Philosophical Foundations of Physics

by Rudolph Carnap (1966)

CHAPTER 23
Theories and Nonobservables

ONE OF THE most important distinctions between two types of laws in science is the distinction between what may be called (there is no generally accepted terminology for them) empirical laws and theoretical laws. Empirical laws are laws that can be confirmed directly by empirical observations. The term "observable" is often used for any phenomenon that can be directly observed, so it can be said that empirical laws are laws about observable.

Here, a warning must be issued. Philosophers and scientists have quite different ways of using the terms "observable" and "nonobservable". To a philosopher, "observable" has a very narrow meaning. It applies to such properties as "blue", "hard", "hot". These are properties directly perceived by the senses. To the physicist, the word has a much broader meaning. It includes any quantitative magnitude that can be measured in a relatively simple, direct way. A philosopher would not consider a temperature of, perhaps, 80 degrees centigrade, or a weight of 93 pounds, an observable because there is no direct sensory perception of such magnitudes. To a physicist, both are observables because they can be measured in an extremely simple way. The object to be weighed is placed on a balance scale. The temperature is measured with a thermometer. The physicist would not say that the mass of a molecule, let alone the mass of an electron, is something observable, because here the procedures of measurement are much more complicated and indirect. But magnitudes that can be established by relatively simple procedures-length with a ruler, time with a clock, or frequency of light waves with a spectrometer-are called observables.

A philosopher might object that the intensity of an electric current is not really observed. Only a pointer position was observed. An ammeter was attached to the circuit and it was noted that the pointer pointed to a mark labelled 5.3. Certainly the current's intensity was not observed. It was inferred from what was observed.

The physicist would reply that this was true enough, but the inference was not very complicated. The procedure of measurement is so simple, so well established, that it could not be doubted that the ammeter would give an accurate measurement of current intensity. Therefore, it is included among what are called observables.

There is no question here of who is using the term "observable" in a right or proper way. There is a continuum which starts with direct sensory observations and proceeds to enormously complex, indirect methods of observation. Obviously no sharp line can be drawn across this continuum; it is a matter of degree. A philosopher is sure that the sound of his wife's voice, coming from across the room, is an observable. But suppose he listens to her on the telephone. Is her voice an observable or isn't it? A physicist would certainly say that when he looks at something through an ordinary microscope, he is observing it directly. Is this also the case when he looks into an electron microscope? Does he observe the path of a particle when he sees the track it makes in a bubble chamber? In general, the physicist speaks of observables in a very wide sense compared with the narrow sense of the philosopher, but, in both cases, the line separating observable from nonobservable is highly arbitrary. It is well to keep this in mind whenever these terms are encountered in a book by a philosopher or scientist. Individual authors will draw the line where it is most convenient, depending on their points of view, and there is no reason why they should not have this privilege.

Empirical laws, in my terminology, are laws containing terms either directly observable by the senses or measurable by relatively simple techniques. Sometimes such laws are called empirical generalisations, as a reminder that they have been obtained by generalising results found by observations and measurements. They include not only simple qualitative laws (such as, "All ravens are black") but also quantitative laws that arise from simple measurements. The laws relating pressure, volume, and temperature of gases are of this type. Ohm's law, connecting the electric potential difference, resistance, and intensity of current, is another familiar example. The scientist makes repeated measurements, finds certain regularities, and expresses them in a law. These are the empirical laws. As indicated in earlier chapters, they are used for explaining observed facts and for predicting future observable events.

There is no commonly accepted term for the second kind of laws, which I call theoretical laws. Sometimes they are called abstract or hypothetical laws. "Hypothetical" is perhaps not suitable because it suggests that the distinction between the two types of laws is based on the degree to which the laws are confirmed. But an empirical law, if it is a tentative hypothesis, confirmed only to a low degree, would still be an empirical law although it might be said that it was rather hypothetical. A theoretical law is not to be distinguished from an empirical law by the fact that it is not well established, but by the fact that it contains terms of a different kind. The terms of a theoretical law do not refer to observables even when the physicist's wide meaning for what can be observed is adopted. They are laws about such entities as molecules, atoms, electrons, protons, electromagnetic fields, and others that cannot be measured in simple, direct ways.

If there is a static field of large dimensions, which does not vary from point to point, physicists call it an observable field because it can be measured with a simple apparatus. But if the field changes from point to point in very small distances, or varies very quickly in time, perhaps changing billions of times each second, then it cannot be directly measured by simple techniques. Physicists would not call such a field an observable. Sometimes a physicist will distinguish between observables and nonobservables in just this way. If the magnitude remains the same within large enough spatial distances, or large enough time intervals, so that an apparatus can be applied for a direct measurement of the magnitude, it is called a macro-event. If the magnitude changes within such extremely small intervals of space and time that it cannot be directly measured by simple apparatus, it is a micro-event. (Earlier authors used the terms "microscopic" and "macroscopic", but today many authors have shortened these terms to "micro" and "macro".) A micro-process is simply a process involving extremely small intervals of space and time. For example, the oscillation of an electromagnetic wave of visible light is a micro-process. No instrument can directly measure how its intensity varies. The distinction between macro- and micro-concepts is sometimes taken to be parallel to observable and nonobservable. It is not exactly the same, but it is roughly so. Theoretical laws concern nonobservables, and very often these are micro-processes. If so, the laws are sometimes called micro-laws. I use the term "theoretical laws" in a wider sense than this, to include all those laws that contain nonobservables, regardless of whether they are micro-concepts or macro-concepts.

It is true, as shown earlier, that the concepts "observable" and "nonobservable" cannot be sharply defined because they lie on a continuum. In actual practice, however, the difference is usually great enough so there is not likely to be debate. All physicists would agree that the laws relating pressure, volume, and temperature of a gas, for example, are empirical laws. Here the amount of gas is large enough so that the magnitudes to be measured remain constant over a sufficiently large volume of space and period of time to permit direct, simple measurements which can then be generalised into laws. All physicists would agree that laws about the behaviour of single molecules are theoretical. Such laws concern a micro-process about which generalisations cannot be based on simple, direct measurements.

Theoretical laws are, of course, more general than empirical laws. It is important to understand, however, that theoretical laws cannot be arrived at simply by taking the empirical laws, then generalising a few steps further. How does a physicist arrive at an empirical law? He observes certain events in nature. He notices a certain regularity. He describes this regularity by making an inductive generalisation. It might be supposed that he could now put together a group of empirical laws, obsene some sort of pattern, make a wider inductive generalisation, and arrive at a theoretical law. Such is not the case.

To make this clear, suppose it has been observed that a certain iron bar expands when heated. After the experiment has been repeated many times, always with the same result, the regularity is generalised by saying that this bar expands when heated. An empirical law has been stated, even though it has a narrow range and applies only to one particular iron bar. Now further tests are made of other iron objects with the ensuing discovery that every time an iron object is heated it expands. This permits a more general law to be formulated, namely that all bodies of iron expand when heated. In similar fashion, the still more general laws "All metals . . .", then "All solid bodies . . .", are developed. These are all simple generalisations, each a bit more general than the previous one, but they are all empirical laws. Why? Because in each case, the objects dealt with are observable (iron, copper, metal, solid bodies); in each case the increases in temperature and length are measurable by simple, direct techniques.

In contrast, a theoretical law relating to this process would refer to the behaviour of molecules in the iron bar. In what way is the behaviour of the molecules connected with the expansion of the bar when heated? You see at once that we are now speaking of nonobservables. We must introduce a theory-the atomic theory of matter-and we are quickly plunged into atomic laws involving concepts radically different from those we had before. It is true that these theoretical concepts differ from concepts of length and temperature only in the degree to which they are directly or indirectly observable, but the difference is so great that there is no debate about the radically different nature of the laws that must be formulated.

Theoretical laws are related to empirical laws in a way somewhat analogous to the way empirical laws are related to single facts. An empirical law helps to explain a fact that has been observed and to predict a fact not yet observed. In similar fashion, the theoretical law helps to explain empirical laws already formulated, and to permit the derivation of new empirical laws. Just as the single, separate facts fall into place in an orderly pattern when they are generalised in an empirical law, the single and separate empirical laws fit into the orderly pattern of a theoretical law. This raises one of the main problems in the methodology of science. How can the kind of knowledge that will justify the assertion of a theoretical law be obtained? An empirical law may be justified by making observations of single facts. But to justify a theoretical law, comparable observations cannot be made because the entities referred to in theoretical laws are nonobservables.

Before taking up this problem, some remarks made in an earlier chapter, about the use of the word "fact", should be repeated. It is important in the present context to be extremely careful in the use of this word because some authors, especially scientists, use "fact" or "empirical fact" for some propositions which I would call empirical laws. For example, many physicists will refer to the 'fact" that the specific heat of copper is .090. I would call this a law because in its full formulation it is seen to be a universal conditional statement: "For any x, and any time t, if x is a solid body of copper, then the specific heat of x at t is .090." Some physicists may even speak of the law of thermal expansion, Ohm's law, and others, as facts. Of course, they can then say that theoretical laws help explain such facts. This sounds like my statement that empirical laws explain facts, but the word "fact" is being used here in two different ways. I restrict the word to particular, concrete facts that can be spatiotemporally specified, not thermal expansion in general, but the expansion of this iron bar observed this morning at ten o'clock when it was heated. It is important to bear in mind the restricted way in which I speak of facts. If the word "fact" is used in an ambiguous manner, the important difference between the ways in which empirical and theoretical laws serve for explanation will be entirely blurred.

How can theoretical laws be discovered? We cannot say: "Let's just collect more and more data, then generalise beyond the empirical laws until we reach theoretical ones." No theoretical law was ever found that way. We observe stones and trees and flowers, noting various regularities and describing them by empirical laws. But no matter how long or how carefully we observe such things, we never reach a point at which we observe a molecule. The term "molecule" never arises as a result of observations. For this reason, no amount of generalisation from observations will ever produce a theory of molecular processes. Such a theory must arise in another way. It is stated not as a generalisation of facts but as a hypothesis. The hypothesis is then tested in a manner analogous in certain ways to the testing of an empirical law. From the hypothesis, certain empirical laws are derived, and these empirical laws are tested in turn by observation of facts. Perhaps the empirical laws derived from the theory are already known and well confirmed. (Such laws may even have motivated the formulation of the theoretical law.) Regardless of whether the derived empirical laws are known and confirmed, or whether they are new laws confirmed by new observations, the confirmation of such derived laws provides indirect confirmation of the theoretical law.

The point to be made clear is this. A scientist does not start with one empirical law, perhaps Boyle's law for gases, and then seek a theory about molecules from which this law can be derived. The scientist tries to formulate a much more general theory from which a variety of empirical laws can be derived. The more such laws, the greater their variety and apparent lack of connection with one another, the stronger will be the theory that explains them. Some of these derived laws may have been known before, but the theory may also make it possible to derive new empirical laws which can be confirmed by new tests. If this is the case, it can be said that the theory made it possible to predict new empirical laws. The prediction is understood in a hypothetical way. If the theory holds, certain empirical laws will also hold. The predicted empirical law speaks about relations between observables, so it is now possible to make experiments to see if the empirical law holds. If the empirical law is confirmed, it provides indirect confirmation of the theory. Every confirmation of a law, empirical or theoretical, is, of course, only partial, never complete and absolute. But in the case of empirical laws, it is a more direct confirmation. The confirmation of a theoretical law is indirect, because it takes place only through the confirmation of empirical laws derived from the theory.

The supreme value of a new theory is its power to predict new empirical laws. It is true that it also has value in explaining known empirical laws, but this is a minor value. If a scientist proposes a new theoretical system, from which no new laws can be derived, then it is logically equivalent to the set of all known empirical laws. The theory may have a certain elegance, and it may simplify to some degree the set of all known laws, although it is not likely that there would be an essential simplification. On the other hand, every new theory in physics that has led to a great leap forward has been a theory from which new empirical laws could be derived. If Einstein had done no more than propose his theory of relativity as an elegant new theory that would embrace certain known laws- perhaps also simplify them to a certain degree-then his theory would not have had such a revolutionary effect.

Of course it was quite otherwise. The theory of relativity led to new empirical laws which explained for the first time such phenomena as the movement of the perihelion of Mercury, and the bending of light rays in the neighbourhood of the sun. These predictions showed that relativity theory was more than just a new way of expressing the old laws. Indeed, it was a theory of great predictive power. The consequences that can be derived from Einstein's theory are far from being exhausted. These are consequences that could not have been derived from earlier theories. Usually a theory of such power does have an elegance, and a unifying effect on known laws. It is simpler than the total collection of known laws. But the great value of the theory lies in its power to suggest new laws that can be confirmed by empirical means.

CHAPTER 24
Correspondence Rules

AN IMPORTANT qualification must now be added to the discussion of theoretical laws and terms given in the last chapter. The statement that empirical laws are derived from theoretical laws is an oversimplification. It is not possible to derive them directly because a theoretical law contains theoretical terms, whereas an empirical law contains only observable terms.

This prevents any direct deduction of an empirical law from a theoretical one.

To understand this, imagine that we are back in the nineteenth century, preparing to state for the first time some theoretical laws about molecules in a gas. These laws are to describe the number of molecules per unit volume of the gas, the molecular velocities, and so forth. To simplify matters, we assume that all the molecules have the same velocity. (This was indeed the original assumption; later it was abandoned in favour of a certain probability distribution of velocities.) Further assumptions must be made about what happens when molecules collide. We do not know the exact shape of molecules, so let us suppose that they are tiny spheres. How do spheres collide? There are laws about colliding spheres, but they concern large bodies. Since we cannot

directly observe molecules, we assume their collisions are analogous to those of large bodies; perhaps they behave like perfect billiard balls on a frictionless table. These are, of course, only assumptions; guesses suggested by analogies with known macro-laws.

But now we come up against a difficult problem. Our theoretical laws deal exclusively with the behaviour of molecules, which cannot be seen. How, therefore, can we deduce from such laws a law about observable properties such as the pressure or temperature of a gas or properties of sound waves that pass through the gas? The theoretical laws contain only theoretical terms. What we seek are empirical laws containing observable terms. Obviously, such laws cannot be derived without having something else given in addition to the theoretical laws.

The something else that must be given is this: a set of rules connecting the theoretical terms with the observable terms. Scientists and philosophers of science have long recognised the need for such a set of rules, and their nature has been often discussed. An example of such a rule is: "If there is an electromagnetic oscillation of a specified frequency, then there is a visible greenish-blue colour of a certain hue." Here something observable is connected with a nonobservable micro-process.

Another example is: "The temperature (measured by a thermometer and, therefore, an observable in the wider sense explained earlier) of a gas is proportional to the mean kinetic energy of its molecules." This rule connects a nonobservable in molecular theory, the kinetic energy of molecules, with an observable, the temperature of the gas. If statements of this kind did not exist, there would be no way of deriving empirical laws about observables from theoretical laws about nonobservables.

Different writers have different names for these rules. I call them "correspondence rules". P. W. Bridgman calls them operational rules. Norman R. Campbell speaks of them as the "Dictionary".' Since the rules connect a term in one terminology with a term in another terminology, the use of the rules is analogous to the use of a French-English dictionary. What does the French word "cheval" mean? You look it up in the dictionary and find that it means "horse". It is not really that simple when a set of rules is used for connecting nonobservables with observables; nevertheless, there is an analogy here that makes Campbell's "Dictionary" a suggestive name for the set of rules.

There is a temptation at times to think that the set of rules provides a means for defining theoretical terms, whereas just the opposite is really true. A theoretical term can never be explicitly defined on the basis of observable terms, although sometimes an observable can be defined in theoretical terms. For example, "iron" can be defined as a substance consisting of small crystalline parts, each having a certain arrangement of atoms and each atom being a configuration of particles of a certain type. In theoretical terms then, it is possible to express what is meant by the observable term "iron", but the reverse is not true.

There is no answer to the question: "Exactly what is an electron?" Later we shall come back to this question, because it is the kind that philosophers are always asking scientists. They want the physicist to tell them just what he means by "electricity", "magnetism", "gravity", "a molecule". If the physicist explains them in theoretical terms, the philosopher may be disappointed. "That is not what I meant at all", he will say. "I want you to tell me, in ordinary language, what those terms mean." Sometimes the philosopher writes a book in which he talks about the great mysteries of nature. "No one", he writes, "has been able so far, and perhaps no one ever will be able, to give us a straightforward answer to the question: 'What is electricity?' And so electricity remains forever one of the great, unfathomable mysteries of the universe."

There is no special mystery here. There is only an improperly phrased question. Definitions that cannot, in the nature of the case, be given, should not be demanded. If a child does not know what an elephant is, we can tell him it is a huge animal with big ears and a long trunk. We can show him a picture of an elephant. It serves admirably to define an elephant in observable terms that a child can understand. By analogy, there is a temptation to believe that, when a scientist introduces theoretical terms, he should also be able to define them in familiar terms. But this is not possible. There is no way a physicist can show us a picture of electricity in the way he can show his child a picture of an elephant. Even the cell of an organism, although it cannot be seen with the unaided eye, can be represented by a picture because the cell can be seen when it is viewed through a microscope. But we do not possess a picture of the electron. We cannot say how it looks or how it feels, because it cannot be seen or touched. The best we can do is to say that it is an extremely small body that behaves in a certain manner. This may seem to be analogous to our description of an elephant. We can describe an elephant as a large animal that behaves in a certain manner. Why not do the same with an electron?

The answer is that a physicist can describe the behaviour of an electron only by stating theoretical laws, and these laws contain only theoretical terms. They describe the field produced by an electron, the reaction of an electron to a field, and so on. If an electron is in an electrostatic field, its velocity will accelerate in a certain way. Unfortunately, the electron's acceleration is an unobservable. It is not like the acceleration of a billiard ball, which can be studied by direct observation. There is no way that a theoretical concept can be defined in terms of observables. We must, therefore, resign ourselves to the fact that definitions of the kind that can be supplied for observable terms cannot be formulated for theoretical terms.

It is true that some authors, including Bridgman, have spoken of the rules as "operational definitions". Bridgman had a certain justification, because he used his rules in a somewhat different way, I believe, than most physicists use them. He was a great physicist and was certainly aware of his departure from the usual use of rules, but he was willing to accept certain forms of speech that are not customary, and this explains his departure. It was pointed out in a previous chapter that Bridgman preferred to say that there is not just one concept of intensity of electric current, but a dozen concepts. Each procedure by which a magnitude can be measured provides an operational definition for that magnitude. Since there are different procedures for measuring current, there are different concepts. For the sake of convenience, the physicist speaks of just one concept of current. Strictly speaking, Bridgman believed, he should recognise many different concepts, each defined by a different operational procedure of measurement.

We are faced here with a choice between two different physical languages. If the customary procedure among physicists is followed, the various concepts of current will be replaced by one concept. This means, however, that you place the concept in your theoretical laws, because the operational rules are just correspondence rules, as I call them, which connect the theoretical terms with the empirical ones. Any claim to possessing a definition-that is, an operational definition of the theoretical concept must be given up. Bridgman could speak of having operational definitions for his theoretical terms only because he was not speaking of a general concept. He was speaking of partial concepts, each defined by a different empirical procedure.

Even in Bridgman's terminology, the question of whether his partial concepts can be adequately defined by operational rules is problematic. Reichenbach speaks often of what he calls "correlative definitions". (In his German publications, he calls them *Zuordnungsdefinitionen*, from *zuordnen*, which means to correlate.) Perhaps correlation is a better term than definition for what Bridgman's rules actually do. In geometry, for instance, Reichenbach points out that the axiom system of geometry, as developed by David Hilbert, for example, is an uninterpreted axiom system. The basic concepts of point, line, and plane could just as well be called "class alpha", "class beta", and "class gamma". We must not be seduced by the sound of familiar words, such as "point" and "line", into thinking they must be taken in their ordinary meaning. In the axiom system, they are uninterpreted terms. But when geometry is applied to physics, these terms must be connected with something in the physical world. We can say, for example, that the lines of the geometry are exemplified by rays of light in a vacuum or by stretched cords. In order to connect the uninterpreted terms with observable physical phenomena, we must have rules for establishing the connection.

What we call these rules is, of course, only a terminological question; we should be cautious and not speak of them as definitions. They are not definitions in any strict sense. We cannot give a really adequate definition of the geometrical concept of "line" by referring to anything in nature. Light rays, stretched strings, and so on are only approximately straight; moreover, they are not lines, but only segments of lines. In geometry, a line is infinite in length and absolutely straight. Neither property is exhibited by any phenomenon in nature. For that reason, it is not possible to give an operational definition, in the strict sense of the word, of concepts in theoretical geometry. The same is true of all the other theoretical concepts of physics. Strictly speaking, there are no "definitions" of such concepts. I prefer not to speak of "operational definitions" or even to use Reichenbach's term "correlative definitions". In my publications (only in recent years have I written about this question), I have called them "rules of correspondence" or, more simply, "correspondence rules".

Campbell and other authors often speak of the entities in theoretical physics as mathematical entities. They mean by this that the entities are related to each other in ways that can be expressed by mathematical functions. But they are not mathematical entities of the sort that can be defined in pure mathematics. In pure mathematics, it is possible to define various kinds of numbers, the function of logarithm, the exponential function, and so forth. It is not possible, however, to define such terms as "electron" and "temperature" by pure mathematics. Physical terms can be introduced only with the help of non-logical constants, based on observations of the actual world. Here we have an essential difference between an axiomatic system in mathematics and an axiomatic system in physics.

If we wish to give an interpretation to a term in a mathematical axiom system, we can do it by giving a definition in logic. Consider, for example, the term "number" as it is used in Peano's axiom system. We can define it in logical terms, by the Frege-Russell method, for example. In this way the concept of "number" acquires a complete, explicit definition on the basis of pure logic. There is no need to establish a connection between the number 5 and such observables as "blue" and "hot". The terms have only a logical interpretation; no connection with the actual world is needed. Sometimes an axiom system in mathematics is called a theory. Mathematicians speak of set theory, group theory, matrix theory, probability theory. Here the word "theory" is used in a purely analytic way. It denotes a deductive system that makes no reference to the actual world. We must always bear in mind that such a use of the word "theory" is entirely different from its use in reference to empirical theories such as relativity theory, quantum theory, psychoanalytical theory, and Keynesian economic theory.

A postulate system in physics cannot have, as mathematical theories have, a splendid isolation from the world. Its axiomatic terms- "electron", "field", and so on-must be interpreted by correspondence rules that connect the terms with observable phenomena. This interpretation is necessarily incomplete. Because it is always incomplete, the system is left open to make it possible to add new rules of correspondence. Indeed, this is what continually happens in the history of physics. I am not thinking now of a revolution in physics, in which an entirely new theory is developed, but of less radical changes that modify existing theories. Nineteenth-century physics provides a good example, because classical mechanics and electromagnetics had been established, and, for many decades, there was relatively little change in fundamental laws. The basic theories of physics remained unchanged. There was, however, a steady addition of new correspondence rules, because new procedures were continually being developed for measuring this or that magnitude.

Of course, physicists always face the danger that they may develop correspondence rules that will be incompatible with each other or with the theoretical laws. As long as such incompatibility does not occur, however, they are free to add new correspondence rules. The procedure is never-ending. There is always the possibility of adding new rules, thereby increasing the amount of interpretation specified for the theoretical terms; but no matter how much this is increased, the interpretation is never final. In a mathematical system, it is otherwise. There a logical interpretation of an axiomatic term is complete. Here we find another reason for reluctance in speaking of theoretical terms as "defined" by correspondence rules. It tends to blur the important distinction between the nature of an axiom system in pure mathematics and one in theoretical physics.

Is it not possible to interpret a theoretical term by correspondence rules so completely that no further interpretation would be possible? Perhaps the actual world is limited in its structure and laws. Eventually a point may be reached beyond which there will be no room for strengthening the interpretation of a term by new correspondence rules. Would not the rules then provide a final, explicit definition for the term? Yes, but then the term would no longer be theoretical. It would become part of the observation language. The history of physics has not yet indicated that physics will become complete; there has been only a steady addition of new correspondence rules and a continual modification in the interpretations of theoretical terms. There is no way of knowing whether this is an infinite process or whether it will eventually come to some sort of end.

It may be looked at this way. There is no prohibition in physics against making the correspondence rules for a term so strong that the term becomes explicitly defined and therefore ceases to be theoretical. Neither is there any basis for assuming that it will always be possible to add new correspondence rules. Because the history of physics has shown such a steady, unceasing modification of theoretical concepts, most physicists would advise against correspondence rules so strong that a theoretical term becomes explicitly defined. Moreover, it is a wholly unnecessary procedure. Nothing is gained by it. It may even have the adverse effect of blocking progress.

Of course, here again we must recognise that the distinction between observables and nonobservables is a matter of degree. We might give an explicit definition, by empirical procedures, to a concept such as length, because it is so easily and directly measured, and is unlikely to be modified by new observations. But it would be rash to seek such strong correspondence rules that "electron" would be explicitly defined. The concept "electron" is so far removed from simple, direct observations that it is best to keep it theoretical, open to modifications by new observations.

CHAPTER 25
How New Empirical Laws Are Derived from Theoretical Laws

IN CHAPTER 24, the discussion concerned the ways in which correspondence rules are used for linking the nonobservable terms of a theory with the observable terms of empirical laws. This can be made clearer by a few examples of the manner in which empirical laws have actually been derived from the laws of a theory.

The first example concerns the kinetic theory of gases. Its model, or schematic picture, is one of small particles called molecules, all in constant agitation. In its original form, the theory regarded these particles as little balls, all having the same mass and, when the temperature of the gas is constant, the same constant velocity. Later it was discovered that the gas would not be in a stable state if each particle had the same velocity; it was necessary to find a certain probability distribution of velocities that would remain stable. This was called the Boltzmann-Maxwell distribution. According to this distribution, there was a certain probability that any molecule would be within a certain range on the velocity scale.

When the kinetic theory was first developed, many of the magnitudes occurring in the laws of the theory were not known. No one knew the mass of a molecule, or how many molecules a cubic centimetre of gas at a certain temperature and pressure would contain. These magnitudes were expressed by certain parameters written into the laws. After the equations were formulated, a dictionary of correspondence rules was prepared. These correspondence rules connected the theoretical terms with observable phenomena in a way that made it possible to determine indirectly the values of the parameters in the equations. This, in turn, made it possible to derive empirical laws. One correspondence rule states that the temperature of the gas corresponds to the mean kinetic energy of the molecules. Another correspondence rule connects the pressure of the gas with the impact of molecules on the confining wall of a vessel. Although this is a discontinuous process involving discrete molecules, the total effect can be regarded as a constant force pressing on the wall. Thus, by means of correspondence rules, the pressure that is measured macroscopically by a manometer (pressure gauge) can be expressed in terms of the statistical mechanics of molecules.

What is the density of the gas? Density is mass per unit volume, but how do we measure the mass of a molecule? Again our dictionary- a very simple dictionary-supplies the correspondence rule. The total mass M of the gas is the sum of the masses m of the molecules. M is observable (we simply weigh the gas), but m is theoretical. The dictionary of correspondence rules gives the connection between the two concepts. With the aid of this dictionary, empirical tests of various laws derived from our theory are possible. On the basis of the theory, it is possible to calculate what will happen to the pressure of the gas when its volume remains constant and its temperature is increased. We can calculate what will happen to a sound wave produced by striking the side of the vessel, and what will happen if only part of the gas is heated. These theoretical laws are worked out in terms of various parameters that occur within the equations of the theory. The dictionary of correspondence rules enables us to express these equations as empirical laws, in which concepts are measurable, so that empirical procedures can supply values for the parameters. If the empirical laws can be confirmed, this provides indirect confirmation of the theory. Many of the empirical laws for gases were known, of course, before the kinetic theory was developed. For these laws, the theory provided an explanation. In addition, the theory led to previously unknown empirical laws.

The power of a theory to predict new empirical laws is strikingly exemplified by the theory of electromagnetism, which was developed about 1860 by two great English physicists, Michael Faraday and James Clerk Maxwell. (Faraday did most of the experimental work, and Maxwell did most of the mathematical work.) The theory dealt with electric charges and how they behaved in electrical and magnetic fields. The concept of the electron-a tiny particle with an elementary electric charge-was not formulated until the very end of the century. Maxwell's famous set of differential equations, for describing electromagnetic fields, presupposed only small discrete bodies of unknown nature, capable of carrying an electric charge or a magnetic pole. What happens when a current moves along a copper wire? The theory's dictionary made this observable phenomenon correspond to the actual movement along the wire of little charged bodies. From Maxwell's theoretical model, it became possible (with the help of correspondence rules, of course) to derive many of the known laws of electricity and magnetism.

The model did much more than this. There was a certain parameter c in Maxwell's equations. According to his model, a disturbance in an electromagnetic field would be propagated by waves having the velocity c. Electrical experiments showed the value of c to be approximately 3×10^{10} centimetres per second. This was the same as the known value for the speed of light, and it seemed unlikely that it was an accident. Is it possible, physicists asked themselves, that light is simply a special case of the propagation of an electromagnetic oscillation? It was not long before Maxwell's equations were providing explanations for all sorts of optical laws, including refraction, the velocity of light in different media, and many others.

Physicists would have been pleased enough to find that Maxwell's model explained known electrical and magnetic laws; but they received a double bounty. The theory also explained optical laws! Finally, the great strength of the new model was revealed in its power to predict, to formulate empirical laws that had not been previously known.

The first instance was provided by Heinrich Hertz, the German physicist. About 1890, he began his famous experiments to see whether electromagnetic waves of low frequency could be produced and detected in the laboratory. Light is an electromagnetic oscillation and propagation of waves at very high frequency. But Maxwell's laws made it possible for such waves to have any frequency. Hertz's experiments resulted in his discovery of what at first were called Hertz waves. They are now called radio waves. At first, Hertz was able to transmit these waves from one oscillator to another over only a small distance-first a few centimetres, then a meter or more. Today a radio broadcasting station sends its waves many thousands of miles.

The discovery of radio waves was only the beginning of the derivation of new laws from Maxwell's theoretical model. X rays were discovered and were thought at first to be particles of enormous velocity and penetrative power. Then it occurred to physicists that, like light and radio waves, these might be electromagnetic waves, but of extremely high frequency, much higher than the frequency of visible light. This also was later confirmed, and laws dealing with X rays were derived from Maxwell's fundamental field equations. X rays proved to be waves of a certain frequency range within the much broader frequency band of gamma rays. The X rays used today in medicine are simply gamma rays of certain frequency. All this was largely predictable on the basis of Maxwell's model. His theoretical laws, together with the correspondence rules, led to an enormous variety of new empirical laws.

The great variety of fields in which experimental confirmation was found contributed especially to the strong overall confirmation of Maxwell's theory. The various branches of physics had originally developed for practical reasons; in most cases, the divisions were based on our different sense organs. Because the eyes perceive light and colour, we call such phenomena optics; because our ears hear sounds, we call a branch of physics acoustics; and because our bodies feel heat, we have a theory of heat. We find it useful to construct simple machines based on the movements of bodies, and we call it mechanics. Other phenomena, such as electricity and magnetism, cannot be directly perceived, but their consequences can be observed.

In the history of physics, it is always a big step forward when one branch of physics can be explained by another. Acoustics, for instance, was found to be only a part of mechanics, because sound waves are simply elasticity waves in solids, liquids, and gases. We have already spoken of how the laws of gases were explained by the mechanics of moving molecules. Maxwell's theory was another great leap forward toward the unification of physics. Optics was found to be a part of electromagnetic theory. Slowly the notion grew that the whole of physics might some day be unified by one great theory. At present there is an enormous gap between electromagnetism on the one side and gravitation on the other. Einstein made several attempts to develop a unified field theory that might close this gap; more recently, Heisenberg and others have made similar attempts. So far, however, no theory has been devised that is entirely satisfactory or that provides new empirical laws capable of being confirmed.

Physics originally began as a descriptive macrophysics, containing an enormous number of empirical laws with no apparent connections. In the beginning of a science, scientists may be very proud to have discovered hundreds of laws. But, as the laws proliferate, they become unhappy with this state of affairs; they begin to search for underlying, unifying principles. In the nineteenth century, there was considerable controversy over the question of underlying principles. Some felt that science must find such principles, because otherwise it would be no more than a description of nature, not a real explanation. Others thought that that was the wrong approach, that underlying principles belong only to metaphysics. They felt that the scientist's task is merely to describe, to find out how natural phenomena occur, not why.

Today we smile a bit about the great controversy over description versus explanation. We can see that there was something to be said for both sides, but that their way of debating the question was futile. There is no real opposition between explanation and description. Of course, if description is taken in the narrowest sense, as merely describing what a certain scientist did on a certain day with certain materials, then the opponents of mere description were quite right in asking for more, for a real explanation. But today we see that description in the broader sense, that of placing phenomena in the context of more general laws, provides the only type of explanation that can be given for phenomena. Similarly, if the proponents of explanation mean a metaphysical explanation, not grounded in empirical procedures, then their opponents were correct in insisting that science should be concerned only with description. Each side had a valid point. Both description and explanation, rightly understood, are essential aspects of science.

The first efforts at explanation, those of the Ionian natural philosophers, were certainly partly metaphysical; the world is all fire, or all water, or all change. Those early efforts at scientific explanation can be viewed in two different ways. We can say: "This is not science, but pure metaphysics. There is no possibility of confirmation, no correspondence rules for connecting the theory with observable phenomena." On the other hand, we can say: "These Ionian theories are certainly not scientific, but at least they are pictorial visions of theories. They are the first primitive beginnings of science."

It must not be forgotten that, both in the history of science and in the psychological history of a creative scientist, a theory has often first appeared as a kind of visualisation, a vision that comes as an inspiration to a scientist long before he has discovered correspondence rules that may help in confirming his theory. When Democritus said that everything consists of atoms, he certainly had not the slightest confirmation for this theory. Nevertheless, it was a stroke of genius, a profound insight, because two thousand years later his vision was confirmed. We should not, therefore, reject too rashly any anticipatory vision of a theory, provided it is one that may be tested at some future time. We are on solid ground, however, if we issue the warning that no hypothesis can claim to be scientific unless there is the possibility that it can be tested. It does not have to be confirmed to be a hypothesis, but there must be correspondence rules that will permit, in principle, a means of confirming or disconfirming the theory. It may be enormously difficult to think of experiments that can test the theory; this is the case today with various unified field theories that have been proposed. But if such tests are possible in principle, the theory can be called a scientific one. When a theory is first proposed, we should not demand more than this.

The development of science from early philosophy was a gradual, step-by-step process. The Ionian philosophers had only the most primitive theories. In contrast, the thinking of Aristotle was much clearer and on more solid scientific ground. He made experiments, and he knew the importance of experiments, although in other respects he was an apriorist. This was the beginning of science. But it was not until the time of Galileo Galilei, about 1600, that a really great emphasis was placed on the experimental method in preference to aprioristic reasoning about nature. Even though many of Galileo's concepts had previously been stated as theoretical concepts, he was the first to place theoretical physics on a solid empirical foundation. Certainly Newton's physics (about 1670) exhibits the first comprehensive, systematic theory, containing unobservables as theoretical concepts: the universal force of gravitation, a general concept of mass, theoretical properties of light rays, and so on. His theory of gravity was one of great generality. Between any two particles, small or large, there is a force proportional to the square of the distance between them. Before Newton advanced this theory, science provided no explanation that applied to both the fall of a stone and the movements of planets around the sun.

It is very easy for us today to remark how strange it was that it never occurred to anyone before Newton that the same force might cause the apple to drop and the moon to go around the earth. In fact, this was not a thought likely to occur to anyone. It is not that the answer was so difficult to give; it is that nobody had asked the question. This is a vital point. No one had asked: "What is the relation between the forces that heavenly bodies exert upon each other and terrestrial forces that cause objects to fall to the ground?" Even to speak in such terms as "terrestrial" and "heavenly" is to make a bipartition, to cut nature into two fundamentally different regions. It was Newton's great insight to break away from this division, to assert that there is no such fundamental cleavage. There is one nature, one world. Newton's universal law of gravitation was the theoretical law that explained for the first time both the fall of an apple and Kepler's laws for the movements of planets. In Newton's day, it was a psychologically difficult, extremely daring adventure to think in such general terms.

Later, of course, by means of correspondence rules, scientists discovered how to determine the masses of astronomical bodies. Newton's theory also said that two apples, side by side on a table, attract each other. They do not move toward each other because the attracting force is extremely small and the friction on the table very large. Physicists eventually succeeded in actually measuring the gravitational forces between two bodies in the laboratory. They used a torsion balance consisting of a bar with a metal ball on each end, suspended at its center by a long wire attached to a high ceiling. (The longer and thinner the wire, the more easily the bar would turn.) Actually, the bar never came to an absolute rest but always oscillated a bit. But the mean point of the bar's oscillation could be established. After the exact position of the mean point was determined, a large pile of lead bricks was constructed near the bar. (Lead was used because of its great specific gravity. Gold has an even higher specific gravity, but gold bricks are expensive.) It was found that the mean of the oscillating bar had shifted a tiny amount to bring one of the balls on the end of the bar nearer to the lead pile. The shift was only a fraction of a millimetre, but it was enough to provide the first observation of a gravitational effect between two bodies in a laboratory-an effect that had been predicted by Newton's theory of gravitation.

It had been known before Newton that apples fall to the ground and that the moon moves around the earth. Nobody before Newton could have predicted the outcome of the experiment with the torsion balance. It is a classic instance of the power of a theory to predict a new phenomenon not previously observed.

CHAPTER 26
The Ramsey Sentence

[I use ***E*** for the mathematical logic symbol meaning "there exists"]

SCIENTIFIC THEORY, in the sense in which we are using the term-theoretical postulates combined with correspondence rules that join theoretical and observational terms- has in recent years been intensely analysed and discussed by philosophers of science. Much of this discussion is so new that it has not yet been published. In this chapter, we will introduce an important new approach to the topic, one that goes back to a little known paper by the Cambridge logician and economist, Frank Plumpton Ramsey.

Ramsey died in 1930 at the age of twenty-six. He did not live to complete a book, but after his death a collection of his papers was edited by Richard Bevan Braithwaite and published in 1931 as *The Foundations of Mathematics*. A short paper entitled "Theories" appears in this book. In my opinion, this paper deserves much more recognition than it has received. Perhaps the book's title attracted only readers interested in the logical foundations of mathematics, so that other important papers in the book, such as the paper on theories, tended to be overlooked.

Ramsey was puzzled by the fact that the theoretical terms- terms for the objects, properties, forces, and events described in a theory- are not meaningful in the same way that observational terms-"iron rod", "hot", and "red"-are meaningful. How, then, does a theoretical term acquire meaning? Everyone agrees that it derives its meaning from the context of the theory. "Gene" derives its meaning from genetic theory. "Electron" is interpreted by the postulates of particle physics. But we are faced with many confusing, disturbing questions. How can the empirical meaning of a theoretical term be determined? What does a given theory tell us about the actual world? Does it describe the structure of the real world, or is it just an abstract, artificial device for bringing order into the large mass of experiences in somewhat the same way that a system of accounting makes it possible to keep orderly records of a firm's financial dealings? Can it be said that an electron "exists" in the same sense that an iron rod exists?

There are procedures that measure a rod's properties in a simple, direct manner. Its volume and weight can be determined with great accuracy. We can measure the wave lengths of light emitted by the surface of a heated iron rod and precisely define what we mean when we say that the iron rod is "red". But when we deal with the properties of theoretical entities, such as the "spin" of an elementary particle, there are only complicated, indirect procedures for giving the term an empirical meaning. First we must introduce "spin" in the context of an elaborate theory of quantum mechanics, and then the theory must be connected with laboratory observables by another complex set of postulates-the correspondence rules. Clearly, spin is not empirically grounded in the simple, direct manner that the redness of a heated iron rod is grounded. Exactly what is its cognitive status? How can theoretical terms, which must in some way be connected with the actual world and subject to empirical testing, be distinguished from those metaphysical terms so often encountered in traditional philosophy-terms that have no empirical meaning? How can the right of a scientist to speak of theoretical concepts be justified, without at the same time justifying the right of a philosopher to use metaphysical terms?

In seeking answers to these puzzling questions, Ramsey made a novel, startling suggestion. He proposed that the combined system of theoretical and correspondence postulates of a theory be replaced by what is today called the "Ramsey sentence of the theory". In the Ramsey sentence, which is equivalent to the theory's postulates, theoretical terms do not occur at all. In other words, the puzzling questions are neatly side-stepped by the elimination of the very terms about which the questions are raised.

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Suppose we are concerned with a theory containing n theoretical terms: "T_1", "T_2", "T_3" . . . "T_n". These terms are introduced by the postulates of the theory. They are connected with directly observable terms by the theory's correspondence rules. In these correspondence rules occur m observational terms: "O_1", "O_2", "O_3" . . . "O_m". The theory itself is a conjunction of all the theoretical postulates together with all the correspondence postulates. A full statement of the theory, therefore, will contain the combined sets of T- and O-terms: "T_1", "T_2", "T_3" . . . "T_n"; "O_1", "O_2", "O_3" . . . "O_n". Ramsey proposed that, in this sentence, the full statement of the theory, all the theoretical terms are to be replaced by corresponding variables: "U_1", "U_2", "U_3" . . . "U_n", and that what logicians call "existential quantifiers"-'($\boldsymbol{E}U_1$)', '($\boldsymbol{E}U_2$)', . . ., '($\boldsymbol{E}u_n$)' - be added to this formula. It is this new sentence, with its U-variables and their existential quantifiers, that is called the "Ramsey sentence".

To see exactly how this develops, consider the following example. Take the symbol "Mol" for the class of molecules. Instead of calling something "a molecule", call it "an element of Mol". Similarly, "Hymol" stands for "the class of hydrogen molecules", and "a hydrogen molecule" is "an element of Hymol". It is assumed that a space-time coordinate system has been fixed, so that a space-time point can be represented by its four coordinates: x, y, z, t. Adopt the symbol "Temp" for the concept of temperature. Then, "the (absolute) temperature of the body b, at time t, is 500" can be written, "Temp(b,t) = 500". Temperature is thus expressed as a relation involving a body, a time point, and a number. "The pressure of a body b, at time t", can be written, "Press(b,t)". The concept of mass is represented by the symbol "Mass". For "the mass of the body b (in grams) is 150" write, "Mass(b) = 150". Mass is a relation between a body and a number. Let "Vel" stand for the velocity of a body (it may be a macro- or a micro-body). For example, "Vel(b,r) = (r_l, r_2, r_3)", where the right side of the equation refers to a triple of real numbers, namely, the components of the velocity in the directions of x, y, and z. Vel is thus a relation concerning a body, a time coordinate, and a triple of real numbers.

Generally speaking, the theoretical language contains "class terms" (such as terms for macro-bodies, micro-bodies, and events) and "relation terms" (such as terms for various physical magnitudes).

Consider theory TC. (The "T" stands for the theoretical postulates of the theory, and "C' stands for the postulates that give the correspondence rules.) The postulates of this theory include some laws from the kinetic theory of gases, laws concerning the motions of molecules, their velocities, collisions, and so on. There are general laws about any gas, and there are special laws about hydrogen. In addition, there are macro-gas-theory laws about the temperature, pressure, and total mass of a (macro-) gas body. Suppose that the theoretical postulates of theory TC contain all the terms mentioned above. For the sake of brevity, instead of writing out in full all the T-postulates, write only the theoretical terms, and indicate the connecting symbolism by dots:

(T) . . . Mol . . . Hymol . . . Temp . . . Press . . . Mass # . . Vel . . .

To complete the symbolisation of theory TC, the correspondence postulates for some, but not necessarily all, of the theoretical terms must be considered. These C-postulates may be operational rules for the measurement of temperature and pressure (that is, a description of the construction of a thermometer and a manometer and rules for determining the values of temperature and pressure from the numbers read on the scales of the instruments). The C-postulates will contain the theoretical terms "Temp" and "Press" as well as a number of observational terms: "O_1", "O_2",

"O_3" . . . "O_m". Thus, the C-postulates can be expressed in a brief, abbreviated way by writing:

(C) . . . Temp . . .O_1, . . .O_2, . . . O_3 . .
Press . . . O_4 . . . O_m . .

The entire theory can now be indicated in the following form:

(TC) ... Mol . . . Hymol . . . Temp . . . Press . . . Mass . . . Vel . . .; . . . Temp . . .O_1, . . .O_2, . . . O_3 . . ... Press . . . . . O_4 . . . O_m . .

To transform this theory TC into its Ramsey sentence, two steps are required. First, replace all the theoretical terms (class terms and relation terms) with arbitrarily chosen class and relation variables. Wherever "Mol" occurs in the theory, substitute the variable "C_1", for example. Wherever "Hymol" occurs in the theory replace it by another class variable, such as "C_2". The relation term "Temp" is replaced everywhere (both in the T and C portions of the theory) by a relation variable, such as "R1". In the same way, "Press", "Mass", and "Vel" are replaced by three other relation variables, "R2", "R3", and "R4" respectively, for example. The final result may be indicated in this way:

.. C_1 . . . C_2. . . R_1 . . . R_2 .. . R_3. . . R_4. . .; . . . R_1 . . . O_1 . . . O_2 ... O_3... R_2... O_4.. O_m ...

This result (which should be thought of as completely written out, rather than abbreviated as it is here with the help of dots) is no longer a sentence (as T, C, and TC are). It is an open sentence formula or, as it is sometimes called, a sentence form or a sentence function.

The second step, which transforms the open sentence formula into the Ramsey sentence, ^{R}TC, consists of writing in front of the sentence formula six existential quantifiers, one for each of the six variables:

(^{R}TC) (***E*** C_1) (***E*** C_2)(***E*** R_1)(***E*** R_2)(***E*** R_3)(***E*** R_4)[. . . C_1 . . . C_2. . . R_1. . . R_2. . .. R_3. . . R_4. . .; . . . R_1 . . . O_1 . . . O_2 . . O_3 . . . R_2. .. . O_4 . . . O_m .--]

A formula preceded by an existential quantifier asserts that there is at least one entity (of the type to which it refers) that satisfies the condition expressed by the formula. Thus, the Ramsey sentence indicated above says (roughly speaking) that there is (at least) one class C_1, one class C_2, one relation R_1, one R_2, one R_3 and one R_4, such that:

(1) these six classes and relations are connected with one another in a specified way (namely, as specified in the first or T part of the formula),

(2) the two relations, R1 and R2~~ are connected with the m observational entities, $O_1, \ldots, O_m$ in a certain way (namely, as specified in the second or C part of the formula).

The important thing to note is that in the Ramsey sentence the theoretical terms have disappeared. In their place are variables. The variable "C_1" does not refer to any particular class. The assertion is only that there is at least one class that satisfies certain conditions. The meaning of the Ramsey sentence is not changed in any way if the variables are arbitrarily changed. For example, the symbols "C_1" and "C_2" can be interchanged or replaced with other arbitrary variables, such as ' X_1 and "X_2". The meaning of the sentence remains the same.

It may appear that the Ramsey sentence is no more than just an other somewhat roundabout way of expressing the original theory. In a sense, this is true. It is easy to show that any statement about the real world that does not contain theoretical terms-that is, any statement capable of empirical confirmation-that follows from the theory will also follow from the Ramsey sentence. In other words, the Ramsey sentence has precisely the same explanatory and predictive power as the original system of postulates. Ramsey was the first to see this. It was an important insight, although few of his colleagues gave it much attention. One of the exceptions was Braithwaite, who was Ramsey's friend and who edited his papers. In his book, *Scientific Explanation* (1953), Braithwaite discusses Ramsey's insight, emphasising its importance.

The important fact is that we can now avoid all the troublesome metaphysical questions that plague the original formulation of theories and can introduce a simplification into the formulation of theories. Before, we had theoretical terms, such as "electron", of dubious "reality" because they were so far removed from the observable world. Whatever partial empirical meaning could be given to these terms could be given only by the indirect procedure of stating a system of theoretical postulates and connecting those postulates with empirical observations by means of correspondence rules. In Ramsey's way of talking about the external world, a term such as "electron" vanishes. This does not in any way imply that electrons vanish, or, more precisely, that whatever it is in the external world that is symbolised by the word "electron" vanishes. The Ramsey sentence continues to assert, through its existential quantifiers, that there is something in the external world that has all those properties that physicists assign to the electron. It does not question the existence-the "reality"-of this something. It merely proposes a different way of talking about that something. The troublesome question it avoids is not, "Do electrons exist?" but, "What is the exact meaning of the term 'electron'?" In Ramsey's way of speaking about the world, this question does not arise. It is no longer necessary to inquire about the meaning of "electron", because the term itself does not appear in Ramsey's language.

It is important to understand-and this point was not sufficiently stressed by Ramsey-that Ramsey's approach cannot be said to bring theories into the observation language if "observation language" means (as is often the case) a language containing only observational terms and the terms of elementary logic and mathematics. Modern physics demands extremely complicated, high-level mathematics. Relativity theory, for instance, calls for non-Euclidean geometry and tensor calculus, and quantum mechanics calls for equally sophisticated mathematical concepts. It cannot be said, therefore, that a physical theory, expressed as a Ramsey sentence, is a sentence in a simple observational language. It requires an extended observational language, which is observational because it contains no theoretical terms, but has been extended to include an advanced, complicated logic, embracing virtually the whole of mathematics.

Suppose that, in the logical part of this extended observation language, we provide for a series D_0, D_1, D_2, . . . of domains of mathematical entities such that:

(1) The domain Do contains the natural numbers (0, 1, 2, . .)

(2) For any domain D_ns the domain D_{n+1} contains all classes of elements of D_n.

The extended language contains variables for all these kinds of entities, together with suitable logical rules for using them. It is my opinion that this language is sufficient, not only for formulating all present theories of physics, but also for all future theories, at least for a long time to come. Of course, it is not possible to foresee the kinds of particles, fields, interactions, or other concepts that physicists may introduce in future centuries. However, I believe that such theoretical concepts, regardless of how bizarre and complex they may be, can-by means of Ramsey's device-be formulated in essentially the same extended observation language that is now available, which contains the observational terms combined with advanced logic and mathematics.

On the other hand, Ramsey certainly did not mean-and no one has suggested-that physicists should abandon theoretical terms in their speech and writing. To do so would require enormously complicated statements. For example, it is easy to say in the customary language that a certain object has a mass of five grams. In the symbolic notation of a theory, before it is changed to a Ramsey sentence, one can say that a certain object No. 17 has a mass of five grams by writing, "Mass (17) = 5". In Ramsey's language, however, the theoretical term "Mass" does not appear. There is only the variable (as in the previous example) "R3". How can the sentence "Mass (17) = 5" be translated into Ramsey's language? "R_3 (17) = 5" obviously will not do; it is not even a sentence. The formula must be supplemented by the assumptions concerning the relation R_3 that are specified in the Ramsey sentence. Moreover, it would not be sufficient to pick out only those postulateformulas containing "R_3". An the postulates are needed. Therefore, the translation of even this brief sentence into the Ramsey language demands an immensely long sentence, which contains the formulas corresponding to all the theoretical postulates, all the correspondence postulates, and their existential quantifiers. Even when the abbreviated form used earlier is adopted, the translation is rather long:

$(\boldsymbol{E}\,C_1)\,(\boldsymbol{E}\,C_2)\,.\,.\,.\,(\boldsymbol{E}\,R_3)\,(\boldsymbol{E}\,R_4)\,[.\,.\,.\,C_1\,.\,.\,.\,C_2\,.\,.\,.\,R_1\,.\,.\,.\,R_2\,.\,.\,.\,R_3\,.\,.\,.\,R_4\,.\,.\,.;\,.\,.\,.\,R_1\,.\,.\,O_1\,.\,.\,.\,O_2\,.\,.\,.\,O_3\,.\,.\,.\,R_2\,.\,.\,.\,O_4\,.\,.\,.\,O_m\,.\,.\,.$ and $R_3(17) = 5]$.

It is evident that it would be inconvenient to substitute the Ramsey way of speaking for the ordinary discourse of physics in which theoretical terms are used. Ramsey merely meant to make clear that it was possible to formulate any theory in a language that did not require theoretical terms but that said the same thing as the conventional language.

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When we say it "says the same thing", we mean this only so far as all observable consequences are concerned. It does not, of course, say exactly the same thing. The former language presupposes that theoretical terms, such as "electron" and "mass", point to something that is somehow more than what is supplied by the context of the theory itself. Some writers have called this the "surplus meaning" of a term. When this surplus meaning is taken into account, the two languages are certainly not equivalent. The Ramsey sentence represents the full observational content of a theory. It was Ramsey's great insight that this observational content is all that is needed for the theory to function as theory, that is, to explain known facts and predict new ones.

It is true that physicists find it vastly more convenient to talk in the shorthand language that includes theoretical terms, such as "proton", "electron", and "neutron". But if they are asked whether electrons "really" exist, they may respond in different ways. Some physicists are content to think about such terms as "electron" in the Ramsey way. They evade the question about existence by stating that there are certain observable events, in bubble chambers and so on, that can be described by certain mathematical functions, within the framework of a certain theoretical system. Beyond that they will assert nothing. To ask whether there really are electrons is the same- from the Ramsey point of view-as asking whether quantum physics is true. The answer is that, to the extent that quantum physics has been confirmed by tests, it is justifiable to say that there are instances of certain kinds of events that, in the language of the theory, are called "electrons".

This point of view is sometimes called the "instrumentalist" view of theories. It is close to the position defended by Charles Peirce, John Dewey, and other pragmatists, as well as by many other philosophers of science. From this point of view, theories are not about "reality". They are simply language tools for organising the observational phenomena of experience into some sort of pattern that will function efficiently in predicting new observables. The theoretical terms are convenient symbols. The postulates containing them are adopted because they are useful, not because they are "true". They have no surplus meaning beyond the way in which they function in the system. It is meaningless to talk about the "real" electron or the "real" electromagnetic field.

Opposed to this view is the "descriptive" or "realist" view of theories. (Sometimes these two are distinguished, but it is not necessary to delve into these subtle differences.) Advocates of this approach find it both convenient and psychologically comforting to think of electrons, magnetic fields, and gravitational waves as actual entities about which science is steadily learning more. They point out that there is no sharp line separating an observable, such as an apple, from an unobservable, such as a neutron. An amoeba is not observable by the naked eye, but it is observable through a light microscope. A virus is not observable even through a light microscope, but its structure can be seen quite distinctly through an electron microscope. A proton cannot be observed in this direct way, but its track through a bubble chamber can be observed. If it is permissible to say that the amoeba is "real", there is no reason why it is not permissible to say that the proton is equally real. The changing view about the structure of electrons, genes, and other things does not mean that there is not something "there", behind each observable phenomenon; it merely indicates that more and more is being learned about the structure of those entities.

Proponents of the descriptive view remind us that unobservable entities have a habit of passing over into the observable realm as more powerful instruments of observation are developed. At one time, "virus" was a theoretical term. The same is true of "molecule". Ernst Mach was so opposed to thinking of a molecule as an existing "thing" that he once called it a "valueless image". Today, even atoms in a crystal lattice can be photographed by bombarding them with elementary particles; in a sense, the atom itself has become an observable. Defenders of this view argue that it is as reasonable to say that an atom "exists" as it is to say that a distant star, observable only as a faint spot of light on a long-exposed photographic plate, exists. There is, of course, no comparable way to observe an electron. But that is no reason for refusing to say it exists. Today, little is known about its structure; tomorrow a great deal may be known. It is as correct, say the advocates of the descriptive approach, to speak of an electron as an existing thing as it is to speak of apples and tables and galaxies as existing things.

It is obvious that there is a difference between the meanings of the instrumentalist and the realist ways of speaking. My own view, which I shall not elaborate here, is that the conflict between the two approaches is essentially linguistic. It is a question of which way of speaking is to be preferred under a given set of circumstances. To say that a theory is a reliable instrument-that is, that the predictions of observable events that it yields will be confirmed-is essentially the same as saying that the theory is true and that the theoretical, unobservable entities it speaks about exist. Thus, there is no incompatibility between the thesis of the instrumentalist and that of the realist. At least, there is no incompatibility so long as the former avoids such negative assertions as, ". . . but the theory does not consist of sentences which are either true or false, and the atoms, electrons, and the like do not really exist".

Source: *Philosophical Foundations of Physics* (1966) publ. Basic Books Inc. Chapters 23 to 26 reproduced here.