ΔE, is $h\,\Delta\nu$, where $\Delta\nu$ is the error or uncertainty in ν. Now counting is done by reciting the integers, and an error, if made, is at least equal to 1. And we can never be sure that a minimum error has not been made. Since $\nu = n/\Delta t$, n being the number counted and Δt the time interval of counting, $\Delta\nu = \Delta n/\Delta t$. But $\Delta n \geqq 1$. Collecting these results, we have at once

$$\Delta E = h\,\Delta\nu = h\frac{\Delta n}{\Delta t} \geq \frac{h}{\Delta t}$$

so that

$$\Delta t \cdot \Delta E \geq h$$

Again, this result does not agree exactly with Eq. (18.5), the reason being an altered meaning of uncertainty.

One naturally wonders here why Δt should be an uncertainty in time. The presumption is that over this whole interval the frequency was ν and the energy E; in any subinterval the ratio $\Delta n/\Delta t$ might have been different. From this point of view Δt is in fact the smallest stretch of time over which the energy could be guaranteed to have been E. But it is unwise to attempt to sharpen the logic of any single example, for in every one of them one knows what he is going to get, and the rigor is not the optimum attainable in physical reasoning. This is a further reason why the appeal of the uncertainty principle, when delivered piecemeal in specific examples, is weak.

c. Photons, also, are subject to uncertainties. To illustrate, we first recall a fact of classical wave theory, well known to everyone conversant with radio transmission: The shorter the time of emission of a signal, the greater the range of frequencies it is bound to contain.[1] If a signal is to have a single frequency, it must last forever. In mathematical terms: If Δt is the time of emission and $\Delta\nu$ the range of frequencies composing the signal,

$$\Delta\nu \geq \frac{1}{\Delta t} \tag{18.6}$$

Sometimes $\Delta\nu$ is called the width of the frequency band; it represents the range of uncertainty with respect to frequency. In the same sense, Δt is the uncertainty in the time of emission or, in general, the uncertainty in the time at which the signal passes a given point.

Now according to the quantum theory of light the momentum of a photon is $p = h\nu/c$, c being the velocity of light. The uncertainty in momentum, Δp, equals $(h/c)\Delta\nu$. Using Eq. (18.6), we have $\Delta p \geq h/c\,\Delta t$. But what is $c\,\Delta t$? It is the distance over which the wave travels in a time equal to its time of emission, *i.e.*, the length of the wave train constituting the photon, and this is its uncertainty of position, Δx. The result obtained is therefore again

$$\Delta p \cdot \Delta x \geq h$$

d. The photon analogue of Eq. (18.5) is even simpler to demonstrate. For we know that the energy of a photon is $E = h\nu$, and therefore $\Delta E = h \cdot \Delta\nu$. When this is combined with Eq. (18.6), there results at once

$$\Delta E \cdot \Delta t \geq h$$

e. The position of a particle can be determined with the use of a microscope. To locate an electron with atomic precision it is necessary to use light of shortest known wavelength, *i.e.*, gamma rays. But if the wavelength is small, the frequency is large and the momentum $p = h\nu/c$ is correspondingly great. Such light, which is needed for an accurate position measurement, is therefore like a swift projectile and communicates an appreciable momentum to the electron it strikes, thereby altering the elec-

[1] For that reason one cannot play staccato on an organ, each pipe of which has a well-defined but very limited frequency response.

tron's original momentum by an uncontrollable amount. An accurate position measurement "spoils" the particle's momentum.

Conversely, if the momentum of the particle is to be measured with maximum precision, the photon bullet to be reflected from it must have a small momentum of its own and hence a small ν or a large wavelength. But the accuracy as to position conveyed by a signal of large wavelength is small, for certainly the reflecting object cannot be located within a space smaller than a single wave.[1] Simultaneous accuracy of measuring position and momentum (or velocity) is precluded by these facts.

To make these considerations quantitative we recall a theorem of optics, stating that the minimum distance of resolution for any microscope is

$$\Delta x = \frac{\lambda s}{D}$$

In this formula Δx is the distance between two points of the object which can just be recognized as distinct in the microscope; λ is the wavelength of the light employed, s the distance of the object from the instrument, and D the diameter of the objective. For the present purpose, Δx may be regarded as the uncertainty in position of the particle to be viewed.

In reflecting the light, the particle will suffer a recoil which is known from the theory of the Compton effect to be of magnitude h/λ. This recoil is not entirely parallel to the focal plane of the microscope in which the distance x is measured, but its magnitude Δp in that direction is at least $(h/\lambda)(D/s)$. On combining this result with the last equation we have finally

$$\Delta p \cdot \Delta x \geq h$$

as was found before.

f. A simpler example is presented by a stream of particles going through a narrow slit of width d. Since on the other side of the slit the stream also has a width d, this quantity may be chosen to represent the uncertainty of position of the particles, Δx, in a direction across the stream. But the particles are also diffracted and are therefore no longer moving in the same direction after

[1] An ocean wave is not reflected to an appreciable amount by a small object floating on the water.

passing the slit. As is known from elementary optics, the emergent stream has an angular width α such that $\sin \alpha = \lambda/d$ where λ is the de Broglie wavelength. Particles in the extreme rays composing the stream have a momentum $p \sin \alpha$ in the x direction, and therefore the uncertainty in momentum is greater than or equal to this amount,

$$\Delta p \geq p \sin \alpha = \frac{h}{\lambda} \sin \alpha = \frac{h}{\lambda}\frac{\lambda}{d}$$

if use is made of the de Broglie relation $p = h/\lambda$. When we couple the last result with the identity $d = \Delta x$, we obtain $\Delta p \cdot \Delta x \geq h$.

This example also is often cited as demonstrating the destructive action of measurement upon observables. Passage through the slit is taken as a measurement of position, the diffractive effect as a destruction of the precision which the momentum originally possessed. We shall now pause to consider this interpretation of *measurement* and several others more closely.

18.4. What Is a Measurement?

The need for examining the meaning of so familiar a term as measurement does not arise from changes of conception that have occurred in quantum physics. In fact measurement, which is the scientist's ultimate appeal to Nature and therefore has a final reference to that part of man's experience which is not in his control, cannot change its singular character without altering the whole of science in a manner far more profound than even quantum mechanics has attempted. The scientists' basic disposition to acknowledge measurement for what it was and is forms the thread of continuity that runs from classical to modern physics. It is to recall and to reemphasize this element of continuity that a discussion of measurements is required here; the reaction to the novelty of quantum reasoning on the part of scientists has often been a remarkable propensity to use the term *measurement* very loosely, even to relinquish its traditional significance in classical physics. This, we feel, is unfortunate.

Avoiding all sophistication, we easily see that measurement involves (1) an object (in our terminology a physical system) upon which an operation is to be performed; (2) an observable whose value is to be determined; (3) some apparatus by means of which the operation can be carried out. The object may be a table, the observable a length, and the apparatus a yardstick. Or the object may be an electron, the observable its momentum, and the apparatus a gamma-ray microscope. In the latter case we should be dealing with a thought experiment leading to a measurement, and we do not wish to exclude such ideal types of operation so long as they do not violate confirmed laws of physics. As to (3), the apparatus may be a counter, an observer's eye or some other sense organ.

Items (2) and (3) are closely related, particularly in atomic instances where observables may be latent and draw their peculiarities from the operational procedures which lead to their determination. Nevertheless, the variety of observables is small, the variety of procedures unlimited, so that observables and operations cannot be identified. As we have shown in Chap. 12, the latter serve as epistemic definitions of the former, and they do this for latent as well as possessed observables of the usual variety. Perhaps this is worth some emphasis: Operations capable of leading to measurement, while yielding numerical values, also define the construct known as the observable.

Object, observable, and apparatus are essential to the performance of a measurement, but they are not by themselves sufficient to constitute one. For suppose you wish to measure the speed of a race horse by means of a motion-picture camera. All three items are at hand, and we shall suppose the required operation to be performed. But it happens that the horse is out of focus or that the film was developed improperly and did not yield the desired information. To be sure, the performance was an experiment, an observation; but was it a measurement? The answer is clearly no. One may wish to call it an unsuccessful measurement; granting this, however, we are forced to confine our attention to successful measurements, since these alone are of interest in science. No departure from this maxim is to be tolerated in any part of science, including quantum mechanics.

A measurement is more than an experiment, more than an observation; it differs from both of these by having as its culmination the emergence of a *numerical value.* Without such a yield what pretended to be a measurement is merely an *operation*, and this term may suitably be used to designate all the general procedures commonly called observation, experiment, manufacture of equipment and what we shall later discuss as "preparation of a state." Of course it includes measurement. Whether the numerical value obtained in a measuring operation is in some sense correct or incorrect is not important from our present point of view. In any case this cannot be decided by reference to the outcome of one measurement alone. Hence an incorrect measurement is still a measurement, but an unsuccessful one (which fails to yield a number) is not.

A single measurement never provides the maximum information desirable concerning an observable, and the scientist is often reluctant to accept the outcome of a single measurement as significant. Whenever possible a *set* of measurements, statistically treated to supply one optimum value, is placed over against a theoretical prediction for confirmation. This was discussed more fully in Chap. 6. None of these features is changed in quantum mechanics; they are indeed more sharply outlined in the light cast upon the problem of measurement by the latency of atomic observables.

Perhaps it is instructive to review the examples of uncertainty described in the foregoing section. The first involved theoretical arguments about how it is possible to construct a wave packet. No apparatus, no number was in evidence; hence the example did not constitute a measurement, and it would be incorrect to suggest that it traces the origin of uncertainty to measurements or indeed to any physical operation.

Example *b*, the determination of energy by counting, does constitute a measurement, for it resorts to a system (whatever possesses the energy), an observable (ν), and an apparatus (the eye, or the spectrograph, or whatever registers the vibrations). And, more important, it yields a number, n, from which ν and E are finally computed. Here a measurement may accurately be said to be the source of uncertainty. It may seem strange, however, that its physical origin is not an interaction with a measuring device but rather a sort of handicap on the part of the observer who can count only in integers and is unable to certify fractional vibrations.

Examples *c* and *d* are very much alike. They reason about observables in terms of theories which allow representations of these observables. Involving no measurements, they suggest another origin of uncertainty: the finite time of emission of light. Uncertainty has here been implanted into the photon at its birth and has thus become singularly unrelated to all human agencies.

Heisenberg's gamma-ray microscope provides measurements of both position and momentum of an electron. In Example *e*, therefore, it is clearly shown that measurement, and in particular that phase of measurement during which the apparatus interacts with the system, *can* be the source of correlated uncertainties in conjugate observables. Heisenberg's book contains other instructive examples of this type.

Finally, in Example *f*, we encounter something which is often misconstrued and needs closer examination. If an electron or in general a free particle goes through the slit, its position is known to be within the width of the slit. There are present all three preconditions of a measurement, the system (particle), the observable (position), and an apparatus (screen with slit). And there *seems* to be available a number, the coordinate of the (center of the) slit which specifies the position of the particle after its passage through the "measuring device." It is strange how readily some physicists have accepted this illusion. For is it not obvious that there is no number that has even the remotest reference to the particle until the particle has been *seen* to go through the slit? Merely setting up the arrangement and then arguing about what happens is not a measurement; to complete it the electron must be subjected to some test of its presence, and this test will introduce further uncertainties. What has been said shows that Example *b* does not represent a measurement; but the remarks just made are more important in a later phase of our discussion and will there be recalled.

It is apparent that the sources of uncertainty are numerous and do not invariably reside within that narrow class of operations called measurements. On the other hand, the effect of measurements upon physical systems is equally difficult to specify in terms of narrow characteristics, as will now be shown. In classical physics it is assumed without serious jeopardy to precision that the effect of measurement on a system is nil. To put it more properly, while an effect is recognized, this effect is presumed to be reducible to negligible magnitude if sufficient care is taken in the measuring operation, and theory gets along without paying attention to it. But two unique circumstances made the adequacy of this presumption dubious for *atomic systems.* One is the smallness of atomic systems and its consequence, the relatively small amount of energy contained in them; another is the fact that energy is quantized, the quantum being comparable in size with the atomic energies in question. Hence the effect of measurement cannot be neglected.

This effect is quite different for different systems and varies all the way from a slight disturbance to catastrophe. To measure a proton's position only a small interference with its state of motion, *here for the moment classically conceived*, is necessary. To measure a photon's position, the usual method is to *destroy* it. For every direct observation of a photon, such as is necessarily involved in a measurement, requires its absorption, requires not only a termination of its state but indeed a destruction of the photon as a real entity. Nor is the photon an exception in this respect. The electron often shows a similar drastic reaction to the act of being observed; the usual way to detect its presence is to collect it on an electrode, which certainly terminates its former state since the electron, when absorbed in a metal, ceases to be the free particle it was before, the particle whose character was to be made manifest in the measurement. Mesons and neutrinos also are destroyed when subjected to measurement. Atomic physics has joined biology in recognizing that significant experiments may kill the system.

Are we then to reject such catastrophic operations from the class of good measurements? To do so would convert experimental atomic physics into a barren field, since it would leave us with a definition useless for most purposes. Quantum theory here comes to our aid and saves the situation through its remarkable virtue of being able to operate with precision in situations where the result of a measurement, in classical terms, is disastrous to its state. It achieves this virtue, not by following through the effect of measurement on observables in detail, as classical physics would be forced to do, but by incorporating statistical uncertainty into its very axioms. By this master stroke it has freed itself of the cumbersome necessity of appraising disturbances due to measurement in every specific instance.

The kind of uncertainty which is *not* automatically dealt with in this way is that attributable to *avoidable* inaccuracies of measurement. Here quantum mechanics is no better off than classical physics and proceeds in the same way as that discipline to eliminate ordinary errors. And this is as it should be, for the precautions which must be taken are concerned with objects of ordinary magnitude and with their adjustment, and classical physics is the proper guide in the performance of such tasks.

One final comment about measurement. In ordinary language, the numerical outcome of a measurement characterizes the state of the observed system at the time at which the observation was made. If the processes constituting the measurement require a finite time, the reference of the measured value becomes slightly inexact but is usually taken to be to the instant when the processes start. A speedometer measures the speed of an automobile at the time it is read, not five minutes ago or five minutes hence. Or, to be very precise, it measures the speed of the car at a time prior to the instant of observation by a small interval, equal to the time required for the torque in a flexible cable to be transmitted from the wheel to the dashboard. But never do we say that we are measuring the value of a quantity at a future time, even though it may be possible to *predict* that value on the basis of a measurement made at present.

In classical physics, where it hardly matters, this is taken for granted. In quantum mechanics where, because of the possible catastrophic end of measurements, clarity as to the time reference is of the utmost importance, a good deal of confusion has arisen. The practice of postdating the import of an observation is not uncommon in the looser discussions of uncertainty. Yet if measurements refer to future states, we are at a loss to understand, for instance, what can possibly be meant by a spectrographic measurement of the wavelength of a photon, for the photon is no longer present after it has been photographed. We see that in quantum mechanics, above all, reference to the state *at* or *before* measurement must be consistently maintained.

Kemble,[1] in carefully surveying numerous possible measurements, uses a language which is perhaps unfortunate. By distinguishing between retrospective and predictive measurements he ignores considerations like those above, and he establishes an artificial barrier between classical mechanics and quantum theory. When carefully considered, his predictive measurements turn out not to be measurements at all but *preparations of states*, for they lack the one element which makes an operation a measurement: the emergence of a number.[2]

18.5. Compatibility of Measurements

The uncertainty principle correlates the "spread" in a Kollektiv of position measurements, made when a system is in a state specified in the most exact quantum mechanical manner (by means of a function φ), with the spread of momentum measurements on the same state. If, for instance, the state is so chosen that all measurements yield the same value for the position of the system (eigenstate of X), then the momentum determination will yield values ranging all the way from $-\infty$ to $+\infty$, no matter by what experimental means the measurement is achieved, and regardless of whether it is performed successively many times upon the same system in this state or performed once upon a large aggregate of similar systems, all in this state. Nowhere, however, does the principle suggest with precision what may be expected in a *single* measurement of any kind. The reason is that the idea of probabilities, as it is employed by workers in quantum physics, is not of the subjective or a priori design but is grounded in *frequencies*.

If nothing is known about the state of an electron and we make *one* measurement of its position, let us say with the use of a

[1] E. C. Kemble, "The Fundamental Principles of Quantum Mechanics," McGraw-Hill Book Company, Inc., New York, 1937.

[2] A monochromator or an electron gun prepares a state. Only when coupled with an absorbing device does it function as a measuring instrument.

gamma-ray microscope, then we can infer absolutely nothing about the probable yield of a momentum measurement. If 100 position measurements are made, all of which show the electron to be at the same place, then it may be expected with fair certainty that 100 momentum measurements would yield widely divergent results; but again nothing can be said as to the outcome of one measurement of P. If 100 position measurements show no uniformity whatever in their results, we can say with reasonable certainty, not what the numerical outcome of the first P measurement will be, but that it will have much the same value as 100 others.

Whatever the state of an electron, every position measurement performed on it will yield a number, and this is equally true of every momentum measurement. If X is measured, a single P measurement can nevertheless be performed, and it will result in a sharp numerical answer if conditions are properly arranged; at any rate no blurring effect of one observation upon the other can be assumed to occur. Indeed there is nothing to prevent us from measuring X and P as nearly simultaneously as we please or as instrumental complications of a purely classical sort will allow us to do. Two microscopes, one employing gamma rays and the other waves of suitable, greater length, may be used simultaneously, one for the purpose of locating the electron and one for determining its position. With luck both can be achieved, for there is no law of quantum mechanics (and no other significant physical consideration) which basically prohibit this double measurement from succeeding. If it were repeated, the values of x and p would spread in accordance with the uncertainty principle. Of course, no one would say after one such pair of measurements that he knows x and p with precision, any more than he would say after one X measurement that he knows the electron's position. The first statement is illicit not because it contradicts the uncertainty principle but because it, like the second statement, contradicts the quantum mechanical meaning of a physical state.

Much confusion has been created by writers [1] who, unwilling to upset their readers' trust in the picturesque notions of hard and determinate moving particles, offend their logical sensibili-

[1] These include even Heisenberg and many of his interpreters.

ties by careless discourse. The ambiguous word *incompatible* is often used to characterize simultaneous measurements of conjugate observables. As we have seen, such measurements are compatible in the sense of being performable and of giving exact answers. It is true that 100 measurements yielding the same p are incompatible with 100 measurements all yielding the same x (except as a freak occurrence of statistics), but this is hardly the sense suggested by the word incompatible when used without qualifications. Kemble [1] does not hesitate to put things even more bluntly: his phrase "the *impossibility* of making simultaneous exact measurements of the Cartesian coordinates and conjugate components of linear momentum" must be misleading to a reader who does not scan his meticulous work with sufficient care to see that its author mixes different ideas of measurement (cf. preceding section) and different formulations of probability (cf. next section).

18.6. Measurements and Eigenstates [2]

It is often said that, when a single measurement of an observable Q is made and the result is found to be q, the state is then *known*, and known to be ψ_q, the eigenstate belonging to the measured value. This, if true, would require a fortuitous and unpredictable, sudden conversion of a state φ before the measurement into the state ψ_q after the measurement.

In view of the foregoing developments such an interpretation raises certain difficulties. First of all, perfectly good measurements in atomic physics (as well as in biology and in the social sciences) may "kill" or annihilate a system, and we may be pardoned if we refuse to discuss the eigenstates of nonentities. Second, every statistician would have the right to accuse the physicist of absurdity if we tried to construct a probability distribution (which a state function always implies) on the basis of a single test. One might ask, therefore, why this view can still be held.

[1] *Op. cit.*

[2] The contents of this section are still under debate and represent a view which is not accepted by all physicists. Readers interested in the "orthodox interpretation" should omit this section.

Perhaps one reason is an unwillingness to embrace consistently the basic epistemology of quantum mechanics, to accept the irremediable latency of observables; another reason may be a misunderstanding of what constitutes a measurement. The strong appeal of these two errors, persuasively combined, is apparent from the following argument, which is found in the literature of quantum physics:

It is desired to measure the "spin," that is, the angular momentum, of a photon. Classically speaking, the spin is simply the direction of polarization of the photon. For that purpose we interpose in the path of the light beam a polaroid analyzer (assumed for the sake of argument to produce no absorption) or a Nicol prism. If the photon passes the analyzer, we know it to be polarized along the direction in which the axis of the instrument was set, and hence we may regard the passage as a measurement. But it is certain that if another analyzer were set in the path of the photon, with its axis parallel to the first, the photon would traverse the second analyzer also, and in fact any number of analyzers set with the same alignment in the photon's path. Is it not obvious, then, that the first measurement produced an eigenstate with respect to spin, with every subsequent measurement yielding the same value of the spin as the first?

The unreformed classicist is swept off his feet by such persuasion. Hence it is well for us to proceed with care. Even if we disregard the fact that talk about a photon going through analyzer 1 at one time, analyzer 2 at a somewhat later time, etc., deals brutally with the spirit of quantum mechanics, we must note a major oversight. How do we know that the photon passed through analyzer 1 if we did not catch it in transit between the two polaroid disks? In what sense, then, is the *conjecture* of its passing a Nicol a measurement? If the conjecture were made into a measurement by inserting a camera or a recoiling set of particles (Compton or Doppler effect), the photon would be very unlikely to go through any Nicol but the first. The passage of a photon through a succession of analyzers is a thought experiment, but not a measurement. This argument therefore completely fails to show that a measurement produces an eigenstate. On the view we hold, this is the only proper conclusion to be drawn, and it is a fortunate one in view of its simplicity.

On the other hand, there is no reason why *some* measurements may not also effect the preparation of a state and thus function to produce an eigenstate. The example often quoted concerns an electron which goes through two parallel photographic plates in succession, blackening a silver grain in each. The two marks of its passage are then supposedly close together, and the first can be regarded as a measurement with respect to the state of the electron at a time prior to its passage through the plates. As such, it does not render that state any more determinate than it was before. But the first passage can also be regarded as preparing a state upon which the second plate is to perform a measurement. That state is indeed an eigenstate of the position observable at the instant of the first passage, but it diffuses rapidly in the manner of Eq. 17.18.

Except for its all too classical tenor there is nothing wrong with this analysis. But if the reader feels that the distinction just made between measurement and preparation of state, and the shift in description which it entails are artificial, we must consider the following point. Suppose the electron, long before its encounter with any plate, is in a sharp state with respect to its energy (or its momentum). This requires that it must remain in that state forever in view of the uncertainty relation between energy and time. To say that a position measurement *alters this state* contradicts the original assumption and makes quantum mechanics an inconsistent theory.

As we have repeatedly emphasized, the statistical character of quantum mechanics has been designed for the purpose of enabling that science to deal with situations in which a single measurement means very little with respect to the state of a system. Among the reasons for this important innovation was the very occurrence of catastrophic measurements, was the need for the physicist to terminate his inquiry at the time of measurement. The argument we are rejecting is one which forgets all this and insists on "tracing things through" when the whole methodology of quantum mechanics gives physics the liberty to forsake such endeavors, when it shows in fact that they must be fruitless.

There is no clear empirical argument by which it is possible to prove or disprove the thesis that a measurement converts a state φ into an eigenstate ψ_q. However, there are two views, both differing from that here discussed, which endow the theory of "acausal jumps," here criticized, with a measure of validity. Except in von Neumann's book, they have not been clearly distinguished from each other or from the view here presented. View 1 avows the subjective or a priori theory of probability and correspondingly sees in a state function not so much an objective ensemble of observable attributes relating to a physical system, as we do, but a subjective *measure of knowledge* on the part of the observer. Knowledge does change abruptly with measurement.

To be sure, when this logical attitude is carried through consistently, it violates many ideas which are commonly accepted and indeed it converts physics into a branch of psychology. But it permits the theory of acausal jumps to be defended. The present author has enlarged on this problem elsewhere and has discussed the pros and cons of the psychological theory of quantum mechanics.[1]

View 2 cannot be compressed into a few lines. It involves an essential enlargement of the axioms which give rise to the standard form of quantum mechanics, those which have here been presented and drawn upon. The extended formalism (to which the following section is devoted) differs from "ordinary" quantum mechanics (to which the treatment thus far has been limited) in the same respects in which statistical mechanics differs from classical mechanics. The tired reader need not struggle with it, for its use has thus far been very restricted.

18.7. Von Neumann's Theory of Mixtures and Pure Cases

In all preceding discussions, when a system was said to be in a state φ, it was meant to be *surely* in that state. This, as we now know, is the maximum information that can possibly be con-

[1] H. Margenau, *Phil. Sci.*, 4:337 (1937).

The claim that states have objective meaning and are independent of what a specific observer knows about them is, of course, wholly compatible with a principal thesis maintained in this book, the thesis that physical description never goes beyond the range of *experience*. Whatever one analyzes is in this basic sense part of one's experience, but within this experience knowledge of verifacts can be distinguished from verifacts. Objectivity, as has been repeatedly emphasized, does not lose its meaning.

veyed about an atomic system, and it corresponds to the classical situation in which all variables of state are specified. And it is hardly necessary to repeat that it amounts of course only to knowledge concerning statistical distributions of observed values, this being the optimum information attainable.

But in classical mechanics one also encounters interesting instances where knowledge is not of this optimal character and where some form of prediction is nevertheless desirable. Such a case would arise if, instead of knowing position *and* velocity of a particle, one has available knowledge of its velocity only. Or instead of knowing its position precisely it may be that we know it only as to its probabilities. Statistical mechanics with its temperature and entropy is concerned with this type of situation. In all such cases, the incompleteness of determination or of knowledge is allowed for by superimposing on the laws of motion the ordinary theory of probabilities.

In quantum mechanics, too, our knowledge may not be maximal. Instead of being certain that an electron's state is φ_1, one may be reduced to saying that it is either φ_1 or φ_2, or that the electron is one of an assemblage of electrons (an actual physical set of electrons, not a Gibbsian ensemble), half of which are known to be in state φ_1 and half in state φ_2. When this contingency arises, incompleteness of determination is again allowed for by superimposing on the axioms of quantum mechanics the ordinary theory of probabilities.

How this can be done with great mathematical elegance and in a manner that permits ordinary quantum mechanics to be regarded as a special case of the extended formalism, was first shown by von Neumann.[1] He distinguishes submaximal knowledge from maximal knowledge and calls the incompletely determined state to which the former refers a *mixture;* the state characterized as heretofore by a single, surely known state function is then often called a *pure case.* It is possible to consider the pure case as a limiting form of mixture.

To represent a mixture of pure-case functions φ_1, φ_2, φ_3, etc.,

[1] J. von Neumann, "Mathematische Grundlagen der Quantenmechanik," Springer-Verlag, Berlin, 1929.

one must know the probabilities [1] with which these pure cases are present. We shall call them w_1, w_2, w_3, etc. This leads us to construct a table, as Table 18.1.

Table 18.1

Pure-case function	φ_1	φ_2	φ_3	...
Corresponding probability	w_1	w_2	w_3	...

The φ's need not be eigenfunctions of any known observables, but each is a well-defined function of the proper arguments for the system. If now we were to construct a new function

$$\varphi = w_1\varphi_1 + w_2\varphi_2 + w_3\varphi_3 + \cdots$$

in the seemingly reasonable hope that this will allow an extension of standard quantum theory to this field of lesser knowledge, we should be disappointed, because the application of the preceding axioms then leads to total nonsense; in fact there is no way in which a single function, subjected to these axioms, can be made to yield agreement with experience. Von Neumann introduced a method which works. From a table like Table 18.1 he constructed a certain matrix, known as the statistical matrix for the mixture, and in terms of this new mathematical tool our former axioms can be rephrased. Both states and observables now become matrices; the mean value of an observable, formerly given by an integral (omitted in our presentation), becomes the trace of the product of two matrices, and so forth. The probability for observing a certain eigenvalue comes out to be what it should, namely, as the ordinary mean of the various quantum mechanical probabilities. But the nicest feature of the scheme is this:

When the mixture reduces to a pure case, that is, when all the w's except one are zero, the statistical matrix takes on a special and very simple form; it becomes what mathematicians term an *elementary* matrix (one whose square is equal to itself). And when an elementary matrix is used in connection with the rephrased axioms, these axioms become identical with the old. We have here, then, an extension of quantum mechanics which is useful for

[1] Here one need not insist on a frequency interpretation of probabilities; the theory makes sense either way!

solving problems of quantum thermodynamics, to which von Neumann applied his method with success.

We embarked on the exposition of the mixture theory because of its significance for the interpretation of measurements, a significance which can now be summarized quite briefly if we are allowed to omit mathematical detail. One may show that usually *the state of a single system, which is a pure case to begin with, is converted by a measurement into a mixture* in which the probabilities for observing different eigenvalues of a given operator appear as ordinary probabilities and are equal to the quantum mechanical probabilities which are computed without von Neumann's extension. This, of course, says exactly what the simpler formalism did if the interpretation we have suggested is put upon it: Measurement destroys the state as a pure case.

In a way the theory of "acausal jumps" also receives support from this conclusion for it does turn out to be true that measurement causes an abrupt change, but from a pure case to a mixture. This change cannot be represented at all by the ordinary theory, and in particular it is not a sudden jump from one pure state (φ) to another (ψ_q).

18.8. Classical Mechanics as the Limiting Form of Quantum Mechanics

Throughout these pages there has been emphasis upon the continuity of scientific reasoning from classical to quantum physics. But as to actual detail, the reader has had to take much for granted. The axioms of the previous chapter bear no resemblance to the laws of classical mechanics; the uncertainty principle and the diffusion of wave packets have no place in the older scheme. It might appear therefore that quantum theory is dealing with a set of facts which are subject to wholly different laws. This however is not true; for classical mechanics is the limiting form of quantum mechanics, which is valid to an extremely high degree of approximation under the following two conditions: (*a*) the mass of the system is large; (*b*) the range of motion or the size of the system is large. What is meant by large will soon be evident; let it be said in anticipation of later results that ordinary visible objects have sufficiently large masses and sizes to make classical description abundantly safe.

The first question which needs to be settled arises in consideration of Heisenberg's uncertainty principle. That principle is true of course for all systems. What it implies for nonatomic situations is best seen on writing Eq. (18.1) in terms of velocities rather than momenta. Since $p = mv$, m being the mass of the "particle," this equation may be put in the form

$$\Delta x \cdot \Delta v \geq \frac{k}{m} \tag{18.7}$$

Now the constant k has a value of 1.06×10^{-27} erg-sec. The mass of an electron is 9.1×10^{-28} gm; its range of motion in an atom is about 10^{-8} cm, and this may here be taken as a rough measure of the uncertainty of position, Δx. We thus obtain

$$\Delta v \geq 10^{8} \text{ cm/sec}$$

In other words, the *uncertainty in velocity of the electron is about* 600 *miles per second*, a very respectable velocity!

Relation (18.7) must also be used for computing the uncertainty in the motion of ordinary objects. Take, for example, a small stone, weighing 1 gm. Its center of mass (which is what matters here) can easily be located within 1 mm; hence we may take $\Delta x = 0.1$ cm. Inserting these values we find

$$\Delta v \geq 10^{-26} \text{ cm/sec}$$

This limit is so small as to be utterly indetectable; an object crawling with a speed so infinitesimal would require a time far greater than the age of our universe to advance 1 mm. The effect of the uncertainty principle upon directly observable motions is therefore completely negligible. In more technical terms, on application to ordinary motions the probability distributions to which quantum mechanical states have reference shrink to forms approximating the delta function of the last chapter, and thus probabilities degenerate to certainties or virtual certainties.

But what about the diffusion of wave packets, treated in Sec. 17.8? If the certainty implied by the delta function is frittered away in time as the pattern spreads, the conclusion just reached is of no interest. To investigate this question we return to Sec. 17.8. The quantity $\sqrt{d^2 + (kt/md)^2}$ represents the width of the diffusing packet at time t, the original width being d. Inserting numbers we find:

a. An electron, confined to a space $d = 10^{-8}$ cm, will double its width in 10^{-16} sec, which is approximately the time required by the electron to traverse a Bohr orbit. In simple language the electron "dissolves" in space very rapidly indeed.[1]

b. A particle of mass 1 gm, located within $d = 1$ mm, will "double its width" in 10^{25} sec. This is a span of time 100 million times the age of the universe. Hence we may well say that systems of ordinary size do not diffuse at all and are not prohibited from having the properties of rigid bodies.

So far so good. But will the axioms of quantum mechanics reduce to Newton's law of motion when the masses are large? The answer is given by a theorem proved by Kennard and by Darwin [2] and produced in a simple form in Heisenberg's aforementioned book. Newton's second law of motion, the only one of importance here, asserts

$$m \frac{d^2x}{dt^2} = F(x)$$

provided that $F(x)$ is the force acting on a particle of mass m at x. By starting with Eq. (17.16), which represents axiom 5, and then applying a few simple calculus operations to it one arrives at the following result:

$$m \frac{d^2\bar{x}}{dt^2} = F(\bar{x})$$

[1] We remind the reader that this occurs because the state assumed is not an eigenstate of the energy observable H. If it were, the state would be stationary and show no diffusion. The wording above is perhaps too picturesque, for it is clear that the electron does not dissolve or diffuse in the manner of an ink drop in water. It is the probability distribution, *i.e.*, a curve like Fig. 16.2, which spreads in time.

[2] E. H. Kennard, *Zeits. f. Physik*, 44:326 (1927). C. G. Darwin, *Proc. Roy. Soc.*, A117:258 (1927). Heisenberg, *op. cit.*, pp. 36–37.

This equation has the same form as Newton's law, but the quantity $\bar{x}$ appearing in it is this *mean position of the wave packet.* Now we have already proved that the wave packet is very narrow for ordinary masses and sizes (this is a consequence of the uncertainty relation, which is itself based on axioms 1 to 4) and that it is permanent in time (consequence of axiom 5). Hence $\bar{x}$ is identical in this case with the classical position of the particle.—A more convincing reduction of quantum mechanics to classical mechanics can hardly be desired.

If any doubt is left as to the validity of classical physics within its proper domain, it is likely to concern the arrangement of observable values regulated by axiom 3, an arrangement which is often discrete. How can the phenomenon of quantization drop out of the picture as we proceed to larger masses and sizes? In classical physics all values are essentially permitted, in quantum theory only some values, and this seems paradoxical.

We have prepared the answer to this question by work already done. Equation (17.6), which is applicable to a free particle moving within a range l, indicates that the separation between energy levels is of the order of magnitude k^2/ml^2. For an electron (with $l = 10^{-8}$ cm) this quantity has the value 10^{-11} erg, which is a sizable amount in comparison with the energy possessed by an atomic electron. For a particle of mass 1 gm, confined within 1 mm, the level separation is 10^{-52} erg, which is utterly negligible. The excluded portions of the energy scale are thus seen to vanish, and quantization becomes entirely ineffective. Although this is shown here for only one example, a similar result holds for all other instances of quantization. Another simple case illustrating this reduction can be found in one of the first textbooks on quantum mechanics.[1]

We have now shown that quantum theory can claim validity under all conditions and yet leave classical mechanics in a position to do business in its good old way. For classical mechanics is the limiting form of that larger theory, the form in which uncertainty and quantization hide themselves in the deep shadows of *ordinary* imprecision.

[1] E. U. Condon and P. M. Morse, "Quantum Mechanics," p. 51, McGraw-Hill Book Company, Inc., New York, 1929.

Summary

Uncertainty or *indeterminacy* in quantum mechanics is a consequence of the change in the description, or explanation, of the facts of physical nature, a change described in the preceding chapter; uncertainty arises from a fundamental shift in the meaning of physical reality. Above all, it is not a mere result of the recognition—albeit a correct recognition—that certain operations cannot be performed. While the usual treatment tends to make the Heisenberg uncertainty principle appear like a proscription of certain experimental procedures or at best as a thesis of limited measurability, it represents in fact a basic departure from the customary approach to reality.

In the present chapter, the principle is first demonstrated in a general way with the use of the axioms of Chap. 17. Its specific bearing upon physical operations is then investigated by examining numerous examples. It is found to be inadequately characterized by the favorite phrase, "indeterminacy is due to the interaction between systems and measuring devices." Often this can be shown to be true; but the principle may also be construed as implying a noncommittal attitude with respect to the wave-particle nature of the system, as a failure of attempts to visualize entities of the microcosm. Philosophically, the uncertainty principle may be said to be a manifestation of what has been called the haziness of Nature.

The analysis of uncertainty rests heavily upon what one agrees to regard as a measurement. A mildly polemic section (18.4) is devoted to this problem, and it is indicated that physicists often talk loosely, confusing measurements with physical operations; indeed mere preparation of a state is often taken to be a measurement. The meaning of uncertainty becomes clearer when a more restricted interpretation of the measuring act is adopted and consistently employed. Quantum mechanics is similar to biology in the need it often has for *destroying* systems by the act of measurement, and the consequences of this necessity must be carefully drawn.

In Sec. 18.8, quantum mechanics is shown to "reduce" to classical mechanics for systems of large mass and/or systems free

to move without effective confinement. Ordinary bodies of our daily experience conform to these conditions. It is therefore proper to regard quantum mechanics as the more general discipline, which takes the form of ordinary mechanics in all circumstances under which the latter is known to be valid.

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CHAPTER 19

Causality

19.1. Total and Partial Causes

The words *cause* and *effect* are among the most loosely used in our language. Elsewhere in this book, when we faced a similar tangle of usage and desired pentecostal illumination, we turned trustingly to science for a decision on the proper meaning of words. Unfortunately, we shall find science of no help in our present quandary, for cause and effect are not primarily scientific terms, despite widespread opinion to the contrary. Science uses them with no less a variety of meanings than does common speech, and, it may at once be noted, the more sophisticated mathematical investigations of science do not use them at all. When scientists talk about causality, they do not talk as experts in a technical field, as they do when discussing the meaning of force or energy or enzymes or mutations. The following pages contain ample evidence of this.[1]

Causality presents no problem if we are willing to accept the most indefinite of all possible cause-effect relations, namely, that which speaks of causes simply as attendant circumstances of an event, process, or thing. In that loosest sense, pneumonia may be the cause of a person's death (although patients often recover from this ailment), the sun's attraction the cause of the earth's motion in an elliptic orbit, the sculptor the cause of a statue, and the triangle the cause of the fact that the sum of its angles is 180°. What these statements have in common is a reference to a very vague kind of relation between two situations, exhibiting hardly more than an opportunity for us to say, in retrospect: Had it not been for the one (cause), the other (effect) would not have

[1] The damaging implication with respect to the contents of this chapter is intended.

occurred. No assertion is made as to an *invariable* sequence of the form Whenever A, then B; for it is not at all true that all patients die from pneumonia; nor does it mean Whenever B, then A, for angles may well add up to 180° if they are not in a triangle. *Temporal* sequence is not involved in all instances either, the sun's attraction and the earth's motion being simultaneous. The last example, which concerns the angles in a triangle, shows nothing more than an analytic implication. In this loosest form, illustrated by the foregoing sentences, the causal relation has no interesting logical properties of its own; it is a mixture of several other more carefully statable relations with which mathematics and logic have effectively dealt. We do not propose to analyze it here.

On the contrary, we shall seek the most restrictive formulation of the causal principle which is in keeping with ordinary discourse, the restriction arising from a desire to establish a relation that is unique and precise and can be lifted above controversy. The way to find it is clearly to analyze a sufficient variety of accepted causal statements, to find what they have in common, and then to formulate this common element with a maximum of precision. Of course there is no assurance that this procedure will not turn up some academic, freakish caricature of what cause and effect usually mean. Whether such is the nature of our results must be left for the reader's judgment.

We start with a list of further propositions, assembled here for inspection and later to be scrutinized. Their order reflects a certain purpose which will become evident as we proceed. It will be observed that the relation of logical (deductive) implication is left out of the causal scheme. This is because we agree at the outset not to count connections like those expressed in geometric theorems as causal ones, for otherwise we should be wasting a good term on something clearly known and analyzed in other adequate ways, and so far as linquistic usage is concerned we should be confusing *reason* and *cause*. The list now follows.

1. The acorn is the cause of the oak.
2. Killer John is the cause of Harry's death.
3. At the opening of the 1933 Chicago World's Fair, a beam of light from a distant star was focused on a photocell, which in turn operated a relay, and so forth. It was then said that emission

of light far away in space and long ago in time was the cause responsible for the opening of the World's Fair.

4. Alcohol is the cause of an automobile accident.
5. Force is the cause of motion.
6. Overproduction is the cause of depression.
7. The movement of two objects toward a common point is the cause of a collision.

This list appears to be representative of the various ways in which the word *cause* is actually used.

We first ask: As elements of experience, as entities, what are cause (C) and effect (E)? In Example 1 both C and E are *things*. In 2, C is a particular thing (a person) and E is an *event*. In 4, C is a class of things, and E a class of events. In 3 both C and E are events or occurrences. Example 5 is hard to classify from our present point of view because force may be a datum (muscular sensation) or a construct. In Examples 6 and 7, C and E are both *temporal phases* in a given process. Overproduction and depression are stages in a continuum of economic changes; movement of object prior to collision and collision are two states in a process of motion. It may thus be concluded that causes and effects, as they are commonly understood, can be (*a*) *things* (particular things or classes of them), (*b*) *events* or occurrences at different points in space and time, or (*c*) *stages* in the same continuous process. Cause and effect are never, so far as we are aware, taken to be immediate sense data; we do not say that the visual perception of lightning is the cause of the auditory perception of thunder. Hence we have at once eliminated raw data from our concerns.

Next it is well to consider the question as to the uniqueness of the causal relation. In what instances would it be most proper to say that the cause assigned by the proposition is the only cause or the only significant cause? This will provide us with a welcome criterion for retaining the most tractable version of the causal relation.

A little reflection will show that, whenever cause and effect are either *things* or *events*, the relation is least unique. In the first proposition of our list, soil, sun, and rain are certainly causal factors in addition to the acorn. In the second, if John is the cause of Harry's death, why not the gun or the surge of anger that took possession of John before he fired? The light beam which caused, supposedly, the opening of the Fair is so remote an agent that half a dozen others could easily be cited as more proximate causes. And so on down the list until we come to item 6. Here one begins to wonder whether any factor in the economic situation prior to the depression could be said to be its cause quite so significantly as the state of overproduction. The causal relevance of that state could perhaps be made more clinching by allowing the word *overproduction* to include various other economic anomalies which usually attend this state, such as aberrations of the prices of stocks and goods, low discount rates, and low wages. At any rate, by exhibiting as cause the *entire* state of economic affairs preceding the depresion one would stand a very small chance of being wrong.

But this would seem trivial, for it would amount to saying that the sum total of all events preceding a given set of events is the cause of the given set. And yet, if we leave out *some* prior events, the cause does not seem quite complete.

The suspicion of triviality arising at this point is dispelled when we note that in our sixth example we did not refer to the state of the whole universe or even of the whole nation prior to the depression as the cause of it; we pointed to a specific *economic* state of affairs. There was involved, to be sure, a "system" called the national economy. That system underwent changes of state, and it was noted that one state characterized by the word overproduction preceded a state called depression. In so far as the meaning of these terms is clear and in so far as the former invariably precedes the latter, their relation is a causal one. And causality is then far from trivial.

An extreme example of this specific meaning of causality is present in the seventh item of our list. When completely analyzed it asserts that there exists a system made up of two particles, the fate of which is independent of the rest of the physical universe. In the succession of states through which it passes there is one, convergence of motions, which is claimed invariably to precede another, contact of the particles. The example, being so simple, is not of great scientific interest. Much of its importance, however, arises from the circumstance that it is possible to select from the universe so limited a system and so succinct a pair of states in constructing the causal linkage.

In summary, then, a cause becomes unique when it refers to a stage in a process involving the whole system under consideration. Or, to put it in terms of our previous analysis, it becomes unique when it refers to the entire *state* of a physical *system*.

The reason why the causal assignment of the first examples in our list was somewhat indefinite is that the causes did not embrace a sufficiently large situation. They were what we shall henceforth call *partial causes*. To illustrate the difference between partial causes and *total cause* we consider again Example 2. John, gun, bullet, anger are all partial causes; the total cause is a situation describable in this way: Harry is alive; John is angry and holds a loaded gun, etc. The total effect would be: Harry is dead; John holds an empty gun, etc.

Let it also be observed that a given total effect has an infinite number of total causes distributed through a temporal sequence, but only one total cause at a given instant. Every frame of a movie film picturing the murder presents a cause of situations presented on later frames.

Chiefly because an analysis of partial causes is never unique, the term *cause*, unless qualified, shall hereafter in this book always mean total cause. We can make no contribution to the endless debate about the causal problem unless this restriction is rigorously maintained.[1]

One final inference is to be drawn from our list of causal instances. All examples have this in common: the cause precedes the effect in time.

When the conclusions noted thus far are compounded, the principle of causality asserts the following: Let *A* and *B* be com-

[1] Frequently a partial cause can supply the total cause through an understanding of *ceteris paribus*, as in the statement: A loose bearing causes an engine to knock. What is meant here is that the state of the system, the engine, is normal except for the one explicitly mentioned defect. In such instances, description in terms of partial causes is of course perfectly proper.

plete states of a specified system at times t_1 and t_2, t_1 being earlier than t_2. If A is realized, B will certainly follow.

The truth of the principle is not obvious, and the principle can be wrong. For it may happen that no system can be found with reference to which causal analysis can be conducted. Clearly, if we were driven to choose the entire physical universe or all of experience, we should count our causal investigations as failures. Or it might be that a system is patently given but that to define a state in causal fashion is impossible. If and when these results occur, we do not readily abandon causal description; we endeavor to redefine systems and states. Hence, while the principle has positive content, it appears nevertheless in the role of a metaphysical maxim as explained in Chap. 5.

19.2. History of the Problem of Causality

The main purpose of the present section, very inappropriately entitled History of the Problem, is to draw attention to the fact that the problem does possess a long and interesting history which defies condensation into a single chapter of a book. Among the more recent works Cassirer's [1] presents a beautiful perspective, and Frank's [2] book will lead the reader through illuminating discussions of the subtler questions of causality, both historical and factual. Frank's treatment is primarily critical. Taking causality to imply positively discernible information, and regarding the principle of causality as operating on the same plane as the laws of nature, his book endeavors to show that the principle achieves little which the laws of nature cannot do, in fact that it is often a very blunt tool for scientific use in comparison with special laws. This positive attitude leads to the somewhat negative conclusion that the role of the principle is not an important one. The present analysis, which acknowledges the regulative function of causality with perfect candor, saves much of its usefulness in the methodological sphere and ends with a *specific formulation* of the causal

[1] E. Cassirer, "Determinismus und Indeterminismus in der modernen Physik," Elanders Boktrykerie Aktiebolag, Göteborg, 1937.

[2] P. Frank, "Das Kausalgesetz und seine Grenzen," Springer-Verlag, Berlin, 1932.

postulate. It thus opens itself to censure or confirmation more effectively than the predominantly critical efforts of many other writers.

Perhaps the oldest known statement of the principle of causality in Western science is that of Democritus, who said: "By necessity are foreordained all things that were and are and are to come." More elaborate was Aristotle's treatment of the causal problem with its distinction between formal, material, efficient, and final cause. In this quadripartite arrangement we see the source of much confusion; the problem, whatever it may be, was rendered incapable of solution by Aristotle's dissection. For it focused the philosopher's attention upon partial causes, which are never unique. Furthermore it shifts the emphasis away from the feature of inevitability, which is clearly inherent in Democritus' formulation and which forms an essential part of the causal relation as we understand it today.

It is characteristic of Aristotle's views that they permit causes or effects to be things, events, stages in a process or processes themselves, that is, permit a promiscuity which, as we have seen, naturally attends the choice of partial causes. The tendency thus initiated was continued and amplified in the Middle Ages and resulted in a proliferation of causes which now seems ridiculous. But it illustrates the point that, if one starts to make a list of partial causes, such a list will never be complete.

On Hume's famous exposition we cannot dwell, except to point out a fact or two which make it memorable though it is no longer acceptable. Hume has not made the transition from partial to total cause which was effected later by men more thoroughly versed in natural science and mathematics, for he still speaks of cause as "an object precedent and contiguous to another." In spite of this defect there is a most important lesson to be drawn from his treatment of the problem. Seeking causes and effects within the realm of the immediate, or as close to it as possible, Hume convinced himself rightly that every possibility of establishing *necessary connection*, that is, inevitability of the causal relation, proves illusory. There is no causality among matters of fact, among data, as has been indicated frequently in earlier chapters of this book. And this conclusion is left intact even when the transition from partial to total cause is made.

Kant's contributions to the problem of causality are important because he assigns a new place to the causal relation; he lodges it among his categories, where it can properly attain the degree of reliability to which it has always laid claim. As is well known, he thus avoided the Humean impasse, which nearly destroyed the significance of causality altogether. In claiming it to be of a priori validity Kant was certainly wrong, since well-established scientific relations of a causal sort have often been at the mercy of contingent fact and causality itself has been meaningfully questioned. But more deplorable is the fact that Kant, despite his remarkable analytic powers, did not endeavor to clarify the meaning of causality in a way to make it accessible to satisfactory treatment.

In the first edition of the "Critique of Pure Reason" one reads: "Everything that happens (begins to be) presupposes something which it follows in accordance with a rule." Nothing is said about the nature of the rule, and this omission makes the statement quite indefinite. Nor is the term *everything* (*alles*) designed to allay the Aristotelian confusion. The latter fault is corrected in the version given to the principle in the second edition of the same work: "All changes occur in accordance with the law of connection between cause and effect." But again, precisely what this alleged law entails is not made clear. The shift to *changes*, however, is significant and illustrates the more analytic temper of the times; it recognizes causes as stages in a temporal process, as total causes, distinct from specific things or events.

The clearest evidence for this shift is found in the classic formulation of the problem by Laplace, to which we now turn. As we do so, we also move to another stage of our inquiry. Hitherto we asked: What is the meaning of the relation between cause and effect? In a qualitative way this is now established, and our interest forthwith seizes upon what may be called the *principle* of causality, the assertion of general validity for the causal relation. In particular, we must find the precise meaning of the causal relation which will make the principle true in scientific instances where it is known to hold.

19.3. Laplace's Demon

According to Laplace the principle expresses that quality of our experience by whose virtue the following statement can be made: [1]

> An intelligence which knows at a given instant all forces acting in nature, as well as the momentary positions of all things of which the universe consists, would be able to comprehend the motions of the largest bodies of the world and those of the smallest atoms in one single formula; provided it were powerful enough to subject all data to analysis. To it, nothing would be uncertain, both future and past would be present before its eyes.

The intelligence here involved, which presumably transcends human limitations, has been called a *demon*. Whether or not this word is appropriate, we shall use it because of its brevity.

The demon has been a bone of contention. Positivists object strongly to his being invoked for the solution of a scientific problem and regard him as a *deus ex machina* who forces the issue in an unnatural way. They would prefer to eliminate him, and with the full approval of most scientists. The endeavor to do this leads at times to another extreme position, which holds causality to be valid when *man*, or the *scientist*, is able to achieve what Laplace asked of the demon. Causality then becomes a question of whether the scientist, knowing the state of the universe at one time, can in fact predict it for all times. Since the answer to this question is obvious (for he certainly cannot), the problem becomes uninteresting. A more careful disposition of the demonic problem must therefore be made.

Laplace meant his demon to be a great mathematician, indeed one who is versed in *all* the tricks of that trade. He introduced him as an ideal arbiter of the mathematical consistency of the situation expressed in the quotation above, as one qualified to pronounce the judgment which the mathematician renders when he says: The solution of an equation exists. This judgment is meaningful even if no computer on earth has found the solution. When this interpretation is accepted, the criterion of causality is the existence of a world formula as envisioned by Laplace. And

[1] Laplace, "Théorie analytique des probabilités."

the word *existence* is to be understood in its strict mathematical sense. With this interpretation, which was clearly intended by Laplace, the lapse of the whole problem into the trivial is avoided.

The demon is to have knowledge of the positions of all "things." Laplace knew Newtonian mechanics and doubtless meant by the positions of things the dispositions of masses in the universe. The early date of his writing (1820) ensures that he could have nothing else in mind, and indeed the context of our quotation bears this out. To him, the universe was an aggregate of mass points, and the knowledge possessed by his demon was knowledge of the "state" of this aggregate in the accurate sense in which the term was defined in Chap. 9. It includes position and velocity of every mass point—although the word velocity does not appear explicitly in Laplace's statement. But when the state of this mechanical system at one time is given, Newton's laws allow its calculation at all times. Mechanics is therefore a causal discipline, and the cause of a given state is the whole state at some earlier time. Each effect has an infinite number of causes, but only one cause at one time. In a way, Newtonian mechanics is the model of a causal discipline in Laplace's interpretation of causality; and when the question as to the validity of the causal principle throughout the physical universe is raised, one simply asks whether all of physical science has the same formal structure as mechanics.

But there are defects in the Laplacian version of causality even after it is given the benefit of this sympathetic interpretation. One difficulty arises from its appeal to the entire universe as the system of which causality is predicated. Not only the feasibility of finding a solution but even the mathematical existence of a solution is seriously drawn into question when the number of particles becomes infinite. It may then be altogether idle to talk about causality. The obvious remedy for this Laplacian disease is to relieve the formulation of its reference to the whole universe, and to apply it to smaller systems.

Were it not for the important fact that systems, finite in extent and in the number of constituent particles, are often known to have a fate which is independent of the rest of the universe, causality would be a futile subject. The availability of finite "closed systems" is a precondition for causality to be meaningful.

The meaning of "closed" in this context will be examined with greater care in Sec. 19.5. The reader may here think of any of the simpler mechanical devices (mass moving in a gravitational field, a pendulum, any other undamped vibrator) or of the sun and planets as closed systems. Systems which conserve energy are always closed in the sense here employed.

Completely closed systems, however, are never found in nature and therefore display the "fictive" character of idealizations. Hence it is common practice to indict the subject matter of causality and therewith causality itself as having only a spurious relevance to actual experience. But this thoughtless attitude throws out the whole of physical science by forgetting that theories never refer to the immediate directly, they require an interposition of rules of correspondence.

All systems to which analysis can be applied are idealizations. This circumstance arises partly from the complexity and partly from what we have called the haziness of the given. To admit that closed systems are idealizations does not, therefore, in the least detract from their importance, for an idealization is that toward which physical procedures *can be made to tend*, and there are ways for distinguishing proper from improper idealizations. These things have been discussed before, principally in connection with the problem of external convergence (Sec. 6.5). For example, the smell of an atom is an improper idealization; the mass of an electron a proper one. A closed mechanical system belongs to the class of proper idealizations.

The occurrence of closed systems is by no means to be expected on a priori grounds. It is occasioned by such contingent facts as the rapid decline of all mechanical forces with increasing distance between interacting particles. Thus causality, even in the condition which it imposes on physical experience before it makes a positive assertion, implies something that is far from obvious. Its implication is: *There are closed systems.*

The other defect in the Laplacian statement is not so easy to remove. It is this: mere existence of a world formula is not enough to characterize causality; certain special things must be demanded of the formula. To see this we consider a system of n particles; for the present argument it does not matter whether the system is closed or not. Whatever its motion, the ith particle will have coordinates x_i, y_i, z_i, which are functions of the time t. If we now *assume these functions to have at least two derivatives*, all the d^2x_i/dt^2, d^2y_i/dt^2, and d^2z_i/dt^2 exist and they too are functions of t. Nothing can therefore prevent us from writing equations of the Newtonian form

$$\frac{d^2x_i}{dt^2} = F_{ix}(t), \text{ etc.} \tag{19.1}$$

and these equations constitute a world formula presumably acceptable to Laplace. It appears not as a single equation but as a set of $3n$ differential equations, which has mathematical advantages over, and could, if desired, be converted into, a single one. Furthermore their solution exists because it was our starting point.

Equations (19.1), however, were dependent only on one assumption: that the coordinates are twice differentiable, *i.e.*, that the motion of the particles is continuous. Does the principle of causality, then, reduce to the weak assertion that *natura non facit saltus*?

19.4. Causality and Prediction

The simplest answer, and one often given by scientists, saves the situation by equating determinism, the view that asserts our principle to be valid, with human ability to predict. We have already touched upon this matter, although none too seriously, and we have indicated that *de facto* no one can predict with accuracy. Shall we then say that our world is "approximately" deterministic? Or shall we yield further and say that accurate prediction at the moment is unimportant, since it is *confidence* in our ultimate ability to predict which counts? The last suggestions are unfortunate, for they convert causality into something very ill-defined if not into a mere vague belief about the future course of science.

On the other hand we shall sooner or later have to face the issue as to the *universality* of deterministic behavior. This may indeed be no more than belief, for it is clearly impossible to demonstrate a proposition about the universe on the evidence furnished by finite empirical knowledge. But if such a belief were founded upon the certainty that in some phases of experience causality does hold, it could be entertained as a reasonable extrapolation of what is known. Only in this sense can determinism be accepted, and we shall discover that at present some fields of science have reached the causal stage, while others have not. Having thus limited our scope we return to the question: Is determinism tantamount to predictability?

Of course it can be so defined. But many people, including the author, sense a curious incompleteness in this drastic disposal of the case and continue to have their curiosities unsatisfied. They feel that the roots of causality go deep below the phenomenon of prediction. Their uneasiness is made evident, perhaps by the following imaginary example.

It is often harder to reach agreement on positive than on negative issues, and in the present instance perhaps a *noncausal* situation is more readily sketched. It would seem that the clearest model of a noncausal universe is a world governed by an irrational god, or a devil, according to inscrutable desires. If this world runs smoothly, if its bodies move continuously from point to point, then, as we have seen, it satisfies Laplace's dictum, and this is why we felt the dictum to be in need of emendation. Now if the noncausal universe can also satisfy the disciples of the doctrine of prediction, that doctrine, too, must be improved before it can claim to define causality. And to satisfy them we assume that the devil lets them know, as prophets, each day what is going to happen. They can now predict, but has the universe become a causal one?

We admit that the example has but little logical force and will leave the predictionist unconvinced. One commits no error of reasoning by answering the last question affirmatively, but one violates an ingrained feeling which makes a distinction between caprice and causal behavior and which takes miracles, such as prophetic prediction, to be symptoms of *in*determinism.

The predictability doctrine puts the cart before the horse. That prediction has much to do with causality is of course not an accident, for if causality holds, then prediction is always possible. Our example cautions us not to state this relation the other way round. The Laplacian version already should have taught us this:

the existence of a formula in principle enables prediction, but prediction (of the prophetic type) need not presuppose a formula. It is our view that causality represents something stronger than and intrinsically different from human ability to predict. Precisely what it is will be discussed in the next section; here we shall first illustrate the distinction once more by something more plausible than the devil, whom we dragged in by the heels. We shall try to find a positively causal situation.

Universal gravitation was the prototype of causal regularity in Laplace's mind, and it still sets the pattern for causal thinking. We are therefore fairly safe in holding out the law of gravitation as a causal law and the situations which it regulates as exhibiting determinism. According to the law of universal gravitation every two particles of matter attract each other with a force equal to kr^{-2}, k being a parameter involving both attracting masses, and r the interparticle distance. This provides a theory which makes prediction possible whenever certain differential equations can be solved.[1]

Now the law contains two constants, k and -2. The word *constant* here implies invariability with respect to space and time. Poincaré would call the first an accidental constant because it may change from case to case, depending as it does on the masses of the particles. But the second is an essential constant, invariable in all applications.

What we wish to envisage is a situation in which the essential constant, -2, is replaced by a number still constant with respect to space but varying in time, let us say in periodic fashion. If this were true we should no doubt begin to search for a "cause" (partial) of this variation, perhaps in the form of an astronomical body which approaches and recedes from the earth with the period observed in the gravitational force, perhaps in the form of an alternately contracting and expanding universe. But let us assume that no such attendant effects are found and we are forced to recognize that, willy-nilly, the "constant" in the law of universal gravitation fluctuates. If the law of fluctuation is known, predic-

[1] This is not in general the case when the number of particles is greater than two. Hence, if *de facto* prediction were the necessary condition for causality, the motion of three bodies would be noncausal motion.

tion is still possible. But in the theory we are about to recommend causality would be said to fail under these conditions.

19.5. Causality as Temporal Invariability of Laws

On the wall of a room in a physics laboratory hangs a pendulum clock. The occupant of the room winds the clock at proper intervals but finds that it runs erratically. After a while his curiosity is aroused, and he decides to get to the bottom of the clock's curious behavior. His hunch is that the clock is not a "closed system," that something unknown to him is happening to it.

The first thing he discovers is a nearby radiator, which is on during the day but off at night. Careful observation indicates that the clock runs faster when the room is warm than when it is cold, and he concludes sooner or later that the oil used for lubricating the works is slowing down the clock at night because of its greater viscosity. The occupant decides, therefore, to keep the radiator on all night.

But still the timepiece is not accurate. There appear to be sudden changes in its rate during the day, not correlated with anything that takes place in the room. Before long our friend remembers the cyclotron in the room below, an instrument which he has thus far regarded as a menace only to his health. He now discovers that it is also responsible for this particular annoyance; every time the cyclotron magnet is turned on or off it jars the gears of the pendulum clock, and it has furthermore given the works a residual magnetization which makes its rate nonuniform.

Our friend, now more determined than ever, has the clock demagnetized and takes it to another room which is far removed from the influence of the magnet and of constant temperature. Now and then he enters the room and takes careful readings, but to his amazement the clock is still erratic. Almost at his wit's end, he mentions his remarkable experiment to the custodian of the laboratory, who admits with some embarrassment that he, too, has become interested in the clock, that he loves to tinker and has from time to time adjusted it.

This would appear to solve the mystery; but the unlikely story goes on. The clock was put in a constant-temperature room, way up in a tower of the laboratory, far from all imaginable disturbances. The room was securely locked and accessible only to our friend. And the clock still was erratic. At times it went twice as fast as normal, at other times much more slowly. Absolutely nothing could be done to make it run consistently.

Under these circumstances the physicist concluded that the clock was a closed system. Its behavior was completely uncorrelated with anything going on outside the clock. But he also concluded that it defied the principle of causality. It may be further recorded that he went stark mad after drawing this last conclusion, but our story officially ends before that incident.

The preceding tale illustrates two things: (*a*) the meaning of closed systems; (*b*) the failure of causality. Our job is now to invert the negative situation and to formulate the positive meaning of determinism. Laplace's statement, even when limited to closed systems, does not exclude the aberrant clock.

If the motion of the clock were described by the laws of mechanics in their accepted form, the coefficients (masses and forces) appearing in them would be functions of the time. When written symbolically the laws of motion are

$$f\left(\frac{d^2x}{dt^2}, \frac{dx}{dt}, x, t\right) = 0 \qquad (19.2)$$

where x represents any or all coordinates of the moving parts, t is the time, and f stands for some continuous function. The presence of the first derivative, dx/dt, may render the system nonconservative (it then loses energy as time goes on) but leaves it closed in our sense. Equation (19.2) is a second-order differential equation with t as independent variable.

A well-behaved clock, *i.e.*, a causal one, will satisfy an equation like (19.2), *but with the variable t missing from the parentheses*. The coefficients appearing in the laws of motion are not functions of the time. Here, then, we have the missing link whose absence made the Laplacian theory define mere continuity and not causality. But before we seize upon it, let us examine conversely what the explicit omission of t from a differential equation like (19.2) generally implies.

It is simply this: The equation is invariant with respect to the substitution of $t - t_0$ for t, provided that t_0 is a constant. In less mathematical language: Any motion which satisfies the laws of motion at time t will also satisfy them if it takes place at time $t - t_0$; the behavior of a system, observed today, is the same as its behavior tomorrow or at any other time if the system starts under the same initial conditions. The exponent in the law of universal gravitation is not a function of t which varies, say, between -1.9 and -2.1, and this is the reason why the motion of all astronomical bodies is independent of the absolute era in which that motion takes place. The absence of t in Eq. (19.2) is what we regard as the essence of the principle of causality. This version translates the customary statement, Whenever A, then B, into more dependable terms without modifying its crucial meaning; in this form the principle bespeaks a kind of consistency of nature which assumes that she remains true to her own precedents.

Causality holds if the laws of nature (differential equations) governing closed systems do not contain the time variable in explicit form. Laws of nature are understood in the sense explained in Chap. 8 and must not be confused with equations of motion, for which the foregoing statement is not true. In particle mechanics, where most of the foregoing considerations originate and where the historical root of the causal principle clearly lies, the laws of nature are differential equations of the second order, and the equations of motion are their solutions (with initial values of x and v assigned). The questions to what extent the principle is relevant, and to what extent it is true, in fields other than mechanics will be discussed later. First it is desirable to stop a moment and look at some of the things indicated and suggested by the formal statement to which causality has now been reduced.

19.6. Causal Necessity, etc.

With respect to *partial* causes the principle says exactly what is commonly understood by it, namely that, if all other causal factors remain constant, a given partial cause invariably produces the same effect. The "other causal factors" are simply the determinants of the total state which remain unspecified. When considering the motion of a body one might be tempted to say, "The cause of its being at point P now is its having been at point Q one second earlier." But this refers only to a partial cause, and the statement is significant only if one adds, "provided that the velocity is ten miles per hour in a certain direction." The state of a moving body involves knowledge of both position and velocity.

When the variables determining a state (*i.e.*, the partial causes making up the total cause) are unknown, the principle is always applied at the risk of some imprecision. For even an appeal to constancy of unspecified causal factors will not help if these factors are unknown. Yet this is nearly always the situation encountered when causal reasoning is applied to the affairs of our daily lives. Far from deprecating it, we merely record this fact together with its main significance: that it is hopeless to expect precision in causal controversies over most practical matters, including matters of conduct and matters of law.

Does a given cause always produce the same effect? And is a given effect always produced by the same cause? If state B is the solution of a differential equation for which state A furnishes a complete set of initial conditions, the two are connected inflexibly; B is exactly determined by A and A by B. Our restriction to total causes was partly motivated by the desire to achieve this unique connection. To see in a very simple instance why partial causes and effects fail to be unique we return to the example of the body moving from Q to P: Unless its velocity at Q is specified, the position at Q one second ago is not the only one that results in its present position at P; and, conversely, if we are told merely that the body was at Q one second ago, we can deduce nothing as to its present position.

Further caution is necessary if uniqueness is to be ensured. Not only must the complete state be given, but the complete system must be kept intact throughout all causal considerations. In the last example the system was a body or, to be quite definite, let us say a particle. The effect was the particle's having position P and a velocity v_P at time t_P, the cause its having position Q and velocity v_Q at time t_Q. These assignments are unique only so long as the particle is not subjected to forces; they are spoiled when forces are introduced. Forces, if present, must therefore be incorporated in the system, and one must speak of a particle subject to gravity, or a vibrating particle with a certain force of restitution, or a freely moving particle, and so forth, to specify the system. Similar care must be exercised in more complex situations. One might say, harking back to one of our previous examples, that Harry's death is the effect, not only of all the visible circumstances enumerated—John's standing where he did, holding a loaded gun, etc.—but also of the fact that he and Harry were in love with the same girl. Just as in the physical example the system, to be fully specified, involves a precise reference to forces, so the system of John and Harry here requires for its complete definition a statement of their fateful affections. But such analogies tend to be artificial because the concepts, system and state, so pivotal in the precise and complete causal judgments that are possible in science, are ordinarily ill-defined.

The *necessity* connecting cause and effect now exposes itself to view. It is not a peculiar attribute of the causal nexus, mysterious, powerful, and inexorable, although in a purely factual sense all these adjectives apply. The point is simply that we have found a way of describing our experience which renders the relation between two states, separated in time, unique (one implies the other) and invariable (the same implication always holds). The force of the principle of causality is methodological, arising from our success at analysis; the principle is a lesson drawn from and continually reinjected into constructive scientific procedures. It lifts the regularities expressed by laws of nature upon a plane of higher generality but does not make them more certain or more secure.

The idea of causality has undergone a transformation not unlike that of other scientific concepts, a change which is perhaps best illustrated in the idea of *force*. Even today, many scientists conceive of forces primarily as pushes and pulls transmitted through contact between bodies. The word calls to mind a sort of muscular exertion. This picturesque supplementation of scientific concepts is pleasing because it gives a poetic quality to inanimate dynamic agencies as, for instance, to the water turning a mill wheel or to the engine slowly dragging a freight train up a hill. In these cases it adds a harmless fancy. But it hinders a proper appreciation of such abstract influences as the force of the earth upon the moon, of a magnet upon iron, and of the so-called "Coriolis force." Here the idea must be stripped of its homely suggestions, purged of anthropomorphism, and established on a larger basis. In a similar way all of modern science tends in the direction of the general and the abstract, as we have already noted.

Causality, too, was originally viewed as a push behind events or as an agency meting out fate. Dynamic enforcement was the graphic term which expressed causal action, necessity was its mode. And somehow, necessity meant more than uniqueness and invariability. This sense is now being lost, and causality has transformed itself into a *methodological* component of scientific understanding.

We have made the claim that the principle of causality, when given the restricted formulation here proposed, is not tautological and can be contradicted by scientific experience. This would occur if the so-called constants of nature changed in time. No one can exclude this possibility from consideration. Changing laws of nature have been suggested by Dirac. A strong case for them has recently been made in a fascinating and well-documented conjecture of Pascual Jordan,[1] who presents evidence for a decline in the force of gravitation and for an increase in the total mass of the universe through spontaneous generation of stars. His physical laws contain the age of the cosmos in an intrinsic manner and are open to confirmation. If these suggestions should prove correct the principle of causality in its present form requires rejection or revision.

Several questions connected with Eq. (19.2) and with the verbal statement of the principle given in the last section still need to be answered. We said that Eq. (19.2) defines causality when the explicit argument t is absent from the function f. And in the verbal formulation we spoke of laws of nature in the form of differential equations. Is there a restriction on the order of these differential equations?

In particle mechanics the equations are of the second order,

[1] P. Jordan, "Die Herkunft der Sterne," Wissenschaftliche Verlagsgesellschaft, Stuttgart, 1947. In E. A. Milne's relativity theory, phenomena are causal in one time scale and not in another.

and particle mechanics is, as was seen, an exemplary pattern for all causal disciplines. But if one stipulated that causal laws shall always be of second order, he would find mechanics to be almost the only causal discipline. Such a restriction is therefore much too drastic. To admit equations of first and second order widens the range sufficiently to include practically all branches of physics; but it makes us wonder whether the use of third-order equations, if it were found necessary, could be said to defeat causality. A restriction on the order of derivatives seems artificial and foreign to the meaning which the term *cause* conveys.[1]

On the other hand, if no restriction whatever were imposed, causality would certainly lose its significance. For an equation like (19.2), even with t explicitly present, can ultimately be reduced to one of the causal form by a sufficient number of differentiations. Hence, unless the order is limited, causal description can always be achieved.

There is no way of avoiding this dilemma except to rely on actual scientific practice. Scientists do not accept a law of nature involving derivatives of arbitrarily high order. If they were forced to employ high-order differential equations, they would be uncomfortable and seek every possibility for removing this complication. But if their search were in vain, high-order equations would become acceptable causal laws. It is advisable therefore, and perfectly proper, to state no restriction as to order, except that it must be reasonably small.

If time-free differential equations for closed systems imply causality, what about open systems? To admit that a system is open makes our restricted formulation inapplicable but leaves the question of its causal behavior undecided, for the system may be part of a larger causal one. When the law of motion of an oscillator subject to an external driving force is written, the force appears as a function of the time and the resulting equation violates our definition of causal behavior. But the system is an open one, and the definition does not apply. If the driving mechanism is included in the description, explicit dependence on t disappears; the whole system is closed and causal.

[1] An equation of fourth order is obtained when the law of motion for coupled oscillations is written in terms of a single displacement. Physicists have not questioned causality in view of that circumstance.

19.7. Causality, Conservation Laws, Relativity

In Chap. 5, where we introduced the group of metaphysical principles which includes causality, we cautioned against mistaking these principles for neat pigeonholes serving as receptacles for specific items of knowledge. Science is an enterprise that defies static classification; its issues always overlap. Nowhere is this more clearly to be seen than in the present problem. Causality embraces many things, makes possible certain kinds of scientific description known under other names, and is implied by many specific laws. Yet it cannot be said to be identical with any one of them. Because there is always an opportunity in science to denote a specific instance of the causal relation by another name, scientists are not forced to appeal to it directly and sometimes scorn its use as vague.

Causality is a close relative of the laws of conservation of energy and momentum and also of the principle of relativity. It might be said to be the parent of the former law, a brother of the latter.

As to the conservation laws, they come about as follows: An equation of the type (19.2) with t missing possesses an "integrating factor"; multiplication by this factor converts the left-hand side into a perfect differential.[1] Its integral is then a constant, and the constant is given a name such as *energy* or *momentum*.[2] If motion takes place in more than one dimension, so that the quantities

[1] It is well known that a second-order equation in which the independent variable does not occur explicitly can always be reduced to a first-order one by the substitution $dx/dt = p$, $d^2x/dt^2 = p(dp/dx)$; but every first-order equation is integrable.

[2] The most familiar instances of this procedure are the derivation of the energy principle in mechanics and in electrodynamics. In mechanics, one starts with Newton's second law, $m\, d^2x/dt^2 = F(x)$, multiplies both sides by dx, obtaining $mv\, dv = F\, dx$, and then integrates: $\frac{1}{2}mv^2 - \int F\, dx = \text{const.}$ The quantity $\frac{1}{2}mv^2$ is called kinetic energy, $-\int F\, dx$ is called potential energy, and the sum of the two is "conserved" (see Sec. 9.2).

In electrodynamics, a similar integration of Maxwell's equations leads to the constancy of $\frac{1}{8\pi}\int (E^2 + H^2)\, d\tau + \frac{1}{4\pi}\int \mathbf{E} \times \mathbf{H} \cdot ds$. The first of these expressions is then interpreted as energy residing in the volume, the second as energy flux across the surface.

y, z, dy/dt, d^2y/dt^2, dz/dt, d^2z/dt^2, etc., also appear in F, the story is not quite so simple. Explicit absence of t is then not a sufficient condition for proper integrals (integrals which are point functions) to exist, and other conditions must be met. These are well known to the physicist and do not require elaboration here.

What we have shown is this: The causal structure of the laws of nature is always one of the conditions which lead to conservation laws. Precisely what physical quantity will be conserved can be decided only when the laws themselves are known. It is erroneous to say that the principle of causality merely asserts the existence of conservation laws; causality and conservation are not equivalent, but the former is a necessary condition for the latter; it is a weaker statement and lies deeper in the methodological structure of natural science. If further evidence is desired for the view that causality is *not equivalent* to conservation, we may recall what physics does with "nonconservative" fields: it continues its causal description (nobody would claim that causality breaks down in a vortex field) although a potential energy does not exist.

The spirit of the causal principle, long embodied in the natural science of all ages, asserted itself with renewed vigor in the demands of the theory of relativity. Noting that traditional science placed upon the laws of nature only the mild requirement of temporal invariance, Einstein insisted upon other, more stringent restrictions, upon invariance with respect to transformations in which the observer passes from one system of reference to another. The special theory of relativity maintains invariance relative to *inertial* systems; the general theory attempts to preserve it for all space-time transformations. In the deepest sense, therefore, the theory of relativity is a natural complement of the principle of causality and indeed its ultimate fulfillment.

This book contains no special chapter on relativity. The reason is obvious, for relativity is now part of classical physics. Its epistemology is not different from that discussed in connection with several specific disciplines (Chaps. 8 to 11), and it has seemed wiser to reflect the point of view of relativity throughout our treatment than to allot a special chapter to it. Relativity is no longer a subject hidden in a remote niche of physics; it pervades and dominates the entire field.

19.8. Causality in Other Disciplines

It will be clear that we have no desire to prove or disprove the *universal* truth of a principle of causation or to establish determinism as a *necessarily* valid scheme. Nor are we ready to say that there can be no science which fails to embrace the principle. But the interesting fact remains that at present all branches of science that have reached a satisfactory state of precision espouse causality as a principle of their methodology, indeed that they employ it in the form of temporal invariability of laws in which it has here been presented. This is shown in some detail in Table 19.1, where all features significant in causal description are collected for various branches of physics. Column (6) shows the type of law which connects a state [column (3)] at time t_1 with the state (similarly defined) at time t_2. The presence of entries in column (6) for all horizontal instances establishes causality.

As was indicated, there can be no unambiguous causal relation unless states are specified as functions of time and unless laws are given by whose means transformations of states in time can be effected. The states, in turn, must refer to some well-defined closed system which is self-sufficient for purposes of explanation.

A review of Chaps. 8 to 11 will clarify most of the symbols used in Table 19.1 and will illustrate many finer points of their meaning. In the first row, $\mathbf{p}_i$ and $\mathbf{r}_i$ stand for the momentum and position vectors of the ith mass point, the total number of mass points being finite. The separation of variables into three groups, variables of state, independent variables, and observables, which is shown in columns (3), (4), and (5), is somewhat artificial and has been made to stimulate thought about details. *All* variables in the three groups are of course observables, though sometimes latent observables. But among them, certain critical ones are discovered as peculiarly significant, as carriers of the whole of causal description, and are then selected to be variables of state. The independent variables form a sort of background for all physical description and are not characteristic of special systems. Time is always among them, and for continuous systems there are the space coordinates as well.

Table 19.1

Branch of science (1)	System (2)	Variables of state (3)	Independent variables (4)	Observables (5)	Type of Law (6)	Order of differential equation (7)
Particle mechanics	Set of mass points	$\mathbf{p}_i$, $\mathbf{r}_i$	t	Energy velocity angular momentum, etc.	Ordinary differential equation	2
Mechanics of rigid bodies	Rigid body	$\mathbf{p}$, $\mathbf{r}$, α, β, γ, $\dot{\alpha}, \dot{\beta}, \dot{\gamma}$	t	Energy velocity angular momentum, etc.	Ordinary differential equation	2
Continuum mechanics	Elastic continuum	S (strain tensor)	x, y, z, t	Pressure, energy, momentum, wavelengths, etc.	Partial differential equations	1 in x, y, z 2 in t
Hydrodynamics	Fluid	$\mathbf{v}$ (velocity)	x, y, z, t	Pressure, energy, momentum, flux, vortex strength, etc.	Partial differential equations	1
Electrodynamics	Field	$\mathbf{E}$, $\mathbf{H}$	x, y, z, t	Energy, momentum, charge density, current, etc.	Partial differential equations	1
Thermodynamics	Body (*e.g.*, gas)	P, V, T	None (or all)	Heat content, free energy, entropy, etc.	Ordinary equations	0
Statistical mechanics	Probability aggregate, involving distribution of molecules	Mean values of observables	x, y, z, t	Energy, velocity, momentum, etc.	Laws of probability calculus, coupled with those of point mechanics	Mixture
Quantum mechanics	Entities of the microcosm	φ (state function)	x, y, z, t	Energy, velocity, momentum, etc.	Partial differential equation (Schrödinger's)	2 in x, y, z 1 in t

The variables of state of a rigid body are **p** and **r**, momentum and position of its center of mass, plus three angles (α, β, γ) and their time derivatives. P, V, and T mean pressure, volume, and temperature, and these variables are proper for a gas. If another type of thermodynamic body were considered, this list might have to be extended.

Thermodynamics shows other remarkable idiosyncrasies: it is the only branch of physics in which t is not an independent variable; in a strict sense thermodynamics has no independent variables. Many observables are placed on the same footing, and analysis proceeds by playing them all against each other. The observational situation is usually such that any one of them can be varied at will while all others are kept constant, and hence any observable can be treated as independent. The final column shows a zero for thermodynamics because the "laws of motion" in the form that is considered basic (equations of state) are primitive equations, there being no special independent variable with respect to which one can differentiate "impartially." As every student of the subject knows, thermodynamics abounds with derivatives, but these do not function in causal prediction as we have used the term.

Quantum mechanics is included in Table 19.1. No need exists for treating it separately so long as we maintain consistently the present point of view. If any of the other disciplines is causal, then clearly quantum mechanics is also a causal discipline. To be sure, its observables are often latent, as we have duly noted. But this fact, though very important in a study of prediction, certainly does not abrogate its commitment to the principle of causation. We shall have more to say on that point in the next section.

More serious is the causal condition of thermodynamics which does not use the time variable at all in its undertakings. It merely says what happens in the long run when certain changes in its state variables are made, and it refuses to say at what time they will be completed. This accords perfectly with our insistence that the laws shall be time-free, but it fits the prescription so automatically as to make the scheme trivial. In thermodynamics, therefore, causal description degenerates to a level of relative insignificance; one may or may not wish to classify this science as a causal one for that reason, although it would be wrong to say that it violates causality. Like all other metaphysical principles, the causal one has neutral regions where its application is not worth emphasizing.

19.9. Causation in Biology

The reader will note that all branches included in Table 19.1 are parts of physics. The effect of this limitation is serious only so far as biology and the social sciences are concerned, since astronomy, chemistry, and all types of engineering are fundamentally present in our tabulations. To insist that a similar tabulation can be made for biology is perhaps not safe at the moment; at any rate it is the task of experts in that field. But we shall indulge in a few general comments illustrating our point of view.

Whether biology is a causal system or not, whether it uses a formulation of the principle of causality different from that used by the physical sciences are questions to be settled not by consideration of the *problems* of that science but by a careful study of their *solutions*. Talk about the complexity of organisms, about the sum that is more than its parts, usually winding up in the reminder that biology is a field for which causal analysis is inadequate, proves nothing. Aristotelian physicists have said the same thing about inorganic nature. Indeed every field of human interest in which significant variables (variables of state) have not been discovered is for that very reason complex and intractable, and to say that it defies causal analysis is merely to admit that it has thus far not been penetrated by the usual methods of science. What the causal pessimist meant to be a statement of fact is to the working biologist a challenge.

To settle the problem of causality in biological science, as in physics and everywhere else, attention must be given to those phases of the science which have reached the stage of solution. But when consideration is limited to important recent advances, for example those arising from the work on mutations and general genetics, where specific answers are at hand, the impression becomes strong that such work indicates complete reliance upon the deterministic structure of reality. These accomplishments all proceed from the conviction that the laws valid today will also be valid tomorrow.

Even the most fundamental sciences have causal gaps. The difficulties of the many-body problem leave a possible doubt regarding the exact lawfulness of the moon's motion; our ignorance of nuclear forces veils precise nuclear laws; the chemist, if he insists, can make a point of the random character of molecular actions so long as detailed forces between molecules have not been calculated. It is always easier to perceive chaos than order. But the reasonable scientist ignores the minor gaps in the action of causal laws and interpolates across them, as he does when drawing a smooth curve through a finite number of points.

A science enjoying the richness of data possessed by biology is likely to show a feature which, although present on the lower plane of physics, did not come to a clear focus there. It is the opportunity to represent one given empirical sequence by several different causal chains, and we shall call it the *multiplicity of causal schemes*. One of its simpler instances occurs in the different explanations offered for the same phenomena by thermodynamics and by statistical mechanics. The laws of thermodynamics represent a causal system at least in the minimal sense in which we are ready to accept the term. That causal mode of explanation proceeds with its own set of states and variables of state and remains entirely within the conceptual limits imposed thereby. Statistical mechanics, on the other hand, effects a causal reduction of the same observed facts, but in a different conceptual sphere. Its systems and states are different logical entities from those of thermodynamics; ultimate reference to the same body of immediate experience is achieved by different rules of correspondence, as we have seen. Confusion results only when the two are mixed unwittingly as, for example, when the probability laws regulating the aggregates of statistical mechanics are injected into thermodynamics as though they were propositions concerning thermodynamic experience. Aside from that, there is no harm in such multiplicity of causal schemes, which is the reflection of the circumstance that a single object of experience, like an ordinary body, can be a system of thermodynamics and of statistical mechanics at the same time. A similar multiplicity is apparent in the choice we have of regarding a given object as a set of mass points, a rigid body, or a charge distribution in an electromagnetic field. Nor does this pose a deep metaphysical problem; it is a reminder that the facets of reality are numerous.

In biology, multiplicity of causal schemes is probably important enough to be studied in its own right. It may give rise to levels of explanation, perhaps to an entire hierarchy of explanations, each a causal one, and each at a different stage of organizational integration. Thus there may be encountered a theory framable in terms of molecules and molecular forces, another one in terms of thermodynamic systems, another in which cells and cytological interaction are basic concepts, and perhaps one that speaks of stimuli and responses. If a prognosis can be based on physics, one may judge it to be a very long time before the vertical connections between these schemes are completely understood.

The interpretation of causality advanced in this chapter, stemming from an analysis of method in the physical sciences, is not natural to the biologist and has rarely been employed in biological discussions. In a vague form it runs through all researches, but it is often displaced by other views that color biological reasoning. The stand that biological causation is different from causation in the inanimate world is still widely held. How that position is reached may be seen in an interesting paper by R. S. Lillie,[1] whose treatment typifies an approach which we have here neglected.

To him, the essential elements of causation are spatial and temporal contiguity. Phenomena change in causal fashion if spatiotemporal connections can be traced between them. Following H. J. Jordan in his use of the phrase "causal structure," Lillie asserts that "Interconnection of this type pervades nature and may be described as causal structure." From this point of view the problem develops further, as follows: "The biological question is: what gives coherence to the multiplicity of events in the system and confers unity on the organismic sequence, considered as a whole? This is the problem of integration, which must be regarded as the essential problem of biological causation."

[1] R. S. Lillie, Biological Causation, *Phil. Sci.*, 7:314 (1940).

At this point the problem of causality has been lifted out of its historical setting and has lost all those distinctive methodological features which make it worth discussing. What Lillie calls the problem of biological causation is the problem of biology, and causality is thus identified with biological research. It is probably not well to diffuse the issue to that extent. But to avoid such generalities it is necessary to curb judgment at the outset: causality must not be defined as mere spatial and temporal contiguity, which can be present without causal nexus. If one event is found to influence another, physical contiguity is thus automatically established. Radiation fields, action at a distance can always provide the interconnections which causality according to this view demands. But it seems that more is required, and we hold the additional requirement to be, not unity of the organismic sequence, but invariability of the laws that govern the sequence.

19.10. Causality in Quantum Physics; Complementarity

Although the causal status of quantum mechanics has been fully reviewed in the preceding pages, the controversial aspects of this lingering problem make a brief summary desirable. In the early breath-taking decade of discovery ending in 1935, no simple slogan save "violation of causal reasoning" was deemed sufficiently dramatic to describe the revolutionary qualities of the new knowledge. Meanwhile the novel science has had time to settle and to embed itself in the general structure of physics. It is now no longer an illusion to see causality restored, since with the recognition that φ functions are states which satisfy a law (Schrödinger's time equation) quite similar to those in the more accustomed branches of science, has come a synthesis with older views.

The causally evolving φ states are not immediately tied to single observations; they refer, as we have seen, to aggregates of observations. Classical description had become noncausal because observables, thought to be possessed by physical systems, had been found to be latent and had refused to give consistent values in repeated observations. This necessitated a reformulation of states in terms of probability distributions: rules of correspondence of a hitherto unexpected type had to be introduced *to restore causality*. But otherwise the states are just as good as ever, for there are many fields of mathematical science that operate with constructs quite remote from immediate experience. In quantum mechanics, then, the basic mode of description has remained unaltered, while the rules of correspondence have undergone radical changes.

It is true, of course, that a single measurement of an observable carried out upon an atomic system does not in general determine a state and hence cannot be the basis of causal predictions. Nor is it possible to know the outcome of a single measurement when full knowledge of a state (φ function) is at hand, for a state is not related to a single measurement. But a state at t_1 does entrain uniquely a state at time t_2. In a very fundamental sense this is no stranger than the circumstance that the mechanical state of a system of masses, even when completely known, allows no prediction of their color. For this purpose, positions and velocities would be the wrong variables. The analogy here drawn is correct provided that we do not let our expectations drift further than these statements warrant. It happens to be possible for physicists to *augment* the mechanical description of masses by introducing nonmechanical entities like charges regulated by electrodynamic laws and in that way, through coupling diverse elements of discourse, finally to account for color despite the blindness of mechanics with respect to that observable. This is a matter of good fortune, not guaranteed by the success of classical mechanics itself. In quantum mechanics, because of the latency of atomic observables, no mixing of discourse, no juggling of "hidden parameters" will wring knowledge of individual observational events from states. But states continue to evolve in causal fashion.

Those who deny causality in quantum physics revive the battle between Kant and Hume. If the causal connection is defined as existing on the P plane, relating immediate observations, then strict causality has been lost in modern physics. But this kind of causality, we fear, had been disavowed many times prior to the present era and was in fact stillborn. For did not Hume himself declare man's belief in it unwarranted, and did not every classical empiricist who interpreted causality as a nexus of immediate experiences expose it as an illusion? Kant's greatest achievement, as we see it, was to destroy the belief that laws of nature directly involve immediate observations. And if this is true, causality reigns in quantum physics as it did in the classical theory of nature.

Our whole analysis of scientific method, the emphasis we have placed on constructs and verifacts, our view of the rules of correspondence clearly commit us to this last position. We do not hold that scientific reasoning *must* be causal in order to be intelligible or to be valid. Continued adherence to the causal postulate is at present a more radical and unpopular tenet than its disavowal; yet we maintain it because *no deductive theory even before quantum mechanics defined states in terms of immediate observations*, and we should misjudge history and science if we were to ascribe the use of abstract states, states not linked *immediately* with observations, to quantum theory as a pioneer departure.

To summarize: Classical physics defined states in terms of observables possessed by systems. These states were causally related. But since possessed observables are linked directly to single observations, it did not matter much whether scientists kept their ideas straight. No factual harm was done if the relation between states was inadvertently transferred across the rules of correspondence into the immediate plane of perception. Atomic physics with its latent observables introduced statistical rules of correspondence, and while states are still causally related, a transfer across these more complicated rules cannot be made.

The practical consequences of this change are wholly beyond dispute among scientists. Humean causality is quite definitely gone. Single events of great magnitude cannot be said to have the picturesque single-event causes which classical mechanics envisaged: even though I know the exact state of a neutron (having a sharp energy) that is capable of setting off an atomic bomb, and know precisely the location of the block of plutonium that is ready to be exploded, I cannot predict whether the disaster will occur. The fate of the globe, as a single event, may hide itself within atomic uncertainty. All this is true. But we should add that it is not a very practical consideration, for I am unlikely to encounter a neutron having a sharp energy (and therefore an entirely unknown position), and if its energy is only approximately known, its position and the prospect of its encounter with the plutonium block can usually be predicted with a certainty sufficient for practical men.

An apparently very impartial view with regard to the problem of causality is taken by Bohr, who has staked the weight of his authority on a pronouncement called the *complementarity principle.* We quote Bohr's own summary of his principle at length.[1]

> A causal description in the classical sense is possible only in such cases where the action involved is large compared with the quantum of action, and where, therefore, a subdivision of the phenomena is possible without disturbing them essentially. If this condition is not fulfilled, we cannot disregard the interaction between the measuring instruments and the object under investigation, and we must especially take into consideration that the various measurements required for a complete mechanical description may only be made with mutually exclusive experimental arrangements. In order fully to understand this fundamental limitation of the mechanical analysis of atomic phenomena, one must realize clearly, further, that in a physical measurement it is never possible to take the interaction between object and measuring instruments directly into account. For the instruments cannot be included in the investigation while they are serving as means of observation. As the concept of general relativity expresses the essential dependence of physical phenomena on the frame of reference used for their co-ordination in space and time, so does the notion of complementarity serve to symbolize the fundamental limitation, met with in atomic physics, of our ingrained idea of phenomena as existing independently of the means by which they are observed.

Physics has a choice, Bohr says, between describing nature in terms of classical observables and in terms of abstract states, such as φ functions. The first choice permits visualization but requires that causality be renounced; the second forbids visualization but allows causality to be retained. And these alternatives can never be reconciled. It is clear that Bohr interprets causality in Humean fashion so far as the first choice is concerned, in Kantian fashion when he speaks of the second.

[1] N. Bohr, *Nature*, 131:422 (1933). The best exposition of Bohr's views, not available when this was written, may be found in "Einstein," The Library of Living Philosophers, Ed., P. A. Schilpp, 1949.

No fault can be found with the factual content of the complementarity principle, nor can one deny that it conveys the restless spirit of modern physics in a most serviceable way. But as a basic epistemological discovery it has some serious disadvantages which must be pointed out. Bohr does not ask science to make a choice—he asks science to resign itself to an eternal dilemma. He wants the scientist to learn to live while impaled on the horns of that dilemma, and that is not philosophically healthy advice. We believe that science, in all its applications of quantum mechanics (which involve Schrödinger's equation), has in fact made its choice, and its choice was the second alternative.

Or, to look at the situation the other way around, Bohr's principle puts nature on the fence and leaves it there. Moreover, it appears in many of his writings that he is happy to have it balanced and believes it will stay on the fence forever. Now there have been many periods in the history of science in which rival theories competed for victory, but ultimately a decision has always been reached. The agnosticism implied when complementarity is accepted is a dangerous thing, for it provides pseudo solutions for other dichotomies. It invites the speculation that the mind-body problem, the organic-mechanistic aspects of living matter, the problem of free will are merely manifestations of complementarity and have now received the full degree of elucidation of which they are capable. To be sure, Bohr himself[1] has counseled against such misapplications, but physicists have at times found the temptation very strong.

19.11. Cause and Purpose

According to Sec. 19.5 certain differential equations with time-free coefficients are the hallmarks of causality. But physical science also contains laws which are expressed as integral equations, and these can be regarded as the modern carriers of the Aristotelian final cause, now called purpose. To see this, consider Hamilton's principle,

$$\int_{t_1}^{t_2} L\,dt = \text{minimum} \qquad (19.3)$$

[1] N. Bohr, *Phil. Sci.*, 4:289 (1937).

L is a function of the mechanical variables of state, the velocity (v) and position (x) of a particle now (at time t_1) located at a point P. These variables in turn are functions of t, the time, and one wishes to know *what* functions they shall be; for when v and x are given as functions of t, the path of the particle is determined. Equation (19.3) says that these variables depend on t in a manner which will make the time integral of the quantity L as small as possible while the particle proceeds from t_1 into an unknown future. More briefly put, nature "wants to" conserve its precious L, and she adjusts the particle's motion with this "end in view." That is indeed the closest contact made anywhere between physical science and purpose. Hence the question of teleology in its most nearly scientific form reduces to this: Are certain aspects of physical nature to be described in terms of integral rather than of differential principles? We believe that this is the form of question to which the "goal-seeking tendency" must ultimately submit itself.

To gain a partial answer we note what the physicist does when he applies Eq. (19.3). By means of a trick well known to mathematicians (finding Euler's equations, which are necessary conditions for having the integral a minimum or a maximum) he converts the integral relation into a set of differential equations called Lagrange's equations, and these are of the causal type. He has thus—this may come as a shock to metaphysicians—*transformed a purpose into a cause.*

Apart from clear analytic formulations, purpose and cause mean very little indeed. Does the stone fall to the ground *because* its present position is one of a sequence that terminates in making contact with the ground or *in order* to make its present position one of that sequence? When I seek a goal, I am in one sense achieving a purpose, but is not that goal, regarded as a psychological motive, also the cause of my actions? De Nouy,[1] in endeavoring to establish the need for purposes in the process of evolution, permitted himself an interesting lapse. He likened the development of life to the gradual passage of small rivulets of water down a hillside. Mechanical forces, due to obstacles in the water's path, cause innumerable deviations from the straight-line

[1] Le Comte de Nouy, "Human Destiny," Longmans, Green & Co., Inc., New York, 1947.

downward trend. These incidental forces, De Nouy holds, can be explained in causal terms, but the downhill tendency is a teleological one and is superposed on the mechanical causes. The error is obvious, for we know that the force of gravity is no less mechanical and no less causal than the forces due to stones in the water's path, but the error is instructive in this respect. For a scientist who understands the workings of the obstructions but does not, at a given stage of knowledge, understand gravity, the former would appear as causes and the latter as a purpose. What seems now to be a purpose may become a cause when the horizon of understanding is widened.

It is fortunate, therefore, that natural science does furnish a rigid frame to which the cause-purpose controversy can be attached. Are the workings of nature such as to *require* integral formulations of its laws? There are, to be sure, integral formulations like Eq. (19.3), among them [1] Fermat's principle of least time, Gauss' principle of least constraint, the principle of least action, Hamilton's principle; but the question is: Can these and all other integral principles be transformed into differential equations?

It seems at the present time that they can be so transformed. This view has been defended by Born, who emphasized the spurious character of purposes injected into nature. All theories of modern physics that are reasonably complete and employ integral equations permit the transformation in question, and whether the integral or the differential form is used depends frequently on convenience. Even in quantum mechanics both alternatives are at hand; the variational (integral) principle very often provides an easier solution of a given problem than its practical equivalent, the Schrödinger equation.

In all fairness, however, we wish to record that Planck, even to the end of his tragic life, looked upon the validity of integral principles as monumental demonstrations of purpose in the universe.

To the student of psychology and of social science the preceding remarks must seem like an undignified dismissal of the pur-

[1] See Lindsay and Margenau, "Foundations of Physics," John Wiley & Sons, Inc., New York, 1936.

pose problem. It should be remembered, however, that we are dealing only with questions to which the present methods of natural science can effectively provide answers. The limitations imposed by our framework can be placed in evidence by stating the problem in the form: Does *nature* involve purposes? If, at the outset, purpose is regarded as a concomitant of consciousness, the problem falls within the province of introspective psychology, where the methods of natural science have not come as yet to full fruition.

Summary

To render the meaning of the cause-effect relation precise it is necessary to distinguish between partial and total causes. A partial cause may be a thing or an event, whereas a total cause is always a stage in a process. The most definite formulation, and the one here adopted, is in terms of the states defined in Chap. 8.

Laplace's doctrine of causality is the epistemological interpretation of Newtonian physics. In focusing attention upon the existence (in the mathematical sense) of a world formula and in shifting emphasis away from mere human ability to predict, Laplace's theory achieves the correct perspective. But it describes nothing more than continuity of motions. To convert it into an accurate statement of the causal principle an important restriction is to be placed upon the world formula: it must be a differential equation or a set of differential equations which do not involve the time variable explicitly. This added restriction may be shown to have as an important consequence the validity of conservation laws (energy, momentum, etc.). Furthermore, there is a close affinity between the causal principle thus modified and the postulates of the relativity theory.

A brief inspection of nonphysical sciences, particularly biology, brings to light a universal concern for causality in this amended form. To indicate the extent of penetration of causal analysis into the various parts of physics a table is given (page 413) in which the causal features that are significant from the present point of view are exhibited in detail. Among other things, this study suggests that thermodynamics is the least causal of the branches of physical science.

The question as to the function of cause and effect in quantum mechanics is reopened, and it is shown that this discipline does not depart from the methodology of the older theories if states are interpreted in the manner explained in Chaps. 16 to 18. Bohr's principle of complementarity is reviewed and shown to be a correct but noncommittal solution of the general problem.

An attempt is finally made to assess the meaning of purpose as distinct from cause. It appears that purpose is frequently injected into situations which are not fully analyzed and that purposes can often be transformed into causes when the complete solutions are at hand. But it is not possible to say in the present state of science that such transformations are always possible.

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CHAPTER 20

The Exclusion Principle

20.1. Its Origin and Function

THE EXCLUSION PRINCIPLE plays an important role in quantum mechanics and has effects that are almost as profound and as far-reaching as those of the principle of relativity in classical physics. There are, in fact, striking similarities between the two, similarities with respect to the methodological plane on which they operate and with respect to the restrictive quality they exhibit. Both principles enact vetoes on a very basic level of physical description.[1]

Relativity demands that laws of nature which do not conform to its requirement of invariance shall be dismissed from consideration; the *exclusion principle* says that states (in the quantum sense) which fail to have certain mathematical properties are not realized in nature. Needless to emphasize, both principles are verified successfully in elaborate and painstaking ways, and the legal flavor of the preceding statement is unfortunate; yet it is a fact that relativity and exclusion guide research as procedural vetoes and not as descriptive maxims derived from experience.

Because they play this role, one is almost tempted to list these principles among the metaphysical requirements of Chap. 5. Certainly, they are more basic and more general in their application than the special laws of nature which they force into conformity with their demands. On the other hand, such demands are somewhat more specific than those of logical fertility, extensibility, causality, and so forth, and the discovery of both principles is so recent that their complete amalgamation with the more traditional methods of science has perhaps not taken place. And it should also be said that exclusion at present shows marks of be-

[1] A third principle of this type, as yet not completely developed, is Born's reciprocity postulate. For its exposition, see M. Born, *Nature*, **163**:206 (1949).

ing incompletely understood, raising hopes of even greater discoveries to come, discoveries for which the exclusion principle is but an early harbinger. In view of these facts we list the two remarkable vetoes as principles of physics rather than including them among the metaphysical requirements of all exact science. The day may come, however, when perhaps after some metamorphosis they should be thus promoted.

The fundamental importance of relativity has been recognized by philosophers from the very beginning. Discovered at a time when little else was going on, Einstein's new theory fascinated the scientific world and captured in particular the imagination of mathematicians. It immediately resolved problems known to many and of long standing, problems which by their very obstinacy had impressed themselves as important on the minds of all physicists. The solution was bold and had implications that were tantalizing to the nonscientist. Thus it came about that philosophical fancy was aroused; relativity was studied in its methodological aspects, and a fruitful interplay between physics and philosophy took place.

The exclusion principle was discovered by Pauli in 1925; it illuminated at once an enormous field of physical facts and was accepted with wide acclaim by physicists. But it was born amid a flurry of factual discoveries at a time when the whole new structure of quantum theory came into being, and therefore it was regarded by many as but another interesting theorem comprised within the formalism of the new discipline. Its success in solving problems was greater even than that achieved by relativity, but the problems it solved were very technical and therefore of interest to few, such as the problems concerning the details of atomic structure; or they were so old that people had become accustomed to living with them, as for instance the problem of chemical valence. As a result, the principle was embodied in the axiomatics of quantum mechanics; its peculiar methodological significance passed from view.

20.2. Statement of the Exclusion Principle

There are several ways of formulating the principle. One enjoys particular favor in the more elementary treatments of the quantum theory because it is couched in terms of models and allows ready visualization. We shall call it the *simple* formulation. Unfortunately, the philosophical importance of the principle is not fully apparent in its simple form, which makes it look like an incidental proposition regarding certain elementary particles of physics. To recognize it as a regulatory standard of great generality the meaning of exclusion must be stated in another, mathematical way. The next section will be devoted to this *mathematical* formulation, which, for the sake of completeness and for reasons of pedagogy, will be preceded by the simple statement.

Pauli discovered his principle in analyzing the motion of *electrons*. Although it is also applicable to other entities (this will be shown later), we shall at present consider it only in connection with electrons, in fact only in relation to the problem of atomic structure. To start, we recall an ancient dictum, presumably scientific, which says that two bodies, or particles, cannot occupy the same place at the same time. This impenetrability axiom, with due refinements, may be considered as the precursor of the exclusion principle, for the latter says: *No two electrons can be in the same state of motion*. But a good deal of interpretation is necessary before this phrase becomes meaningful. For what is a state of motion?

In classical physics the state of motion of a particle is defined as the aggregate of its coordinates and its velocity components. With this understanding, the principle excludes the possibility of two particles having the same position in space and moving with the same velocity, and it therefore amounts to a limitation even more trivial than the impenetrability axiom. But the meaning of *state* is not to be taken in the classical sense.

The simple formulation has reference to the Bohr theory of electrons or to that remnant of it which is kept alive in modern physics as a concession to our habit of pictorialization. According to this theory the electron normally moves in orbits around nuclei, and each orbit is characterized by quantum numbers. At first there were three of these: one (n) a measure of the electron's distance from the nucleus, one (l) a measure of its angular momentum, and one (m) a number serving to fix the orientation of an electron orbit in space. Pauli noticed that no more than *two* electrons could have the same set of quantum numbers, n, l, and m.

Thus the electron in an (unexcited!) hydrogen atom has the set (1, 0, 0); the two electrons in helium have the same set; but the third electron in lithium may not take on these values. The "K shell" of electrons is filled, and the supernumerary electron in lithium has to seek another set of quantum numbers called the "L shell." This process goes on throughout the periodic table.

A puzzling feature in this amazing scheme is nature's preference for pairs. Since an assignment of sex to electrons is not a plausible procedure, one begins to suspect a defect in the analysis just presented. For example, if the description of a state in terms of three quantum numbers were incomplete, if a larger set were required, the two members of a pair might differ with respect to a quantum number which was left out of account in this description. Such was indeed the explanation of the pairs: in addition to moving in an orbit each electron was found to revolve about its own axis, to "spin," as physicists put it, and this spinning motion could proceed in one of two ways. An additional quantum number, s, with values -1 and $+1$, had to be introduced, and the exclusion principle thus took a more plausible form: *No two electrons can have the same four quantum numbers* n, l, m, *and* s. The occurrence of pairs with respect to the first three quantum numbers had thus become legitimate.

This simple form of the principle is very fruitful. It leads at once to an understanding of the shell structure of atoms, the main facts of chemical valence, spectroscopy, and magnetism. But its logical weakness cannot be concealed.

There is something unsatisfying about a factual pronouncement of a descriptive sort at so deep a methodological level. One wants to know why electrons behave that way. The principle in this form alludes to a peculiar and almost irrational propensity of nature, much like her former abhorrence of a vacuum. Further analytic penetration of it is therefore desirable, and it has been performed chiefly by Heisenberg.

At this stage of understanding embarrassing questions can be asked, and because they often baffle students of physics, we shall briefly raise and answer them. Why is it that only electrons *in the same atom* are affected by the exclusion principle? Two helium atoms contain four electrons, two of which have identical sets of quantum numbers n, l, m, and s. Is the obedience to the dictates of exclusion conferred upon the electrons by some influence from the nearest nucleus? If so, how does an electron know which nucleus it is to obey when atoms are brought very close together?

The exclusion principle does not limit its validity to the electrons of a single atom; it treats all electrons in all atoms equally. The misunderstanding voiced in the preceding questions arises from excessive emphasis on the usual set of quantum numbers which we have just explained. They are correct only for an electron that moves about a nucleus. If an electron is in the force field of two helium nuclei, a new set of quantum numbers characteristic of the two-center problem is required, and with respect to that set all four electrons in the two helium nuclei *do* have different quantum numbers. We see therefore that the principle is not discriminatory as to individual atoms and does not result from a proprietary relation between a force center and the electrons which move about it.

Curiously enough, exclusion rules the behavior of all known elementary particles except photons. No one quite understands this strange exception. Composite systems like deuterons, alpha particles, atoms, and molecules may or may not obey the principle, but the exact behavior can always be deduced from their composition (see Sec. 20.4).

20.3. Mathematical Formulation

In quantum mechanics, the state of an electron is represented by a function, $\varphi(x, y, z)$ (see Chap. 17), the square of which is the probability that the electron be found at the point x, y, z of ordinary space. When dealing with two electrons we must extend this method of description, and the extension is fairly obvious. In place of the three coordinates of a single electron, which shall now be called x_1, y_1, z_1, six are to be used: those referring to electron 1 and those referring to 2. The latter may be represented by x_2, y_2, z_2. Hence the state of two particles is a function of coordinates in the six-dimensional "configuration" space familiar from the classical dynamics of two-particle systems: $\varphi = \varphi(x_1, y_1, z_1, x_2, y_2, z_2)$.

As before, $\varphi^2(x_1, y_1, z_1, x_2, y_2, z_2)$ represents the probability that electron 1 shall be found at $x_1y_1z_1$, electron 2 at $x_2y_2z_2$.

Let us denote by P_1 the point $x_1y_1z_1$, by P_2 the point $x_2y_2z_2$. One may then write in place of $\varphi(x_1, y_1, z_1, x_2, y_2, z_2)$ simply $\varphi(P_1, P_2)$ or, as a further abbreviation, $\varphi(1, 2)$. Now it is important to notice that $\varphi(1, 2)$ is not in general equal to $\varphi(2, 1)$. An example will make this clear. In the simplest case, where both particles are confined to move in a line, only x_1 and x_2 are required as coordinates, and a possible state function would be $\varphi(1, 2) = \sin x_1 \cos x_2$. But $\varphi(2, 1)$ then equals $\sin x_2 \cos x_1$. This is not equal to $\sin x_1 \cos x_2$, as is seen from the circumstance that for the points $x_1 = \pi/2, x_2 = 0, \varphi(1, 2) = 1$, whereas $\varphi(2, 1) = 0$.

The whole class of functions $\varphi(1, 2)$ can clearly be divided into three subclasses, defined by the following criteria:

$$
\begin{aligned}
&S\colon\ \varphi(2, 1) = \varphi(1, 2)\\
&A\colon\ \varphi(2, 1) = -\varphi(1, 2)\\
&M\colon\ \varphi(2, 1) \text{ is neither } \varphi(1, 2) \text{ nor } -\varphi(1, 2)
\end{aligned}
$$

Functions satisfying criterion S are called symmetric functions; those satisfying A are called antisymmetric; the remainder are said to be mixed. The last subclass contains by far the greatest number of specimens. Examples of the three classes are:

$$
\begin{array}{ll}
x_1 + x_2 & (S)\\
x_1 - x_2 & (A)\\
2x_1 + 3x_2 & (M)
\end{array}
$$

Pauli's exclusion principle in its mathematical formulation requires that *all functions representing states of two electrons must be of the antisymmetric variety.*

We shall now prove that this rather abstract statement is indeed equivalent to the simpler form. Assume particle 1, if present alone, to be represented by the state function $u(x_1, y_1, z_1)$, or briefly $u(1)$. Its probability of being at P_1 will then be $u^2(1)$. Similarly, let particle 2 when present alone be in state $v(2)$ with probability $v^2(2)$ of being found at P_2. According to one of the familiar rules of the probability calculus the probability of two independent events is the *product* of the probabilities of the individual events. When applying this rule to our situation we find that the probability of finding particle 1 at P_1 and simultaneously particle 2 at P_2 is $u^2(1) \cdot v^2(2)$, and this forces us to choose as the state function of the system of two particles

$$\varphi(1, 2) = u(1) \cdot v(2) \tag{20.1}$$

a simple product of the individual states. We have here discovered a rather general result, also demonstrable in other ways: The state function of a number of independent, or noninteracting, particles is the product of the states of the individual particles.

But the function $\varphi(1, 2) = u(1) \cdot v(2)$ does not in general satisfy the exclusion principle! To make it do so one must subject it to a mathematical process, called "antisymmetrization" by the wanton despoilers of our language. The process is to subtract from $u(1) \cdot v(2)$ the same product but with its arguments interchanged or, put briefly, with an "exchange of particles"; to wit, the antisymmetrized form of $u(1) \cdot v(2)$ is

$$\varphi(1, 2) = u(1) \cdot v(2) - u(2) \cdot v(1) \tag{20.2}$$

This function clearly belongs to class *A*. It can also be shown that there is no other way in which $u(1) \cdot v(2)$ can be made antisymmetric. The exclusion principle thus demands for pairs of independent electrons a function of the form (20.2), not of the form (20.1) suggested by probability reasoning alone. The function (20.2), however, vanishes if u and v are equal;[1] in other words, the probability of every composite state built from equal individual states is zero. This is precisely what the simple form of the principle asserts.

If the number of particles is greater than two, the construction of antisymmetric states becomes a little more difficult but the theory is otherwise unchanged. Again it happens that the antisymmetric function vanishes when *any* two of the individual states from which it is compounded are the same. But the general case yields nothing new, and we dismiss it from further consideration.

[1] In terms of the older theory equality of u and v means equality of quantum numbers.

20.4. Consequences of the Exclusion Principle; Quasi Forces

Exclusion of identical individual states is only one of the consequences of the principle; other effects, some of far-reaching importance and almost unforeseen, are created by the formal choice of antisymmetric functions. Foremost among the surprises is a new kind of force of nondynamic origin, a quasi force that can be traced directly to the mathematical properties of functions having the form (20.2).

We have seen why, on the basis of simple probability reasoning, a function given by Eq. (20.1) represents a state in which the two particles do not interact. This is a perfectly general result: the state of several noninteracting physical systems is always a single product of the states of individual systems. Conversely, if a state is not of the product form, it must represent interacting individuals, or, in more technical language, it implies a correlation [1] between the systems. Now, as we have seen, the exclusion principle demands a function of the form of Eq. (20.2), which is the difference of *two* products and therefore implies correlations.

A correlation between particles, however, when interpreted in terms of orthodox mechanics, can mean nothing but the existence of forces between them. The antisymmetric functions required by the exclusion principle therefore suggest the action of forces. And yet these forces are not of dynamical origin, as the following example will show.

A single particle, not subject to forces and moving along the x-axis with velocity proportional to V_1, has a state function $u = e^{iV_1x}$ (cf. Eq. 17.8). A second particle of the same kind would, if present alone, have its state represented by e^{iV_2x} provided that its velocity is proportional to V_2. When both are present, $\varphi(1,2)$ is

[1] The meaning is this: Choice of $u(1)\cdot v(2)$ as state function corresponds to a probability which is also of the product form: $w = p(1)\cdot q(2)$. Consider now the following two probabilities: (1) that particle 1 shall be found at a; (2) that particle 1 shall be found at b. If the *ratio* of these two probabilities, R, is independent of the position of particle 2, the particles are said to be *un*correlated. In our case, $R = p(a)q(2)/p(b)q(2) = p(a)/p(b)$, the function $q(2)$ cancels out. But the dependence on the position of particle 2 would remain if the function w were not of the product form.

not the simple product $e^{iV_1x_1} \cdot e^{iV_2x_2}$ but the antisymmetric function

$$\varphi(1, 2) = e^{i(V_1x_1+V_2x_2)} - e^{i(V_1x_2+V_2x_1)}$$

The probability is formed by taking the square of the absolute value of φ:

$$w(1, 2) = \text{const}\,[1 - \cos(V_1 - V_2)(x_1 - x_2)] \qquad (20.3)$$

We have here derived a most amazing result, namely, that it is impossible for the two particles to come together and likewise that they cannot possess the same velocity! For if $x_1 = x_2$, $w(1, 2)$ becomes zero even if the velocities are different, and the same happens when $V_1 = V_2$ even when the particles have different positions. They can neither be at the same place nor move with the same velocity since the probability for these conditions vanishes. The particles, though initially assumed free, are seen to avoid each other, to be as different as possible with regard to both position and velocity. Or, to put the conclusion in more proper but equivalent language, the chance of finding[1] the particles at different places and with differing velocities is far greater than the chance of finding them in similar conditions of motion. The physical effect is one of repulsion, reminiscent of the impenetrability principle of old.

But strangely, the tendency to avoid each other depends on the relative velocity of the two particles. When Eq. (20.3) is plotted as a function of the difference $x_1 - x_2$ for a given constant value of $V_1 - V_2$, the point $x_1 - x_2 = 0$ is seen to be a minimum, the curve rising on both sides of it. The greater the value of $V_1 - V_2$, the steeper the rise on either side and hence the smaller the range in which the particles shun each other. Conversely, if $V_1 = V_2$, the range of spatial exclusion is infinite. In a crude manner of speaking, each particle wants to be alone; each runs away when it "smells" the other, and its sense of smell is keener the more nearly its velocity equals the other's. Somehow, this analogy with sentient behavior seems more appropriate for the description of the correlations here encountered than an appeal to ordinary forces, the chief reason being the very unusual character of the forces, and especially the velocity dependence,

[1] Note here the footnote on page 346.

which would be required for the purpose. Nevertheless the question must be raised: Can the exclusion principle be accounted for, or replaced by, the assumption of special forces, perhaps unconventional forces?

It is to be noted first that the use of antisymmetric functions does not preclude the presence of forces in the ordinary sense. If the particles considered in the example above were electrons which repel each other dynamically by virtue of their charges, this effect can be incorporated into the state functions. The presence of ordinary forces would then show itself over and above the correlational avoidance just examined. But a discussion of these matters would take us far afield. The particles we dealt with were in fact assumed dynamically free, as the functions e^{iVx} chosen to represent their states assure us.

Further reflection upon the question of equivalence between our principle and the action of forces brings to light another curious situation, explained perhaps most readily in connection with a physical example. The calculations underlying the account to be given are simple but will not be included here. Consider two particles at the ends of a steel spring, and think of the spring as vibrating. The quantum energies of this system, when computed in the ordinary way and without imposing the exclusion principle, are $(n + \frac{1}{2})\epsilon$, where ϵ is a constant and n is any integer or zero. Exclusion, however, forces n to be an *odd* integer. The state of lowest energy, which would be $\frac{1}{2}\epsilon$ if the principle were not valid, is raised to $\frac{3}{2}\epsilon$. So far there is nothing to prevent us from saying that this is a true dynamic effect, resulting from an error in our appraisal of the forces supplied by the spring. But the price we pay is this: We must assume a different fictitious spring for every value of n; the spring changes its physical properties from one energy state to another. The physicist has decided that this is too high a price, and the philosopher will no doubt agree with him. Again, metaphysical decorum decides an issue, and the decision here is in favor of correlations that are *not* reducible to forces of the old, accustomed kind.

In physics, they are nevertheless called forces, though their nature is by no means misunderstood. Everyone familiar with the quantum theory knows that *exchange forces* (this is the name of the correlation effects) are not to be confused with ordinary forces and are very difficult to explain to the uninitiated. They are of extraordinary importance and of universal occurrence, providing explanations for all phenomena which arise from the cohabitation of physical particles. Chemical binding, the cohesion of solids, the properties of crystals, magnetism, and many other so-called "cooperative" effects cannot be understood without them.

There is an interesting and far-reaching parallelism between the postulate of general relativity and the exclusion principle that extends even to their consequences. General relativity creates physically perceptible forces out of the metric of space; by endowing its equations with the formal property of invariance it is able to account for gravitation, no reference being made to the ordinary concept of force. The exclusion principle imposes another formal property, antisymmetry, upon the state functions of quantum physics and thereby produces correlations which are tantamount to forces.

In connection with relativity the remarkable emergence of nondynamic forces has been widely recognized, and it has been said that the relegation of forces to geometry is the greatest achievement of relativity theory. No such discernment has accompanied the discovery of the exclusion principle, which has in fact been even more successful than relativity in extracting physical forces from mathematical symmetries. Here is a range of problems that abound with stimuli for philosophic reflection.

20.5. Generalizations and Limitations

Our study thus far has been careful to suggest that the requirement of antisymmetry be maintained for the states of electrons only. Restriction to only one kind of particle is in serious contrast with the demands of Chap. 5, and it might be hoped that its applicability be more general. Such hope is satisfied, but not in unlimited measure; for it turns out that the exclusion principle holds for most but not for all elementary particles. The best-known exception is the photon.

For the moment we leave aside this exception and consider the particles (electrons, positrons, neutrinos, protons, neutrons, mesons) that are subject to the exclusion principle, noting the important qualification that the principle applies only to particles of the *same kind*. Thus, the states of two protons are antisymmetric but the states of a system of one proton and one neutron are not. The evidence for this pronouncement is complete and unquestionable, for the system composed of a proton and a neutron forms one of the simplest nuclei (the deuteron) and has been subjected to exhaustive study. Similar knowledge comes from the field of molecular spectroscopy, where homonuclear molecules (for example, N_2) are found to obey symmetry rules, while heteronuclear molecules (for example, HCl) do not.

It is fortunate, indeed necessary for consistency, that the principle should lose its grip upon dissimilar particles. For as will be seen in the next section, antisymmetry confers something akin to indistinguishability upon the particles whose states it couples, and *different* particles are of course distinguishable. But there is another consequence to be drawn from the foregoing premises, a consequence which limits the principle in another way.

Imagine that particle 1, the coordinates of which enter in Eq. (20.2), is in fact not a single electron but is composed of two electrons or, in general, of two particles that are subject to Pauli exclusion. And let us suppose that the particle labeled 2 in Eq. (20.2) has the same hidden composition. It is then easy to see that the antisymmetric function, Eq. (20.2), is not a correct state for the system of four particles. For if the members of pair 1 are interchanged, the function should change its sign but does not—as indeed it could not, because the members of the pair are not specified separately. On the other hand, if pair 1 is interchanged with pair 2, the function (20.2) changes sign. But in this case it should not do so because an interchange of pairs is equivalent to a double interchange of individual members, hence the correct antisymmetric function must change its sign twice and therefore restore itself. The conclusion to be drawn is this: If the particle of which the coordinates appear in the state function and with respect to which antisymmetry or symmetry is determined is itself composed of two elementary particles satisfying the exclusion principle, the function must be *symmetric*. By a simple generalization, the same remark applies to particles composed of any even number of elementary ones. In this way, the principle of antisymmetry automatically changes over into one of symmetry under clearly defined conditions. Nuclear and atomic physics furnish an abundance of evidence for the alternation of symmetries, which manifests itself most strikingly in the statistical behavior of different substances. The superfluidity of helium at low temperatures is believed to be an example of it.

The photon is known empirically to exist only in symmetric states. Hence the thought occurs at once that perhaps it has a hidden composition. Conjectures ascribing to it two "subphotons" or two neutrinos have been freely made, but all have failed by running into contradictions with established theory or empirical fact. At present the exceptional role played by photons is not entirely understood.

But an interesting parallel between the anomalous symmetry of photon states and another anomalous behavior can be established, a parallel which is likely to point someday to a more satisfying connection. All particles with antisymmetric states have angular momenta of magnitude $(n + \frac{1}{2})h/2\pi$, n being zero or an integer; the photon alone (and possibly some kinds of mesons) has quanta of angular momentum that are integral multiples of $h/2\pi$. There is obviously a deeper meaning concealed in the double malfeasance of the photon; to discover it is one of the challenges of atomic physics.

In Chap. 17 attention was given to the manner in which states change in time. It is Eq. (17.16) that propagates states, converts a function φ_1 at one time into φ_2 at some later time. Now it is all very well to require that state φ_1 shall be antisymmetric; but will it remain antisymmetric as time goes on? The exclusion principle has no jurisdiction over the temporal changes, which are regulated by the Schrödinger equation alone; the principle is therefore void if it is incompatible with that equation. Fortunately it can be shown that the Schrödinger equation can never "mix symmetries" —those states which are originally antisymmetric will have this property forever. Consistency is saved.

The permanence of symmetries limits the causal relation between states in a definite but very minor way, for it prevents the development of states having one kind of symmetry from those having another. The principle may therefore he said to limit the free causal nexus between all imaginable states, or even to channel causality in accord with a specific pattern. But it will be seen in the next section that the excluded states are usually of little interest because they would also be ruled out for other reasons.

Here again, one encounters a striking resemblance to the state of affairs prevailing in relativity theory, where causality is similarly restricted to certain classes of events. Causal connection extends only to events which lie within each other's light cones. Here, too, the limitation upon causality is implied and automatically effected by the laws of motion, not enforced by external regulation. But there is one important difference between the two situations. The exclusion principle completely annuls all states to which causal evolution may never carry a system, while the principle of relativity, acting less drastically, assigns them to some other causal pattern.[1]

20.6. The "Identity" of Electrons

It may be shown that an antisymmetric state function implies the same observable properties for one particle as for any other. For example, the probability of finding electron 1 at the point P_1 while electron 2 is at P_2 is found by squaring $\varphi(1, 2)$ of Eq. (20.2); it is

$$u^2(1)v^2(2) - 2u(1)v(1)u(2)v(2) + v^2(1)u^2(2)$$

Now the probability that electron 1 occupy P_1 regardless of the position of the other electron is obtained by integrating this expression over the space of particle 2. When this is done and use is made of certain simple mathematical properties of states, the result is $u^2(1) + v^2(1)$. But a similar integration extended over the space of the first particle, and therefore yielding the probability that the second particle be located at P_2, leads to $u^2(2) + v^2(2)$, the *same function* with arguments corresponding to the point P_2. Hence the observable distribution in space of the two

[1] For details, see H. Weyl, "Raum, Zeit, Materie," Springer-Verlag, Berlin, 1921; also, H. Weyl, "The Open World," Yale University Press, New Haven, 1932.

particles is identical, and a position measurement is incapable of distinguishing between them. What has here been shown for positions holds for all other observables as well; energy and momentum measurements are equally noncommittal as to the identity of the particles and do not permit us to "tell them apart." Furthermore, the same indiscriminateness prevails in general for all constituents of a system containing more than two particles of the same kind.

The physicist often speaks of this circumstance as the *identity of electrons*. Perhaps this phrase is unfortunate, for it invites philosophic speculations which the scientific facts do not warrant. The electrons described by an antisymmetric function cannot be distinguished by their observable properties, it is true; and if this statement were taken in the classical sense, with all observables interpreted as uniquely possessed attributes, the electrons would indeed be *identical*—they would be one and the same entity. For if two particles, in the ordinary meaning of that term, are always found at the same place, with the same velocity, the same energy, and so forth, they must be reckoned as one.

In the quantum theory, however, the observables of electrons are latent attributes, and the naïve conclusion just sketched requires modification. A single measurement can no longer reveal the electron's physical condition in a significant way; but an aggregate of observations, suitably conducted, may yield information that is crucial on this point. Thus, when many position measurements are performed, it is quite easy to ascertain, by summing over their distribution, whether two electrons are involved or only one. In this way *number* becomes an observable despite the indistinguishability of the numbered entities.

One is tempted to recall in this connection Leibnitz' famous principle of the identity of indiscernibles: "Non dari in natura duas res singulares solo numero differentes." The plausibility of this assertion comes from ordinary, large-scale experience, from the understanding implicit in classical physics, and it is limited to that domain. As we have seen, quantum mechanics presents clear examples of its violation in the use of the exclusion principle; it must therefore be recorded that Leibnitz' conjecture has lost its validity in modern physics.

20.7. Exclusion and Biological Organization

Even before Pauli's discovery, physics had ceased to be mechanistic, for it had moved beyond the obvious, perceptible, and intuitable elements of description suggested by the word *mechanism*. When more clearly analyzed, the essence of mechanistic reasoning is seen to cluster around two beliefs: first that entities (in our terminology these are the constructs called systems) are divisible into parts, and second that these parts are localizable in space and time. Mechanistic reasoning therefore looks with special confidence to those two sciences which have traditionally implemented these beliefs: geometry and dynamics.

The gradual degeneration of the philosophy of mechanism is perhaps best observed in the transformations which these two disciplines have undergone. Space is not available here to trace all their recent changes, but since many points of interest in this connection have already been argued, a brief summarizing reminder may not be amiss.

The principal thing that has happened to geometry in physics, in addition to a clearer understanding of its constructional character, is a proliferation of dimensions. Ordinary three-dimensional space has transformed itself into the multidimensional phase space of statistics and into the configuration space of quantum mechanics. The last is of particular importance because it bears the hallmarks of being essential and unavoidable in the ultimate description of nature, while the former can perhaps be regarded as a convenient artifact.

What has occurred in dynamics forms the subject matter of Chaps. 16 to 19. The emergence of latent observables has rendered strict localizability of elementary particles impossible. It was this state of ineffectuality in which mechanism found itself when the exclusion principle began to illuminate the scientific scene.

Prior to that time, all theories had affected the *individual* nature of the so-called "parts"; the new principle regulated their *social* behavior. With respect to a single particle it has nothing to say. And what it says for aggregates, though most important, cannot be expressed in terms of *dynamic* regulation. It is as though here, for the first time, physics had discovered within its own precincts a purely social law, a law that is simple in its basic formulation and yet immense in its collective effects. Mechanistic reasoning, already far behind, has gone out of sight as a result of this latest advance.

The advance has also led physics to higher ground from which new and unexpected approaches to foreign territory can be seen. Not too far ahead lies the field of biology with its problems of organization and function, and one is almost tempted to say that modern physics may hold the key to their solution. For it possesses in the Pauli principle a way of understanding why entities show in their togetherness laws of behavior different from the laws which govern them in isolation.

A few specific examples may serve to strengthen this thesis. In the process of building up complex atoms from their constituents, the nucleus and the extranuclear electrons, exclusion plays a decisive role. We need the principle here only in its qualitative form, and we apply it to the electrons which are added, one by one, to the positive nucleus as we pass in review all the elements of the periodic table. The nuclear charge is assumed to increase by one every time an electron is added. The first electron, forming a hydrogen atom, can move in any way permitted by the quantum laws of motion. A second electron, if present alone, would settle into the same energetically most favorable state as the first; but *the presence of the first electron forces the second into a different state of motion.* It is as though the second electron "knew" the first was there even though it does not interact with it by forces of the familiar kind. The helium atom thus owes its characteristic properties to a nondynamic interplay, a sort of tactful avoidance between its parts. And this avoidance has highly important and surprising effects; because of it helium is chemically inactive; the diamagnetism shown by this element is directly traceable to the "tact" of the second electron. And so the process goes on through 94 or more stages, at each of which the Pauli principle, supervening the normal tendencies of the electrons, decrees what the character of the element shall be.

As nuclear research progresses, it is becoming more and more apparent that the same formal rules which have been recognized in the external structure of atoms also govern the composition of nuclei. Only the constituents, and hence the forces between the constituents, are different in the two cases; the possible species of nuclei are formally predetermined in the same way as the valences of atoms.

The evolutionary series stops with uranium or, if the new synthetic elements are included, with number 96. It stops not because the constructional plan is altered, but because the scheme begins to require the dynamically impossible. Dynamics asserts itself and sets limits to the organizing pattern: the nucleus of an element of atomic number 100 is unstable because it would possess too many repelling protons, and this reason is not unlike the stability considerations which are invoked for the termination of the mammalian evolutionary series with the elephant as regards weight and bulk.

Emergence of new properties on composition is a rather general phenomenon in modern physics and owes its occurrence to the exclusion principle. When two hydrogen atoms approach, they may either attract or repel each other. This effect depends on the state of the individual partners and therefore presents no aspects which cannot be brought within the scope of ordinary dynamics. Suppose that the forces are attractive and a molecule has been formed. If now a third atom is brought near, it is *always* repelled; the alternative of attraction has been eliminated. The forces have ceased to be additive because of the verdicts of the exclusion principle. A new phenomenon called "saturation" of forces appears, and this cannot be interpreted by an appeal to the normal two-body interactions in the classical sense. Saturation, however, may be viewed as incipient organization.

It is unfortunate that the interesting vista opened by these remarks cannot be pursued much further with precision at the present time. For in the detailed application of the exclusion principle to the problem of many particles, mathematical difficulties arise, difficulties of an annoying sort which can be removed only by most tedious and time-consuming calculations and which furthermore promise no changed view of the landscape on removal. Physicists and chemists are inclined, therefore, to go on from here by ingenious guesses and to confine their efforts to obtaining results of immediate practical value. The feeling is, however, that the situation is in principle well understood, and if this understanding is at all correct, we have started on a promising approach to biology's most baffling question. Books like Schrödinger's "What Is Life?" [1] are encouraging indications of its future fruitfulness.

Before concluding this discussion, let us take a brief appraising glance at one of the fields of physical research in which the mathematical difficulties of the many-body problem have been partly overcome: the structure of crystals. Here the number of similar constituent atoms (or ions or molecules) is so great that the laws of large numbers aid in the solution of the problem. In the process of constructing a crystal from its atomic parts, new properties are seen to emerge, and these properties have no meaning with reference to the individual parts: among others, ferromagnetism, optical anisotropy, electrical conductivity appear, all "cooperative phenomena" (the term is actually used in the theory of crystals) which owe their origin directly or indirectly to the exclusion principle.

We have not yet reached the stage of biological function, self-maintenance, self-repair, homeostasis, and so forth. While we can understand how the whole can be greater than the sum of its parts, there is no definite indication that the exclusion principle can be operative in *dividing* wholes into parts with separate functions. There are, to be sure, phenomena of secondary decomposition: crystals form domains of peculiar homogeneity, each domain containing a great number of atoms, and many domains making up the crystal. But in going beyond this modest indication one would lose himself in unwarranted speculation.

Change in physical description from its classical character to freer and more abstract form has brought with it the possibility and the need for purely formal principles like the exclusion principle, the immense fertility of which has here been demonstrated. Who is to say that others of similar fruitfulness will not be discovered, in the face of which the phenomena of the organic world may yield to scientific explanation? The vitalist who might resent such encroachment is always at liberty to say that physics has become vitalistic. He may even assert with considerable justifica-

[1] E. Schrödinger, "What Is Life?" The Macmillan Company, New York, 1945.

tion that the exclusion principle is one source of the vitalizing breath.

Summary

In its simpler form, the exclusion principle (Pauli principle) requires that no two particles of the same kind—such as electrons, protons, neutrons—can be in the same state at any one time. The older quantum theories interpreted this as saying that no two particles can have the same full set of quantum numbers. The more accurate mathematical form of the principle, which is discussed in Sec. 20.3, is shown to be equivalent to this statement under proper conditions.

There is much similarity between the present topic and the old assertion that two bodies cannot occupy the same place at the same time. On more careful investigation it turns out, however, that bodies avoid each other to the extent to which their velocities are alike. Perhaps the most spectacular application of the exclusion principle is to the "building-up" process of the elements. This is sketched in Sec. 20.7, where it is also shown that different atoms owe their characteristic features to a kind of social behavior of the electrons which may be summed up by saying: One electron knows what the others are doing and acts accordingly. And this knowledge is not conveyed by forces, or dynamic interactions, of the ordinary kind.

The exclusion principle introduces a correlation into the behavior of particles which, though its effects are similar to the effects of forces, has no explanation in dynamic terms. The resemblance with the principle of relativity is strong: relativity succeeds in accounting for certain forces (gravitational forces) by reference to the formal requirement of invariance; exclusion succeeds in accounting for other forces (*e.g.*, chemical valence) by reference to the formal requirement of antisymmetry. Both are requirements imposed upon the manner in which we formulate our experience.

Leibnitz' principle of the "identity of indiscernibles" is considered from the point of view of modern physics and is shown to fail inasmuch as two electrons, which differ in no observable respects, remain nevertheless two entities.

Finally, Sec. 20.7 surveys the prospects offered by the exclusion principle—and possibly other principles of the same formal kind—for understanding the problems of living matter. These prospects seem very promising.

Selective Readings

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CHAPTER 21

The Contours of Reality

21.1. A Résumé

This book began with an analysis of experience, an analysis admittedly incomplete and looking with unconcealed scientific bias to phases of experience that contribute most to physical reality. Mindful of the easy fallacy which equates what is physically real with what matters, we conceded at the outset that much, indeed most, of our active human experience with its multitonal emotive qualities, with its stretches of monotony and its peaks of sudden elation, its sense of duty, freedom, and responsibility, is at present neutral to the established concerns of reality. But no prediction was made that they will remain forever neutral to the principles by which *physical* existence sustains itself.

The central distinction in our analysis of experience was between its immediate and its rational elements, between data and constructs. Again, it was not claimed that these formed two clearly separable, mutually exclusive sets of entities with a dividing boundary definite enough to satisfy the desires of class-minded logicians. But the distinction is good enough for scientific purposes; the important results of our quest for reality are independent of the precise location of the boundary between sense and reason; the distinction in question is recognized and sanctioned in all respectable treatments of philosophic problems; and finally there is the undeniable evidence of our own actual interpretation of experience: normal behavior is conditioned by the acknowledgment of a difference between thought and perception.

Data have an ephemeralness, a rhapsodic spontaneity, a nakedness so utterly at variance with the orderly instincts that pervade our being and with the given unity of our own experience as to be unfit for use in the building of reality. The constructs, on the other hand, are foot-loose, subjective, and altogether too fertile with logical implications to serve in their indiscriminate totality as material for the real world. They do, however, contain the solid logical substance which a stable reality must contain. To join these incompatibles and to erect a structure firm enough to stand, indeed to stand in such alignment with the fleeting ingredients of all that is immediately given as to be its significant representation—to achieve this goal a selective process is applied to constructs. This process is threefold.

The tripartite division here suggested is not a serial arrangement of phases through which science (or common sense) must pass successively in order to confer reality upon its findings. In action, the phases are interlinked and may occur in random order; they are ingrained in the methodology of science, are often obscured from view and in need of discovery. But an understanding of the meaning of science and the meaning of reality can no longer be attained without them.

The first part of the selective process—first in logical and not in temporal priority—is the imposition of metaphysical requirements upon constructs, resulting in a sifting of the rational material similar to the refinement of crude ore into useful metal. Metaphysics here enters the scene in the form of epistemological postulates of relatively enduring structure, not as eternally preformed ontological certainties and not as supernatural directives. When scrutinized with moderate care these postulates, or principles, present the spectrum we surveyed in Chap. 5, with its list of six loosely named requirements. Fulfillment of these requirements renders a construct acceptable for cognitive purposes but does not ensure its validity.

In addition to this logical component in the evolution of knowledge, *i.e.*, the component by which rationalism claims hold on the interpretation of experience, we recognize other equally important measures wherein empiricism asserts itself as a final arbiter of validity. These form the second and the third phase of the process that leads in the end to reality. The second was symbolized in Chap. 6 by the circuit of verification. Although it cannot operate independently of the third, which is the establishment of rules of correspondence between data and constructs, its status within epistemology is unique and invites a separate discussion.

Unification has its start in what was called the *P* plane of experience or Nature (cf. Fig. 6.1), the realm of the evanescent surges of sensory perception. This limited conception of Nature is the standard point of departure of most present forms of science, but it is not thus restricted by the character of experience and may well include the whole of what is immediately given.[1] At any rate, verification proceeds from the *P* plane with the aid of correlations to the field of acceptable constructs, where it is self-propelled by the inherent logical nexus of theories until it reaches a place or places whence a second application of correspondences allows the verifying linkage to emerge a second time in Nature. And here the decision falls. Verification has succeeded if prediction scores a hit upon the *P* plane; otherwise it has failed. In the case of failure, what may be concluded is simply that something was wrong, and no indication is ordinarily available as to the source of trouble. This may lie in an erroneous connection between constructs, in some hidden inconsistency which rational scrutiny might expose, or in an erroneous correspondence. But when the circuit is successfully completed, the set of constructs involved in the circuit is said to be valid with respect to the terminal observations. Constructs verified or validated in this way by a sufficient number of data are verifacts.

The sense of the weasel word *sufficient* becomes fully clear only through an elaborate analysis of scientific procedure that places in evidence the *deductive* character of scientific reasoning. Such an analysis was attempted throughout this book; it defies a brief summary. But the more specific considerations which make possible an agreement between pointlike predictions and necessarily indeterminate data, considerations which soften the meaning of exact agreement required for verification, are condensed in Chap. 6.

Bridges between Nature and constructs are formed by relations here called rules of correspondence, the third theme of our present discussion. These are identical with Northrop's epistemic correla-

[1] That is, it might include sudden pain, spontaneous feelings of elation, unreasoning desires, and so forth.

tions. If we were to state the effect of modern physics upon the philosophy of science in a sentence, we should have to say that modern physics has made apparent the function of this third phase of epistemology. And it has done so by contributing new and less obvious rules of correspondence, rules that could no longer hide under the cloaks of vaguely conceived identities. The reifying passage from a certain set of sense impressions to the construct "stone" would hardly call for the introduction of anything so pretentious as a rule of correspondence, and analysis can fairly afford to ignore them. Nor is there much need for recognizing their rather trivial function in any of the correlations effected through intuitable models so customary in classical physics. But when a connection is to be established between the position of the hands of a clock and the time of relativity, or between the multiple and discordant observations of an electron's momentum and its quantum state, a new kind of bridge appears, a bridge neither logical nor empirical, and nothing less than a distinct methodological element in its own right. It is as though the concept of identity between the perceived and the constructed, tenable to a degree in classical science, had opened up to form a sort of action-at-a-distance identity which is the substance of our rules of correspondence.

As was demonstrated, these rules are often operational definitions; they always draw their sanction from success in manifold applications. Without them constructs are empty. In the course of scientific progress they are known to change, known to change more rapidly than the metaphysical principles; but their life is longer on the whole than that of constructs, which change with every major discovery.

Among the limited set of constructs that can claim the rank of verifacts at any time one can discern *systems*, *observables*, and *states*, but this is not an exhaustive classification. The property of physical reality attaches primarily to these, and we suggested that it be limited to them. Such things as instantaneous values of an observable, as for example the assignment of the quality red or the wavelength 6,800 angstrom units to an object emitting light at the moment, fall into a twilight zone of reality where a clear verdict is not demanded and where personal preference may well decide, in the present stage of science, upon the momentary reality of the color. But questions of this sort are never meaningless; to brand them with that label is to deny science forever a reasonable decision in these matters. Primarily, then, we regard as the content of the real world the verifacts called systems, observables, and states.

Then we noted how this collection of reals falls curiously short of public approbation because it excludes the palpable facts of Nature. Having concluded, however, on reliable grounds that data and constructs cannot both define a unique physical reality (for if they are indiscriminately joined together, we get all *experience* back again), we decided to scrutinize what language means by the "reality" of perceptions, data, and the like. And we discovered that it means authenticity, historicity, means their having actually occurred as warranted by memory. This led us to recognize *historical* reality as a complement to *physical* reality, at least to the extent to which historicity is maintained in memory. But when the memory of impressions becomes dim, when the impressions antedate memory, and when the memory of an occurrence is not our own, the perceptual event becomes a construct, in need of being verified. And so it comes about that historical reality, as it moves back into the past of our own experience, merges with physical reality and eliminates the need for a distinction.

21.2. Reality in Quantum Physics

In quantum mechanics the problem of reality seems to center in the meaning of states. Here it became clear for the first time, and not without a shock to the physicist, that even a physical state, previously regarded as a nice and tidy collection of possessed observables geared together and running like wheels in a clock, is in fact a composite of latent observables loosely coupled to Nature by rules of correspondence of a statistical sort. If we hold fast to the thesis that states have physical reality, then we are definitely admitting the irrelevance of a single datum for physical reality. On the other hand, a single datum *is* relevant for historical reality whatever view is accepted about the meaning of states.

One may defy the quantum theory and continue to regard individual events and observations as components of physical reality, thus identifying physical and historical reality. In doing so, however, one runs straight into two of the most serious difficulties that have ever beset a science. The first is apparent from Chap. 6, where the indeterminate background of all immediate perception was examined: even in the *presence* of external convergence of data (the type of convergence which fails in quantum mechanics) the problem of internal convergence raises issues sufficiently grave to render doubtful the significance of a single datum for reality. The second difficulty is this: Since only states (φ functions) are subject to the laws of nature—and we mean here what was formerly called the "laws of motion," the most fundamental formalism by means of which reality propagates itself in time—and since single data are not subject to them, the individual components of historical reality can never be made to satisfy the metaphysical requirements characteristic of scientific procedure. Nor can they be verified. Hence there is no alternative; the φ states of modern physics must be accepted as a valid part of physical reality.

The systems themselves have undergone only minor changes. These changes include, to be sure, renunciation of many intuitable properties, and this will seem like a radical departure to many minds. But strictures on models have long been developing, and the ban pronounced by quantum physics is hardly more than the culmination of a long-time trend. On the whole, systems have retained the same observables they always had, but the observables are latent. Elementary particles still carry the attributes of mass, position, momentum, and so forth, although to say that they "have" position in the older sense is ambiguous.

To return once more to states: It may well be that some readers wonder what the shouting is all about. The "crisis" in physics has perhaps occupied our attention in a measure destructive of larger perspective. We should therefore note that the idea of state now established in quantum mechanics is not new to science; its precursor has long been accepted in biology. Since the days of Gregor Mendel (1866) or, rather, since the belated discovery of his work in 1900, the quantum theory of states has been successfully used in genetics. To take a well-known example we consider the flower four-o'clock (*Mirabilis jalapa*), which has two pure strains, a red and a white variety which will here be designated by r and w. When a red flower is crossed with a white one, all members of the first filial generation are pink hybrids; they will be labeled p.

Now assume that inbreeding occurs between these hybrids. Every specimen of the second filial generation certainly has one of the three observable properties r, w, or p and will disclose it when it blooms. Before it develops a flower, it is in a perfectly definite, and we would say in a perfectly *real*, state, which however can be described only by noting that the probability of the specimen's being r is 1/4, its being w is 1/4, and its being p is 1/2. "Measurement" must in this case wait until the flower blooms. Nor will a single "measurement" carried out upon a random specimen define a *state*, provided that we mean by state its generation with respect to the crossing of given pure strains (*e.g.*, belonging to the second filial generation just considered), for if the flower does turn out to be p, for example, one may not conclude that it belongs to the second filial generation; succeeding generations also have members of this type though with differing probability distributions. Note also that the first filial generation is an eigenstate with respect to the observable "color" and that its eigenvalue is p.

If belonging to some specific generation (with respect to the crossing of two pure varieties) is a real attribute of *Mirabilis jalapa*, then it is very difficult to see why the reality of quantum mechanical states should raise any serious question.

21.3. Why Not Simple Realism?

Our unreflective lives proclaim the truth of realism; in their normal pursuits all men grant tacitly the prior existence of an external world of which they, themselves, form unique parts. This, too, is a fact of experience, and a reasonable theory of knowledge must render account of it. It is rather more than a *fact* of experience, for the practical tenets of realism illuminate and inform the whole of our nonspeculative experience. Unless the theory outlined in this book agrees with that recognition, unless realism is, as it were, a first approximation to it, the theory has no merit.

Now with respect to primitive matters of everyone's experience, as for example the reality of the objects of our daily lives, the constructional view presented gets to precisely the same factual consequences as realism, be it naïve, reformed, or critical. Only it has to balance itself clumsily on stilts, taking many steps to its goal, while realism walks to it with normal gait. This is because in the simple acts of reification that impart significance to primitive cognitions the rules of correspondence are so obvious, the passage performed with their aid is so short as to make these rules uninteresting. And in their absence the process of knowledge becomes a series of "causal" steps in which one element of experience "suggests" another by association or otherwise. Since the territories joined by unrecognized rules are always, or at least appear to be, adjacent, the distinction between immediate data and constructs seems beside the point and may as well be dropped in favor of a kind of interaction of a real world and sentient organisms. Realism, therefore, can well be regarded as a first approximation to the cumbersome formulations we have attempted, an approximation that is good in the region of experience interpretable by "short" rules of correspondence, the region near the *P* plane.

Although the equivalence just established is true in all practical respects, within the limits stated, there remains the vast metaphysical difference often contained in the realistic thesis that the physical world is outside experience, which we deny. The advantage gained by it is an irreducible sort of uniqueness attaching to the experiencing ego, similar to the distinction conferred upon the terrestrial globe by the Ptolemaic hypothesis. In our view, the reflecting (not experiencing) ego is initially a construct to be verified, a construct of remarkable universality, enabling a self-reference of every part of experience. That such self-reference is possible, and hence that the ego construct can be verified, is noteworthy enough, may indeed be the most noteworthy fact of our experience; but it is not thereby exempt from rational and empirical examination.

This view suffers no defeat from the objection that we assume and verify the existence not only of our own but of *other* selves. There is no reason—one might argue—why each of us cannot regard *his* experience as constructed and verified in the manner we advocate, aside of course from its being had in its immediacy; but when there are other selves doing the same constructing and verifying, must there not be another dimension in which these selves exist as countable individua? And if so, why not place ourselves and the objects of our experience within that required new dimension and accept realism without reservations? The answer is: We do not need an additional dimension to accommodate other selves. We establish the self-reference of a large part of our own experience by noting the ever-present possibility of a transition between two closely related types of experience: being aware of something, and being aware of being aware of something. These are joined by a rule of correspondence having a vast range of applicability. And the totality of constructs linked by this rule to Nature, united into one great abstraction, make up our self. There is no essential difference between this result and the verification of such a physical construct as mass. And if there is no paradox in the assignment of mass to remoter kinds of experience than that in which it is first established, if we deem it proper to impute mass to photons and to all other forms of energy, then there is nothing uncalled for in the act of transferring the self-reference that is confirmed in certain parts of our experience to others where it cannot directly be tested, namely, to other persons.

To put the matter differently, one might say that the notion of self arises from the experience of second-order awareness, if this phrase may be used to describe the state of being aware of being aware of something. In that same sense, the self of others is a certification of the possibility of third-order awareness. Clearly, fourth-order awareness is also possible and denotes someone else thinking about a third person's awareness, and so on. All this can be adequately described within my own experience, and it is clear that if we once succumb to the temptation of acknowledging the existence of the *one* additional metaphysical dimension which realism demands, there is no way of avoiding a silly encumbrance with a nestling infinite hierarchy of dimensions, much like Zeno's nest of spaces.[1]

Flights into generalities like the preceding are not considered in good taste by modern scientists, and we hasten to come down to earth with a simpler lesson, perhaps of interest to them. Realism, it is true, represents a streamlined version of the methodology of science, satisfactory as a working hypothesis. Why then should the scientist bother to probe beyond it?

The answer is twofold. First, the simple version is unable to represent correctly the parts of scientific experience made significant by "long" rules of correspondence; it fails to explain the remoter regions of the *C* field. Second, to renounce the careful circumspection which a more elaborate epistemology enforces involves a danger for science and for human progress. The attitude of realism, or idealism, or indeed any attitude that projects a single phase of experience beyond the confines of experience relinquishes control over reality to agencies of doubtful competence. And it leaves science without defense against fairies, ghosts, and goblins. For as we have tried to show, unless it be found in the counterplay of construction and verification, there is no available criterion to give reality its warrant and to set it apart from the unreal.

It must seem strange to the philosophic reader that I have repeatedly singled out realism for direct attack, and some may say justly, that I have been none too specific in selecting my target (since there are many kinds of realism). Worse, I am to be criticized for warming up some of the older arguments. In answer to these accusations I offer only this excuse: There is a need for such treatment. Realism, usually in some inarticulate and nonspecific form that is disturbed by finer distinctions, is the philosophy of the working scientist. His philosophic interest can best be aroused, it seemed, by attacks upon this crucial center of complacency. Thus, while realism is a good school for scientists, it is also a hard school to graduate from, and this book was meant to offer aid for graduation.

[1] One of Zeno's paradoxes runs as follows: "If all that exists were in space, space also would have to exist in space, and so ad infinitum."

21.4. Other Kinds of Reality

Thus far our sole aim has been to present a clear view of *physical* reality and of the means whereby it is established. In achieving it we found it necessary to take cognizance of a type of reality different from that of our main query and yet closely associated with it. This was historical reality, the kind which the scientist encounters in his data. Now, as the end of our book approaches, we may perhaps take liberties and look around the fringes of our problem to see whether there are still other types of reality and what their claims for attention may be.

The philosopher would be shortsighted if he failed to see their presence. For convenience we shall put them into two classes, to be labeled *A* and *B*. Class *A* contains types of reality intrinsically unattainable by the methods described in this book, class *B* the types which may conceivably be exposed to view by a more extended application of scientific method than has at present been made. But before surveying them we shall pause briefly to summarize some essential aspects, often regarded as shortcomings, of physical reality, aspects which provide perennial motives for a search of other types.

Prime among these is the *immanent* character, to use a Kantian phrase, of scientific method and of physical reality. Being part of experience, physical reality cannot function as the why of experience. Before it, the fact of experience remains an unfathomed mystery. Although the account given in this book may seem pretentious by being systematic or by attempting to be complete—and I know how unwise it is nowadays to write systematic philosophy—the why of experience and hence the why of reality are problems it does not endeavor to solve. To be sure, reality can have no *cause* in the physical sense of the word. This invalidates our phrasing of the question but not its meaning. At this point, the scientist bows out, and the philosopher of existence enters the scene.

The rise of existential philosophies in our day, besides manifesting the grave emotional concern of mankind over the present state of the world, might almost be regarded as an indication of a growing acknowledgment of the sort of reasoning that fills this book. For in addressing themselves precisely to the fundamental problem left open by scientific method, these philosophies largely accept by implication what that method teaches. It seems that the division between philosophy of science and philosophy of existence represents the one natural and perhaps the ultimate cleavage of the philosophic field, and history may be headed in the direction of accepting this division.

We have said that immanence is one noteworthy aspect of physical reality which has driven speculation beyond science. The other is the dynamic quality of physical reality, called impermanence by its detractors. According to our view reality changes when new discoveries are made. But our indoctrination with principles of being and our historic concern over immutables make us want to say that our *knowledge* of reality changes when discoveries are made. When such a statement is not put to philosophic use, *i.e.*, when the scientist makes it, it is harmless, of course. When, on the other hand, it is taken for the starting point of philosophic pursuits, inquiry is off on an easy slide into uncharted territory. In this way the dissatisfaction with the dynamic aspects of physical reality, coupled with the more legitimate desire for a solution of the basic riddle of existence, has motivated the attempts of class *A* defined near the beginning of this section, attempts which endeavor to compound reality by methods essentially uncontrolled by science. We now turn to them.

A_1. The convergence theory has already been mentioned (Chap. 5). It accepts on the whole the epistemology of science but couples it with a belief in the uniqueness or predetermination of scientific progress, the end of which determines what is truly real. It is a curious mixture of knowledge and belief, a definition of the real in terms of an ideal. Science gives no assurance of ultimate convergence, nor does it deny the possibility of convergence. Hence the faith in question is not excluded on scientific grounds. It is a powerful and reasonable concomitant of scientific procedure, but it cannot define physical reality; it cannot show that atoms, stones, and stars are real in the manner in which science asserts their reality. The postulate of convergence is, in fact, a theory of history and not of science.

A_2. A view sometimes described as critical realism or, in Kant's case, as critical idealism seeks to establish permanence in the realm of being by postulating a secondary and a primary form of physical reality. Let us illustrate. In Fig. 21.1, P is again the plane of perception, and R_1 is the set of verifacts constituting reality according to our treatment. The theory under discussion surmises the existence, in a sense not definable by any procedures considered in this book, of a counterpart of R_1 which is here called R_2

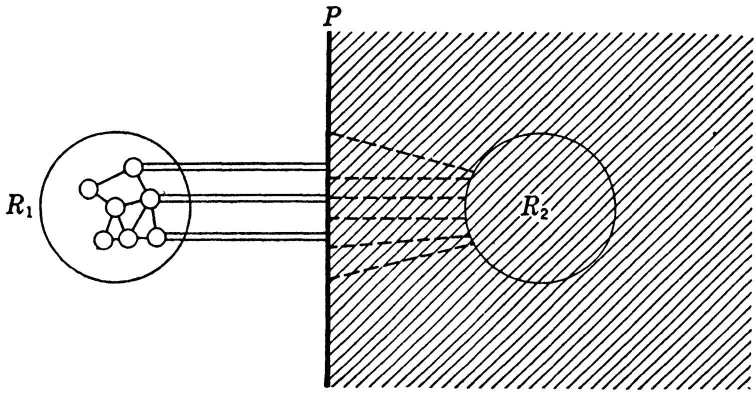

Figure 21.1

in the shaded (imperceptible) region beyond P. The constituents of R_2, the primary reality, somehow "effect" the perceptible emergence of data on P. The relation between R_2 and P, and hence finally between R_1 and R_2, may or may not be intrinsically unknowable. In the latter case the shaded region, though inaccessible to direct perception, is open to inference by procedures in R_1. But whatever supposition is adopted, the parts of R_2 are not real in the same sense as atoms, stones, and stars are real; hence we must say that R_2 does not define *physical* reality.

The theories labeled A_1 and A_2 can of course be combined, for it is fairly natural for the proponent of A_2 to look upon primary reality as the entelechy responsible for scientific progress and for its uniqueness. And clearly, there are many other views that can be fashioned on the model of these two. They have little to do with the subject matter of this book for reasons already stated.

B_1. We turn, then, to modes of reality belonging to class B (cf. above), realities essentially within reach but not yet embraced by scientific method. Here the range for speculation is unlimited, for who can foresee what results a continued application of epistemology, even in its present form, to psychology and the social sciences may bring? And, what is perhaps more important, there is the likelihood that unexpected discoveries in these less developed fields may occasion a change or an extension of the present metaphysics that will draw within the grasp of scientific method many phenomena now scientifically obscure. What would happen, for instance, if the postulate of causality were truly abandoned or if many-valued logics and their mathematical consequences were adopted, is at present anybody's guess.

For there can be science without causality, a science in which even the fundamental laws change in time. Some cosmological theories attempt this radical innovation, either by assuming certain universal constants of nature, like the speed of light and the charge of an electron, to change their values slowly as time goes on or by permitting the laws of physical nature to be different in different ages of the universe. If such a methodological change were some day to be accepted, that is, if scientists were to acquiesce to temporal variations of laws without endeavoring to find a causal explanation of the variations, the life sciences would be strangely affected. Laws regulating an organism in its infancy may then be different from the laws that govern its adolescent behavior; the psychological problem of learning, the problem of instincts and native skills in animals and man appear in a different light and are perhaps brought nearer solution. But such solutions are not generally considered acceptable today in view of our metaphysics.

It is much more difficult to imagine the effect upon science of a departure from two-valued logics, and there seems to be nothing in the present structure of the exact sciences which suggests such a departure. This is because all our theory today ultimately goes back to a limited set of mathematical operations, like the law for adding integers, and with respect to these laws *tertium non datur*. An equation like $p + q = r$ is *in principle* either true or false; it is quite impossible to choose integers p, q, and r for which this proposition is anything but true or false. A third category, "meaningless," can of course be introduced by violating the rule that these elements be integers; a statement like "An elephant + America = 9" clearly belongs to that class. But we have not developed thus far any form of usable mathematics that pays the slightest attention to such statements or, indeed, that does not break down when they are included in it.

Reichenbach [1] believes that quantum mechanics represents an application of many-valued calculi to physics. We fear that his suggestion does violence to the straightforward habits of that branch of science. On the experimental side, an observation always reveals one of only two alternatives: it answers yes or no to every acceptable question. So far as measurement is concerned, an electron is either present within a given volume or it is not. The fact that one may not draw the usual inferences concerning the electron's fate from such an observation is the responsibility of the laws of nature and not the laws of logic; it is in a basic sense not more significant than the triviality that the size of a ship allows no conclusion as to the name of its captain.

And on the side of theory the situation is in essence equally simple although different. When the state of an electron is given, then a statement like "The electron is at *xyz*" is indeed neither true nor false; it is in fact as obscure as "The electron is blue" and equally uninteresting. It is not asked, nor is any part of the theory based on it. But the prediction "The probability of finding an electron in a given volume (properly interpreted as frequency of observations) has the value ½" is always either true or false and can be verified or confuted.

Alterations in the metaphysics of science have in the past come very slowly, have come as a last resort of explanation in the wake of factual discoveries. Whether this will be true in the future or whether we are entering an era in which men more conscious of their metaphysic will successfully experiment with it no one can say. To do so would be a new way of experimenting with reality.

B_2. Profound changes in the meaning of reality will occur if the range of data, the P plane of experience, is enlarged even though all other elements of method remain the same. A strong plea for the desirability of such enlargement comes from the phe-

[1] H. Reichenbach, "Philosophical Foundation of Quantum Mechanics," University of California Press, Berkeley, 1944.

nomenologists, who wish to extend the area of immediacy in an interesting way. "Immediate 'seeing,'" says Husserl,[1] "not merely the sensory seeing of experience but seeing in general as primordial dator consciousness of any kind whatsoever, is the ultimate source of justification for all rational statements. Essential insight is a primordial dator act and as such analogous to sensory perception. . . ." Thus, by an examination of certain phases of pure consciousness, by direct *Wesensschau*, phenomenology wishes to uncover data having at least the trustworthiness of sensory experience to which science now limits its concern, hopes to discern what it calls "eidetic" truth, the stable component of a "presuppositionless" philosophy.

Science is probably too reserved in its reluctance to honor introspective evidence, and phenomenology presents a welcome challenge to it which can only be stimulating to both disciplines. Yet it must be seen that an uncritical admission of so-called eidetic structures of consciousness, on a par with what is now regarded as scientific data, is wholly disastrous. This is not because the former are intrinsically less true than the latter (which would be an erroneous judgment)—my inability to conceive of an object as being at the same time large and small is just as assured as the perception which reveals an object to be large—but rather because neither introspective nor external data carry within themselves any warrant of authenticity. *Science* has been able to develop *theories* furnishing criteria for the rejection of illusory data. Until similar criteria for excluding abortive introspections are at hand the scientist has a right to be skeptical about the proposals of phenomenology and about the forms of reality they envisage.

21.5. Physical Reality and Values

The last section has dealt with attempts at augmenting physical reality and with their prospects, but attention was confined to problems of factual apperception and knowledge. All these attempts share with the philosophy developed in this book the

[1] E. Husserl, "Ideas" (translated by W. R. B. Gibson), The Macmillan Company, New York, 1931. See also M. Farber, "The Foundations of Phenomenology," Harvard University Press, Cambridge, Mass., 1943.

quality of being insufficient for the representation of human experience in at least two major fields, the areas of feeling and of value judgments. It is of course not for the philosopher to say that these defects are necessary and are traceable to *fundamental* limitations of scientific method; nor is the scientist free to deny it. The proper attitude on the part of the philosopher is open-mindedness, while it is clearly the job of the scientist to recognize the insufficiencies and then to attempt their removal. He may not be successful; science may have essential limitations which are not at present within sight. But the gain for humanity that might attach to success is great enough to justify a straining after the smallest chance.

Let us not fall prey to an absurd and fraudulent argument which the enemies of science have concocted: that if science regulated the emotions, judgments, and actions of mankind, human beings would become regimented automata, and life would not be worth living. For there is a great difference between understanding phenomena well enough to control them when control is indicated, and not understanding them. Knowledge of the laws of motion of material bodies does not lessen one's pleasure in playing dice or in any activity where, for enjoyment or for any other purpose, one intentionally discards scientific treatment and invites the pleasant chicanery of chance; but such knowledge does help in the construction of houses and automobiles. To say that we are destined to become robots if society's functioning were known as accurately as the behavior of gases is an error because *knowing* and *regulating* are different things. Science may not create values, yet it alone provides the means for their realization.

Indifference of physical reality with respect to the affective qualities of experience is generally recognized and needs no emphasis here. A person dear to me is a valid construct in the same sense as, and therefore no more real than, the umbrella in my closet. Obviously, this kind of statement leaves out a wealth of aspects more cherished in life than the facts of reality. But here again it is not certain that the processes of scientific understanding, and with them the idea of reality, cannot be enlarged sufficiently to express that added richness of experience. There was a time in the history of the young science of colorimetry when representation was bound to the two dimensions of hue and brightness, and men had reason to suspect that this science would never render adequate account of the living vividness of actual color sensations. Today, with the introduction of saturation as a third variable in the description of color, we are very much nearer that goal. And so it may happen in the representation of emotions.

As to values, however, the story is somewhat different. In my view, which is not the only one consistent with the epistemological theory presented in this book and therefore cannot be argued without an appeal to extrascientific convictions, natural science contains no *normative* principles dealing with ultimate goals; physical reality is the quintessence of cognitive experience and not of values. Its significance is in terms of stable *relations* between phases of experience, and since it draws its power from relations, reality cannot create an *unconditional* "thou shalt." To know physical reality is to know where to look when something is wanted or needed to be seen; it is to be able to cure when a cure is desired, to kill when killing is intended. But natural science will never tell whether it is good or bad to look, to cure, or to kill. It simply lacks the premise of an "ought."

Now some oughts are very easily smuggled into science. One may say that psychology, anthropology, and sociology can determine what is good for the human species, then regard this as a scientific finding and base upon it a scientific code of ethics. The multiple reference to science in this and similar proposals is unwittingly designed to camouflage the fact that the statement is in fact a recommendation of hedonism, albeit in a modern and altruistic form. But never can it relieve us from the necessity of an ultimate nonscientific commitment, in this instance from a dedication of ourselves to the maxim that *it is good to seek the goal of hedonism*. Methodologically, the acceptance of this "doctrine" or any of its competitors in the field of ethics is the counterpart of an acceptance of the principles and postulates of science. In consequence of their lying at different parts but in the same basic stratum of experience, moral postulates are not reducible to scientific ones, nor the reverse. I should regard the failure to recognize this as an impediment to the progress of both science and philosophy, as comparable to the error on the part of a metaphysician of the old school who takes science to be the description of some sort of preformed being.

Because of this irreducibility, however, there arises the possibility of erecting ethics and science as parallel structures, of utilizing the *formal* principles of science in an endeavor to make the appeal of ethics more firm and more universal. This chance looms here as a vague but promising conjecture, which we can only allude to in this book. Yet it should perhaps be said that the considerations set forth in it were largely prompted by the hope that clarity with respect to scientific method, once attained, might later lead to a decision on the applicability of that method to problems in the normative disciplines: in this sense, the present volume merely covers the preliminaries for a larger and most pressing task.

Should this conjecture be true, then a subject like ethics must be developed in three interlocking stages. It must form a postulational discipline in which (*a*) a basic code is adopted, (*b*) that basic code is developed through its formal consequences and applied in action, (*c*) these actions or their social consequences are tested against some standard of validity. The outcome of such tests decides the ethical correctness of the moral code.

The first reaction to this graceless and seemingly artificial proposal must be one of thorough skepticism, based on the argument that there is no objective standard of ethical validity; that only the adopted code itself can confer ethical validity and that therefore the whole procedure of validation, stage (*c*), is a trivial, rubber-stamping referral of the results of a given set of ethical postulates to the postulates which generated these results. And this would seem unscientific, nay futile.

But let it be said with emphasis that verification in science is not a simple matter either, is in many instances an equally circular undertaking. To the nonscientist, verification is likely to mean a look-and-see procedure, an artless comparison of what is *predicted* with what *is*. The analysis of the preceding chapters has shown how far this is from being true, how naïve it is to suppose that a bare datum of immediate experience invariably carries theoretical significance. On the contrary, it is the formal structure of science that confers relevance on observations; theory determines to a large extent in what manner it will expose itself to test. Historically, therefore, the technique of verification, which is rarely present before a theory is born, developed along with scientific theory and attained refinement in the same measure as the theory did. Is it not unreasonable, then, to doubt the possibility of verifying ethical theory before trying to set up such a theory in postulational form?

In a crude way the opportunity for verifying ethical postulates is always present: it is to see whether the society practicing the code derived from the postulates survives. This corresponds to the naïve scientific look-and-see thesis. On the scientific side, refinement of that thesis came about as theories advanced, and there seems to be little hope for ethics except in this same procedure of starting the postulational formalism with wholehearted acquiescence and developing methods of verification as it goes forward.

But an even greater difficulty, though a less fundamental one, lies in the accomplishment of task (*a*), the acceptance of a basic code. There is the important problem of getting it adopted, and this problem has its taproot in a strange practical attitude with regard to the ethical postulates. Concerning them we often display an irrational insistence on a priori evidence; we demand that they come to us accredited with absolute certainty or with divine sanction, forgetting altogether that *scientific* postulates are initially tentative and are confirmed with use. The postulates of arithmetic are not CERTAIN nor are they TRUE, for there are many instances in our simplest experience which violate them thoroughly. One cloud in the sky plus another cloud do not always make two clouds. But successful science, valid in a large though not unlimited domain, has nevertheless proceeded from the axioms of arithmetic. Fuller realization of these facts may perchance make the problems of the normative sciences more tractable.

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