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* *These last three volumes, taken together, constitute the POSTSCRIPT TO THE LOGIC OF SCIENTIFIC DISCOVERY*

QUANTUM THEORY AND THE SCHISM IN PHYSICS

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Note on Numbering of Sections. The sections in each of the three volumes of the *Postscript* are numbered consecutively, beginning with section 1. The original section numbers, indicating the order of the sections within the *Postscript* as a whole, are given in starred brackets in the Tables of Contents. Ed.

EDITOR'S FOREWORD

1. *The* Postscript *and Its History.*

THIS book, *Quantum Theory and the Schism in Physics*, is the third volume of Sir Karl Popper's long-awaited *Postscript* to *The Logic of Scientific Discovery*. Although it was written some twenty-five years ago, it has never before been published. Yet it remains as timely as when it was first written: it challenges some of the most fundamental assumptions of current research in physics; and it also sketches a new cosmology.

Together with other parts of the *Postscript* (all of which are now being published), this volume was written mainly during the years 1951–56, at the time when *Logik der Forschung*, Popper's first published book (1934), was being translated into English as *The Logic of Scientific Discovery*.

The different volumes of the *Postscript* were originally part of a series of Appendices to *The Logic of Scientific Discovery*, in which Popper proposed to correct, expand, and develop the ideas of his first book. Some of these Appendices were in fact included in *The Logic of Scientific Discovery* when it was published in 1959. But one group of the Appendices took on a life of its own, and gradually grew into a single, closely integrated work far exceeding the original *Logik der Forschung* in length. It was decided to publish this new work—called the *Postscript: After Twenty Years*—as a sequel or companion volume to *The Logic of Scientific Discovery*. And it was accordingly set up in type, in galley proofs, in 1956–57.

Within a few months of the anticipated publication, however, the project came grinding to a halt. In *Unended Quest*, his intellectual autobiography, Sir Karl has reported of these galley proofs: 'Proof-reading turned into a nightmare. . . . I then had to have operations

Not that I think that criticizing Einstein or Schrödinger is *lèse majesté*; after all, I have myself criticized Einstein's determinism, his and Schrödinger's intermittent inclination towards a subjective theory of probability, and in part also Schrödinger's particle theory. But I feel that the manner in which they have sometimes been criticized is symptomatic of lack of appreciation.

About Einstein I hardly need to say what anybody who has any knowledge of the history of human thought can fail to realize: that perhaps no other man since Galileo, Kepler, and Newton has done so much to open our minds.

As to Schrödinger, many of the physicists of the new generation know him mainly from the text-books, as the famous author of the wave equation. This is a pity. For Schrödinger's *Collected Papers on Wave Mechanics* are a classic. I think they are unique. The directness of Schrödinger's approach, the depth and the sheer beauty of his ideas, his lucid presentation, his wonderful detachment and self-irony, the fact that he looks for, and finds, a refutation: the whole is a wonderful human document, a work of art, an adventure of ideas which has few parallels.

And it is *physics*, in the grand style. Schrödinger is not only the real father of the *formalism* of the quantum theory, he is first of all a physicist who tries to understand the physical world in which we live. He is, like Einstein, a true heir to the speculations of Faraday, and he is the first to have shown us that matter may one day be explained as a disturbance of something that is not in its turn material.

I do not think there is any chance that this something will turn out to be 'mind-like' or 'spiritual', as some apologists of the spiritual try to make out. Nor do I think that the human mind or the human spirit needs an apologist; or that we can anticipate its future adventures and discoveries.

As to the great founders of the new theory of matter—from Planck, Einstein, Bohr and de Broglie to Schrödinger, Pauli, Born, Heisenberg and Dirac—I may be allowed to apply to them the words of a master. 'I cannot express strongly enough my unbounded admiration for the greatness of mind of these men', Galileo wrote of the founders of the heliocentric system, 'who conceived it, and held it to be true . . . in violent opposition to the evidence of their own senses.'

A METAPHYSICAL EPILOGUE

> There is something besides extension, even something that is prior to extension.
>
> LEIBNIZ[1]

INDETERMINISM and the propensity interpretation of probability allow us to paint a new picture of the physical world. According to this picture, of which only the roughest sketch can be given here, all properties of the physical world are dispositional, and the real state of a physical system, at any moment, may be conceived as the sum total of its dispositions—or its potentialities, or possibilities, or propensities.

Change, according to this picture, consists in the realization or actualization of some of these potentialities. These realizations in their turn consist again of dispositions or potentialities—potentialities, though, that differ from those whose realizations they are. This view corresponds closely to the commonsense view of the world. When a student enters the university there is a certain possibility inherent in this situation of his sitting his finals; of his answering the examination questions more or less successfully; and of his obtaining a degree. When he sits his finals he is realizing the first of these possibilities or potentialities; and at the same time, the new situation which has arisen changes the other two possibilities. In fact, the sitting of his finals can be said to be essentially a possibility of his answering examination questions more or less successfully, and of obtaining his degree. When he has given his answers, and when he has thus realized another set of possibilities or

[1] 'Est aliquid praeter extensionem imo extensione prius', quoted by Kant in *Gedanken von der wahren Schätzung der lebendigen Kräfte und Beurteilung der Beweise*, 1746, end of the first main section, § 1, in *Werke*, ed. Ernst Cassirer, Vol. I, p. 15.

potentialities, a new situation has been created which again changes the potentialities of his obtaining a degree. When he ultimately gets his degree, and thus again realizes certain potentialities, then the new situation creates new potentialities—relating, say, to his making use of his degree, or not making use of it, in his career. We thus obtain a picture of the world which is at once dualistic and monistic. It is dualistic in that the potentialities are potentialities only relative to their possible realizations or actualizations; and it is monistic in that the realizations or actualizations not only determine potentialities, but may even be said to be potentialities themselves. (But we should perhaps avoid saying that they are 'nothing but' potentialities.) Thus we may describe the physical world as consisting of *changing propensities for change*. Although these propensities do not in general determine future changes, they may determine, in some fields of physics at least, the probability distributions—which may include probabilities equal to 1—of the various possible future changes.

This approach contains the suggestion of a theory of matter, or of particles, according to which these are interpreted as realizations of potentialities or propensities, and at the same time as consisting of potentialities or propensities in their turn.

One of the main points of this approach is the suggestion that it may be possible, in this way, to give an indeterministic re-interpretation of Einstein's deterministic programme, and at the same time an objectivistic and realistic re-interpretation of quantum theory. The aim is a picture of a world in which there is room for biological phenomena, for human freedom, and for human reason.

20. *Metaphysical Ideas and Research Programmes, and the History of Physics.*

One day somebody should write the history of physics as a *history of its problem situations*. (This, incidentally, is the way in which all history should be written, I believe, including political history.) Problem situations as they affect the history of physics (as opposed to, say, political history) may be analyzed almost completely in purely logical terms, provided we take notice of the metaphysical ideas which contribute to the creation of problems and

which largely determine the direction in which we seek for a solution.[1]

In science, problem situations are the result, as a rule, of three factors. One is the discovery of an inconsistency within the ruling theory. A second is the discovery of an inconsistency between theory and experiment—the experimental falsification of the theory. The third, and perhaps the most important one, is the relation between the theory and what may be called the '*metaphysical research programme*'.[2]

In using this term I wish to draw attention to the fact that in almost every phase of the development of science we are under the sway of metaphysical—that is, untestable—ideas; ideas which not only determine what problems of explanation we shall choose to attack, but also what kinds of answers we shall consider as fitting or satisfactory or acceptable, and as improvements of, or advances on, earlier answers.

By raising the problems of explanation which the theory is designed to solve, the metaphysical research programme makes it possible to judge the success of the theory as an explanation. On the other hand, the critical discussion of the theory and of its results may lead to a change in the research programme (usually an unconscious change, as the programme is often held unconsciously, and taken for granted), or to its replacement by another programme. These programmes are only occasionally discussed as such: more often, they are implicit in the theories and in the attitudes and judgements of the scientists.

I call these research programmes 'metaphysical' also because they result from general views of the structure of the world and, at the same time, from general views of the problem situation in physical cosmology. I call them 'research programmes' because they incorporate, together with a view of what the most pressing problems are, a general idea of what a satisfactory solution of these problems would look like (*cp. Realism and the Aim of Science*, Volume I of this *Postscript*, section 15). They may be described as *speculative*

[1][See Popper on problem situations and historical writing in *Conjectures and Refutations*, 1963, pp. 66–96; *The Self and Its Brain*, 1977, pp. 151 *ff; Objective Knowledge*, 1972, pp. 166–190. Ed.]

[2][*Cp.* Popper's discussion of metaphysical research programmes in *Unended Quest*, section 33; and in the 'Preface 1982' to the present volume. Ed.]

physics, or perhaps as speculative anticipations of testable physical theories.

In the present section, I shall first give, in approximately historical order, a bare list of ten of the more important metaphysical research programmes that have influenced the development of physics since the days of Pythagoras and Heraclitus; and more especially, since Parmenides, when the *problem of change* became the fundamental problem of metaphysics and of physics.[3]

After having given this list, I shall attempt to add, as an illustration, an outline of the sequence of problem situations that led from Descartes's programme towards Faraday's.

1. *Parmenides's Block Universe.* The nothing (void, empty space) cannot exist: the world is full, it is one block. Motion and change are impossible. A true picture of the world must be rational; that is, based on deduction, and upon the principle of non-contradiction.[4]

2. *Atomism.* Motion, and therefore change, are real. Thus the world cannot be full; the void must exist. The world consists of atoms *and* the void—of 'the full' *and* 'the empty'. *All change is explicable by the movement of the atoms in the void.* There is no qualitative change—only movement and structural change, that is, rearrangement. The void is the space for the possible movements and positions of the atoms.

3. *Geometrization.* An early Pythagorean programme had been the arithmetization of cosmology (including geometry); it broke down with the discovery of the irrationals. Plato turned the tables on this programme; he first conceived the geometrization of cosmology (including arithmetic). The physical world is space filled by matter. Matter is *formed* (or shaped, or moulded) space, and since geometry is the theory of shapes, and of space, the fundamental properties of matter are explained geometrically (in the *Timaeus*). The geometrization of the cosmos and of arithmetic is carried on by Eudoxus, Callippus, and by Euclid. (Euclid did not

[3] *Cp.* my paper 'The Nature of Philosophical Problems', in *Conjectures and Refutations,* 1963, pp. 66–96.

[4] [See also Popper's discussions of Parmenides in *Conjectures and Refutations,* 1963, pp. 38, 79n, 80–1, 142, 145*f,* 147, 150, 159, 400–1, and 405–13; and in *The Self and Its Brain,* 1977, pp. 5, 15, 153. Ed.]

intend to write a textbook of geometry, but to solve the problem of a geometrical theory of the irrationals, and other fundamental problems of Platonic cosmology.[5])

4. *Essentialism and Potentialism.* For Aristotle, space (*topos,* position space) is matter, and pure geometry loses its central place which is taken up by a dualism of matter and form (or essence). *The form or essence of a thing inheres in it, and contains its potentialities.* These realize themselves for the sake of their *final* cause, the end, the aim. (The good is self-realization.)[6]

5. *Renaissance* physics (Copernicus, Bruno, Kepler, Galileo, Descartes) is largely a revival of Plato's geometrical cosmology, his *antecedent* causes ('*universale ante rem*'), and his hypothetico-deductive method. (A little later there is also a revival of atomism.) It turns into:

6. *The Clockwork Theory of the World* (Hobbes, Descartes, Boyle). The essence or form of matter is identical with its spatial extension. (This is a combination of Platonic and Aristotelian ideas.)[7] Thus all physical theories must be geometrical. All physical causation is push or, more generally, action at vanishing distance. All qualitative changes are quantitative-geometrical movements of matter; for example, of the fluid of heat (caloric), or of magnetism, or of electricity. (Compare also the Bernoullian conjecture that atoms are vortices of the ether.)

7. *Dynamism.* All physical causation is to be explained either by push, or else by central attractive forces (Newton). Every *change* of a physical state is functionally dependent upon another *change.* (Principle of differential equations.) According to Leibniz, push must also be explained by forces—by central repulsive forces—because push can only be explained if matter is space *filled by (repulsive) forces.* Thus Leibniz has a dynamical and structural theory of matter. (The theory of central forces was further developed by Kant and by Boscovich.)[8]

[5] See my paper 'The Nature of Philosophical Problems', *op. cit.* [See also 'Back to the Pre-Socratics', in *Conjectures and Refutations,* 1963, pp. 136–165. Ed.]

[6] [See Popper's discussion of Aristotle's essentialism in *The Open Society and Its Enemies,* 1945, Chapter 11. Ed.]

[7] [See further discussions by Popper in 'Philosophy and Physics', *Atti del XII Congresso Internazionale di Filosofia* (Venice, 1958) 2, Florence, 1960, pp. 367–74; and in *The Self and Its Brain,* 1977, especially Chapters P1 and P5. Ed.]

[8] [See further discussions by Popper in *The Self and Its Brain,* 1977, pp. 6, 177–96, Ed.]

8. *Fields of Forces* (Faraday, Maxwell). Not all forces are central forces. There are changing fields of (vectorial) forces whose local changes are dependent upon local changes at vanishing distances. (Principle of partial differential equations, combining Newton's and Descartes's principles of causality.) Matter—that is, atoms or molecules—may become explicable in terms of fields of forces, or of *disturbances* of fields of forces. (*Cp.* Bernoulli's theory mentioned above, under point 6.)

9. *Unified Field Theory* (Riemann, Einstein, Schrödinger). Geometrization of the fields, gravitational as well as electromagnetic. Maxwell's field theory of light is generalized to a field theory of particles, and thus of matter. Matter is predicted to be destructible (with the confirmation of this prediction, the clockwork cosmology—that is to say materialism—is refuted) and to be inter-convertible with radiation, that is to say, with field energy, and thus with geometrical properties of space. However, the view that matter is a disturbance (vibration) of the field is opposed by:

10. *The Statistical Interpretation of Quantum Theory* (Born). Since Einstein's photon theory it becomes doubtful whether even light is nothing but a Maxwellian disturbance, a vibration of the field. For with every train of vibrations (light waves) there is associated a particle-like entity, a photon, which is emitted by one atom and absorbed by one atom. According to de Broglie, there is a similar dualism of particle and wave for particles of matter. Now this dualism is interpreted by Born in a way which may be described almost as a reversion to atomism in its original sense: what exists are corpuscles or particles; and the field and its vibration merely represent the mathematical instruments of an indeterminist particle-physics by which we may calculate the purely statistical probability of finding a particle in a certain state. This view (which I largely subscribed to in *L.Sc.D.*, 1934) appears to be incompatible with the programme of a monistic unified field theory of matter; and what looks like the majority of physicists take the success of the statistical quantum theory to indicate that the Faraday-Einstein-Schrödinger programme of a field theory of matter has to be given up.[9]

[9]Heisenberg expressed this view very clearly in 'Der Begriff "Abgeschlossene Theorie" in der modernen Naturwissenschaft', *Dialectica* 2, 1948, pp. 334–6. I

This brief survey may help us to understand the fundamental problems of physical cosmology, and why they were fundamental. I have in mind such problems as the problem of change in general; of matter and space (of atoms and the void); of the spatial structure of the universe; of causation (of action at a distance or at vanishing distances; of forces; and fields of forces); of the (atomic) structure of matter, and especially of its stability, and the limits to its stability; and of the interaction of matter and light.

It is interesting to note that there have been only three theories of change so far: atomism, which explains qualitative changes by quantitative movements of matter; Aristotle's theory of potentialities and their actualization or realization, which is a qualitative theory; and the theory of disturbances (vibrations, waves) of fields—like atomism a theory which aims at explaining qualitative changes quantitatively, but by changing intensities rather than by the movement of extended matter. [*(Added 1981) Heraclitus, I think, proposed only a programme; and Parmenides's theory was one of non-change.]

Such research programmes are, generally speaking, indispensable for science, although their character is that of metaphysical or speculative physics rather than of scientific physics. Originally they were all metaphysical, in nearly every sense of the word (although some of them became scientific in time); they were vast generalizations, based upon various intuitive ideas, most of which now strike us as mistaken. They were unifying pictures of the world—the real world. They were highly speculative; and they were, originally, non-testable. Indeed they may all be said to have been more of the nature of myths, or of dreams, than of science. But they helped to give science its problems, its purposes, and its inspiration.

An illustration, if only a sketchy one, may be useful here, and I will therefore conclude this section by giving an outline of the history of a particular problem situation—that of the problem of matter since Descartes. (It is part of the history of the transition from the clockwork theory of the world to dynamism, that is to say, from programme 6 to 7.)

criticized his view briefly in my 'Three Views Concerning Human Knowledge', 1955, reprinted in *Conjectures and Refutations*, 1963, pp. 113 *f.* Heisenberg later changed his mind. [See the preceding chapters of this volume of the *Postscript*. Ed.]

The history of the problem of matter has been sketched before, notably by Maxwell (in his wonderful article 'Atom' in the 9th edition, 1875, of the *Encyclopaedia Britannica*). But though Maxwell gave a sketch of the history of the relevant physical and philosophical ideas, he failed to give *the history of the problem situation, and of how this situation changed under the impact of the attempted solutions.* It is this lacuna that I am trying to fill here.[10]

Descartes based the whole of his physics upon an essentialist or Aristotelian definition of body or matter: a body is, in its essence or substance, extended, and matter is, in its essence or substance, extension. (Thus matter is extended substance, as opposed to mind which, as thinking or experiencing substance, is in essence *intensity.*) Since body or matter is identical with extension, all extension, all space, is body or matter: the world is full: there is no void. This is Parmenides's theory, as Descartes understood it. But while Parmenides concluded that there can be no motion in a full world, Descartes accepted a view that can be traced back to a suggestion from Plato's *Timaeus* according to which motion is as possible in a full world as it is in a bucket of water: things may move in a full world by pushing one another round in vortices; they may move like tea leaves in a tea cup.

In this Cartesian world, all causation is action by contact: it is push. In a *plenum,* an extended body can only move by *pushing* other bodies. All physical change must be explicable in terms of clockwork mechanisms (or vortices) in which the various moving parts *push* one another along. *Push is the principle of mechanical explanation.* There can be no action at a distance. (I may mention in parentheses that Newton himself regarded action at a distance at times as an absurd idea, and at other times as a supernatural phenomenon.)

This Cartesian system of speculative mechanics was criticized by Leibniz, on purely speculative grounds. Leibniz accepted the Cartesian fundamental equation, *body = extension.* But while Descartes believed that this equation was ultimate and irreducible, self-evident, clear, and distinct, Leibniz questioned all this: if a body pushes another body along instead of penetrating it, then this

[10]For many years I was in the habit of giving an outline of this story in my lectures. [See Popper: 'Philosophy and Physics', *op. cit.*, and *The Self and Its Brain*, 1977, especially chapters P1, P3, and P5. Ed.]

can only be because they both *resist penetration.* This resistance is *essential* to matter or body; for it enables matter or body to fill space, and thus to be extended in the Cartesian sense.

According to Leibniz, we must explain this resistance as due to forces: the thing has 'a force and an inclination, as it were, to retain its state, and . . . resist the cause of change'.[11] There are forces that resist interpenetration: repelling or repulsive forces. Thus body, or matter, in Leibniz's theory, is *space filled by repulsive forces.*

This is a programme for a theory that would explain both the Cartesian essential property of body—extension—and the Cartesian principle of causation by push.

Since body or matter or physical extension is to be explained as due to forces filling space, Leibniz's theory is a theory of the *structure* of matter, like atomism. But Leibniz rejected atoms (in which he had believed when young). For atoms, at the time, were taken to be very small bodies, very small bits of matter, very small *extensions.* Thus the problem of extension and impenetrability was precisely the same for atoms as for larger bodies. Accordingly, atoms—extended atoms—cannot help to *explain* extension, the most fundamental of all the properties of matter.

In what sense, however, can a part of space be said to be 'filled' by repulsive forces? Leibniz conceives these forces as emanating from, and—*in this sense only*—as located in, unextended points, the '*monads*': they are central forces whose centres are these unextended points. (Being an intensity attached to a point, a force may be compared to, say, the steepness of a curve at a point, that is, to a 'differential': forces cannot be said to be extended any more than differentials, though their intensities may of course be measurable and expressible by numbers; and being unextended intensities, forces cannot be 'material', in the Cartesian sense.) Thus an extended piece of space—a body in the geometrical sense—may be said to be 'filled' by these forces in the same sense in which it may be said to be 'filled' by the geometrical points or 'monads' that fall within it.

For Leibniz, as for Descartes, there can be no void: empty space would be space free of repulsive forces, and since it would not resist occupation, it would at once be occupied by matter. One might

[11]*Philosophische Schriften*, ed. Gerhardt, 2, p. 170.

describe this theory of the diplomat Leibniz as a political theory of matter: bodies, like states, have borders or limits which must be defended by repulsive forces; and a physical vacuum, like a political power vacuum, cannot exist because it would at once be occupied by the surrounding bodies (or states). Thus we might say that there is a general pressure in the world resulting from the action of the repulsive forces, and that, where there is no movement, there must be a dynamic equilibrium due to the equality of the forces present. While Descartes could explain an equilibrium only as mere absence of movement, Leibniz explains equilibrium—and also the absence of movement—as dynamically maintained by equal and opposite forces (whose intensity may be very great).

So much for the doctrine of point-atomism (or of monads) which grew out of Leibniz's criticism of the Cartesian theory of matter. His doctrine is clearly metaphysical; and it gives rise to a metaphysical research programme: that of explaining the (Cartesian) extension of bodies with the help of a theory of forces.

The programme was carried out in detail by Boscovich (who was partly anticipated by Kant[12]). The contributions of Kant and Boscovich will perhaps be better appreciated if I first say a few words about atomism in its relation to Newton's dynamics.

The no-vacuum theory of the Eleatic-Platonic school and of Descartes and Leibniz has one inherent difficulty—the problem of compressibility of bodies, and also that of elasticity. On the other hand, Democritus's theory of 'atoms *and the void*' (this was the motto of atomism) had been designed, very largely, to meet precisely this difficulty. The void between atoms, the porosity of matter, was to explain the possibility not only of movement, but of compression also. But Newton's (and Leibniz's) dynamics created a new grave difficulty for the atomistic theory of elasticity. Atoms were small bits of matter, and if compressibility and elasticity were

[12]Boscovich's *Theoria Philosophiae Naturalis* was first published in 1758 in Vienna; Kant's *Monadologia Physica*, in 1756 in Königsberg. Thirty years later Kant repudiated part of his monadology in his *Metaphysical Foundations of Natural Science*, 1786. Though the essential idea of Boscovich's monadology is to be found in Kant (see Kant, *propos.* iv and v for the *finite* number of discrete monads present in finite bodies, and *propos.* x for the central forces which are attractive over long distances and repulsive over short distances, and for Kant's explanation of extension), Kant's work is extremely sketchy as compared with Boscovich's. [*Cp.* Popper's discussion of Boscovich in *The Self and Its Brain*, 1977, pp. 190–92. Ed.]

to be explained by the movement of atoms in the void, atoms could not, in their turn, be compressible or elastic: they had to be absolutely incompressible, absolutely hard, absolutely inelastic. (This is how Newton conceived them.) On the other hand, there could be no push, no action by contact, between inelastic bodies according to any dynamic theory which—like that of Newton or Leibniz—explained forces as proportional to *accelerations*. For a push given by an absolutely inelastic body to another such body would have to be *instantaneous*, and an instantaneous acceleration would be an *infinite* acceleration, involving infinitely large forces.[13]

Thus only an *elastic* push can be explained by finite forces; and this means that we have to assume that push is always elastic. Yet if we wish to explain elastic push within a theory of inelastic atoms, *we have to give up action by contact altogether.* In its place we have to put *short-distance repulsive forces between atoms*, or, as it might be called, action at short distance, or *action in the vicinity of the agent.* Thus the atoms must repel each other with forces which rapidly increase with decreasing distance (and which would become infinite when the distance became zero).

In this way we are compelled, by the internal logic of the dynamic theory of matter, to admit (central) *repulsive* forces into mechanics. But if we admit these, then one of the two fundamental assumptions of atomism—the assumption that atoms are small extended bodies—becomes redundant; since we have to replace the atoms by Leibnizian centres of repulsive force, we might just as well replace them by Leibnizian *unextended points*, thus identifying the atoms with Leibnizian *monads*. It seems, however, that we must retain the other fundamental assumption of atomism: the void. Since the repulsive forces tend towards infinity if the distance between the atoms or monads tends to zero, it is clear that there have to be *finite distances* between monads: matter consists of a void in which there are discrete centres of force.

The steps described here are taken by Kant and by Boscovich.

[13]The argument is clearly stated by Kant in his *Metaphysical Foundations*, 'General Note to the Mechanic' (the last paragraph of the third chapter); see also his *Monadology*, *propos.* xiii, and his *New Doctrine of Motion and Rest*, 1758 (the section on the Principle of Continuity). Similar arguments may be found in Leibniz who writes (*Mathematische Schriften*, ed. Gerhardt 2, p. 145) 'for it seems that only elasticity makes bodies rebound'.

They may be said to give a synthesis of the ideas of Leibniz, and of those of Democritus and Newton. The theory is, like that envisaged by Leibniz, a theory of the *structure of matter*, and thus an explanatory *theory of matter*. Extended matter is explained by something which is not matter—by unextended entities such as forces and monads, the unextended points from which the forces emanate. The Cartesian *extension* of matter, more especially, is explained by this theory in a highly satisfactory way. Indeed, the theory does more: it is a *dynamic theory of extension* which explains not only equilibrium extension—the extension of a body when all the forces, attractive and repulsive, are in equilibrium—but also extension changing under external pressure, or impact, or push.[14]

There is another development, almost equally important, of the Cartesian theory of matter and of Leibniz's programme of a dynamic explanation of matter. While the Kant-Boscovich theory anticipates, in rough outline, the modern theory of extended matter as composed of elementary particles invested with repulsive and attractive forces, this second development is the direct forerunner of the Faraday-Maxwell theory of fields.

The decisive step in this development is to be found in Kant's *Metaphysical Foundations of Natural Science* in which he repudiates[15] the doctrine of his monadology according to which matter is discontinuous. He now replaces this doctrine by that of the

[14]It is important to realize that Boscovich's forces are not to be identified with Newtonian forces: they are *not* equal to acceleration multiplied by mass, but equal to acceleration multiplied by a pure number (the number of monads). This point has been clarified by L. L. Whyte (in a very interesting note in *Nature* 179, 1957, pp. 284 *ff.*). Whyte stresses the 'kinematic' aspects of Boscovich's theory (as opposed to its 'dynamic' aspects, in the sense of Newton's dynamics). It seems to me that Whyte's comments on Boscovich's critics are correct. (I may perhaps express this by saying that Boscovich gives an explanatory theory not only of extension and gravity but also of Newtonian inertial mass.) On the other hand, although Boscovich's forces are, as Whyte rightly stresses, from a formal or dimensional point of view *accelerations*, they are, from a physical, from an intuitive, and from a metaphysical point of view, *forces*—very much like those of Newton: they are dispositions, existing in their own right: they are the *causes* that *determine* accelerations. (Kant, on the other hand, thinks in purely Newtonian terms, and he attributes inertia to his monads; see his *propos.* xi.)

[15]See the second chapter, theorem 4, especially the first paragraph of Note 1, and Note 2. Kant's repudiation is founded on his 'transcendental idealism': he rejects the monadology as a doctrine of the *spatial structure* of *things in themselves*. (This is for him a 'mixture of spheres'—something like a 'category mistake'.)

dynamic continuity of matter (as an appearance). His argument may be put as follows.

The presence of (extended) matter in a certain region of space is a phenomenon consisting of the presence of repulsive forces in that region—forces able to stop penetration (that is to say, repulsive forces which are at least equal to the attractive forces plus the pressure at that place). Accordingly, it is absurd to assume that matter consists of the monads (from which repulsive forces radiate); for matter would be present at places where these monads are not present, but where the forces emanating from them are strong enough to stop other matter. Moreover, it will for the same reason be present *at any point between* any two monads belonging to (and allegedly constituting) the piece of matter in question.

Whatever the merits of this argument may be,[16] there is great merit in the proposal to try out the idea of a continuous (and elastic) something that consists of forces. For this is simply the idea of a field of forces, in the guise of the idea of continuous matter. It seems to me interesting that this second dynamic explanation of (Cartesian) extended matter and of elasticity was mathematically developed by Poisson and Cauchy, and that the mathematical form of Faraday's idea of a field of forces, due to Maxwell, might be described as a development of Cauchy's form of Kant's continuity theory.

Thus the two theories of Kant and the theory of Boscovich—which were the main attempts to carry out Leibniz's programme of a dynamic theory explaining Cartesian extended matter—became the joint ancestors of all modern theories of the structure of matter (those of Faraday and Maxwell, of Einstein, de Broglie, and Schrödinger), and of the 'dualism of matter and field', which, seen in this light, is perhaps not so deeply rooted as it may appear to those

[16]Like all alleged *proofs* in physics, Kant's proof is invalid, even in the form given here, which attempts to improve a little on Kant's own version. (Kant illicitly identifies 'moving', in the sense of a moving force, and 'movable'; *cf.* the penultimate paragraph of his Note to theorem 4. The ambiguity is bad, but it brings out his intention to identify the presence of a moving force with that of movable matter.)

The logical situation is, in brief, as follows. In this post-critical work, Kant uses his transcendental idealism to remove (by a valid argument, incidentally) his original objections to the doctrine of continuous matter, and he now thinks that he could *prove* continuity—by an invalid argument which, however, is interesting and important because it compelled him to push his dynamism to its very limits (beyond what he anticipated in his definitions).

who, in thinking of matter, cannot get away from a Newtonian or even from a crude Cartesian and non-dynamic model.

Another important influence deriving from the Cartesian tradition—and from the Kantian tradition via Helmholtz—was the idea of explaining atoms as vortices of the ether; an idea that led to Lord Kelvin's and to J. J. Thomson's model of the atom—whose experimental refutation by Rutherford marks the beginning of what may be described as the modern theory of the atomic nucleus.

One of the most interesting aspects of the development which I have sketched is its *purely speculative character*, together with the fact that these metaphysical speculations proved *susceptible to criticism*—that they could be critically discussed. It was a discussion inspired by the wish to understand the world, and by the hope, the conviction, that the human mind can at least make an attempt to understand it.

Positivism, from Berkeley to Mach, has always opposed such speculations; and it is most interesting to see that Mach still upheld the view that there could be no physical theory of matter. (Matter for him was nothing but a metaphysical 'substance' and as such redundant if not meaningless.) He upheld this view at a time when the metaphysical theory of the structure of matter had turned into a testable physical theory. (See also *Realism and the Aim of Science*, Vol. I of this *Postscript*, Part 1, section 17.) It is even more interesting, and somewhat ironical, that these views of Mach reached the peak of their influence at a time when the atomic theory was no longer seriously doubted by anybody, and that Mach's views are still most influential among the leaders of atomic physics, especially Bohr, Heisenberg, and Pauli.

Yet the wonderful theories of these great physicists are the result of attempts to understand the structure of the physical world, and to criticize the outcome of these attempts. Thus their own physical theories may well be contrasted with what these physicists, and other positivists, try to tell us today: that we cannot, in principle, hope ever to understand anything about the structure of matter: that the theory of matter must forever remain the private affair of the expert, the specialist—a mystery shrouded in technicalities, in mathematical techniques, and in 'semantics': that science is nothing but an instrument, void of any philosophical or theoretical interest, and only of 'technological' or 'pragmatic' or 'operational'

significance. I do not believe a word of this post-rationalist doctrine. No doubt we shall have to discard most of our theories many times; but it seems that we have at last found ways to the understanding of the physical world.

21. *Schisms, Programmes, and Metaphysical Dreams.*

The list of metaphysical programmes given in the last section was drawn up with two main purposes in mind. One was to shed some light on the significance of the present crisis in physical theory: the rejection of the Faraday-Einstein-Schrödinger programme has left us without any unifying picture, without a theory of change, without a general cosmology. Instead of a problem situation within a research programme, or relative to a research programme, our fundamental problem situation arises from a schism in physics—from a clash between two research programmes, neither of which seems to be doing its job.

Situations like this have of course arisen before. There was for example the schism between Cartesianism, which permits action only by contact (that is, at vanishing distances), and Newton's theory which permits action at a distance—a schism in which Newton himself took sides against his own theory.[1] But the present situation differs somewhat from all previous ones. Einstein's and Schrödinger's inspiring programme has been attacked by quantum theorists and, according to the judgement of most physicists, has been successfully killed. But those who attacked it have made hardly any attempt to replace it by a similarly powerful programme.

All this, I believe, is due to the prevailing philosophy of science—to the almost universal acceptance of instrumentalism, the theory of theories which Cardinal Bellarmino (one of the inquisitors in the case against Giordano Bruno) and Bishop Berkeley developed in order to oppose Galileo's and Newton's belief that science can search for truth. In the great fight over the fundamental issue of the rationalist tradition—*whether or not the human intellect, unaided by divine revelation, could uncover some of the secrets of our world*—most of the leaders of the quantum theory (except Einstein and Schrödinger) have taken sides with the Cardinal and the Bishop

[1]See my *Conjectures and Refutations*, Ch. 3, especially section iii.

against Galileo, Kepler, and Newton.[2] Although the acceptance of instrumentalism has not by itself led to the destruction of Einstein's and Schrödinger's programme, it has led to almost universal acquiescence in the absence of any alternative programme. Indeed, with Bohr's so-called 'principle of complementarity', instrumentalism proudly announces the 'renunciation' of any such programme. What is left is a tinkering with the formalism—admittedly a legitimate and even a necessary part of the method of trial and error, but only a part of it, and one unlikely to yield important results in the absence of a coherent attempt to understand the world.

David Bohm's heresy does carry on something like a part of the Einstein-Schrödinger programme; I have in mind his attempt to revive de Broglie's pilot-wave theory. But in spite of Bohm's realist and objectivist programme, his theory is unsatisfactory from the point of view presented in this *Postscript*. It is not only bound, like all other deterministic theories, to interpret probabilities subjectively, but it even retains Heisenberg's 'interference of the subject with the object'—although it tries to interpret this interference objectively. As a consequence, Bohm's reply to the Pauli-Einstein criticism (first proposed by Pauli against de Broglie's pilot waves, and later by Einstein against Bohm) seems to me utterly unsatisfactory and unacceptable.[3]

Three fundamental issues have led to the schism:

1. Indeterminism *versus* Determinism.
2. Realism *versus* Instrumentalism.
3. Objectivism *versus* Subjectivism.

The third issue arises, more especially, in connection with Heisenberg's indeterminacy relations and such questions as the reduction of wave packets; and more generally, with respect to the interpretation of probability.

It will hardly be necessary to repeat which sides are taken by the different parties over these three issues.

Einstein, de Broglie, Schrödinger and Bohm are determinists and realists; they are objectivists with respect to the aims of physical

[2]I have described this story in the chapter just quoted. [See also *Realism and the Aim of Science*, Vol. I of the *Postscript*, Part 2, sections 10–14; and section 2 of the present volume. Ed.]

[3]*Scientific Papers Presented to Max Born*, 1953; *cp. L.Sc.D.*, Appendix *xi.

theory, but subjectivists (more or less consistently so) with respect to the interpretation of probability theory.

The orthodox Copenhagen school represented by Bohr and Heisenberg and supported by Pauli, and perhaps to a lesser degree by Born, is indeterminist and instrumentalist, although all its representatives mentioned have a number of anti-instrumentalist remarks to their credit. What is most characteristic of the attitude of this school—one of its *eigenstates* as it were—is an oscillation between the objective and the subjective approach in which, owing to a kind of resonance, all its members share.

My own view is that *indeterminism is compatible with realism*, and that the realization of this fact makes it possible to adopt a consistently objectivist epistemology, an objectivist interpretation of the whole of quantum theory, and an objectivist interpretation of probability.

Although I dislike the subjectivist strain in the orthodox interpretation, I am in sympathy with its rejection of the determinism of Einstein, Schrödinger and Bohm, and with its rejection of *prima facie* deterministic theories in physics; and I agree with the substance (though hardly with the form, or with the prophetic style—the style of historical determinism) of a passage of Pauli's, taken from a letter to Born, in which Pauli rejects the deterministic research programme in the following words: 'Against all retrograde efforts (Schrödinger, Bohm, et al., and, in a certain sense, also Einstein) I am certain that the statistical character of the ψ-function, and thus of the laws of nature—which you have, right from the beginning, strongly stressed in opposition to Schrödinger—will determine the style of the laws for at least some centuries. It is possible that later . . . something entirely new may be found, but to dream of a way back, back to the classical style of Newton-Maxwell (and it is nothing but dreams which those gentlemen indulge in), that seems to me hopeless, off the way, bad taste. And we could add, "it is not even a lovely dream".'[4]

[4]Quoted from Max Born, 'The Interpretation of Quantum Mechanics', *British Journal for the Philosophy of Science* 4, 1953, p. 106. The words replaced here by dots (because they do not seem to be relevant to my discussion at this stage, any more than the historicist and determinist prophecy contained in the words 'for some centuries') are as follows: 'for example in connection with the processes of life'. (*Cp.* section 28, below.) In the *British Journal*, there is an obvious misprint ('Bohr' instead of 'Bohm').

I propose to disregard (or to shelve, 'for some centuries') Pauli's somewhat ungentle and in my opinion 'retrograde' or, more precisely, nineteenth-century historicist[5] manner of expressing his evaluations. For there seems to me much to admire in this passage. I greatly sympathize with his rejection of the Faraday-Einstein-Schrödinger programme (which he somewhat insensitively calls 'the classical style of Newton-Maxwell') on the grounds of its adherence to *prima facie* determinism, and I sympathize even more with his plea for the admission of probabilistic theories and interpretations (even though he describes them as 'statistical'). And I have no objection whatever to his description of metaphysical research programmes as 'dreams', in the sense of wish-dreams; for they are attempts to formulate our hopes, our anticipations, and our ambitions, concerning the growth of our knowledge. Yet the manner in which Pauli speaks here of these dreams betrays an ambivalent attitude towards them. There seems to be a slight but interesting change of attitude between the first two occurrences of the word 'dream' in this passage and the last. If I am not mistaken, these first two occurrences indicate something like that anti-metaphysical and anti-realistic (and thus instrumentalistic) attitude which I have described by the phrase 'tough-and-no-nonsense'. But there is a subtle change when Pauli adds, 'it is not even a lovely dream'. This, it seems, expresses two different feelings. It not only expresses the feeling that Parmenides's metaphysics—the dream of the block universe—is no longer a research programme that can attract and inspire, but also, if I am not mistaken, the longing for something better, the wish to possess a metaphysical picture of the world that is attractive and inspiring.

With such an attitude I entirely agree. Attractive and inspiring as the Einstein-de Broglie-Schrödinger programme was in its grand intuitive conception—the explanation of matter, and all its interactions, in terms of fields, and of disturbances in fields—there is something wrong and inadequate about metaphysical determinism with its block universe. And though it was a lovely dream—and one which once greatly attracted Pauli—it seems almost unavoidable

[5]See my *Poverty of Historicism*, and my *Open Society;* see also *The Open Universe: An Argument for Indeterminism*, Vol. II of the *Postscript*, sections 20–24.

that those who have breathed the freer air of indeterminism should no longer be satisfied by it: Pauli may now well say of it that it is no longer a lovely dream, or a hopeful one.

As to Pauli's dreams of the future of science, we can only guess what they are like. He has expressed his concern about contemporary science and its failure to present a comprehensive and unifying picture of the world.[6] From this remark and from his belief in indeterminism, and in probabilistic laws of nature, we may perhaps guess the outlines of a picture which might be acceptable to him.

In this metaphysical epilogue it is, I frankly admit, 'nothing but dreams' I wish to 'indulge in'. Although the dreams to be described are my own, I hope that in their general tendency they do not differ too much from Pauli's. I suppose that they would differ very little if only Pauli could forget, at least in his dreams, the instrumentalist and subjectivist element of the orthodox creed; that is, if he could forget 'complementarity' or, what amounts to the same, if he would remember that *it is likely that the world would be just as indeterministic as it is even if there were no 'observing subjects' to experiment with it, and to interfere with it.* What I take to be Pauli's main points—indeterminism and the probabilistic character of the laws of nature—will be found to be fully represented in my metaphysical dream. At the same time, and without becoming incoherent, my dream also embraces Faraday's, Einstein's and Schrödinger's programme of a physical reality that is determined by *prima facie* deterministic laws; a reality which I here take to consist of propensities. This makes it possible to join these two views, indeterminism and determinism, in a most natural manner by a correspondence argument (whereby deterministic theories are shown to be approximations of indeterministic theories); and it suggests, at the same time, a theory of matter which explains particles in terms of field concepts.

I will now try to give as rational an account of my metaphysical programme as I can, in the space which remains at my disposal.[7]

[6]*Cp.* W. Pauli, *Studien aus dem Jung-Institut* 4, 1952, pp. 109–194, especially the end.

[7]I originally wrote this, in 1954 or 1955, intending that it should become one of the 'New Appendices' to *L.Sc.D.*, and very little room was left.

22. *Classical Determinism Modified by a Correspondence Argument.*

Let us begin by visualizing our changing world, as it changes its state from instant to instant. The states that belong to consecutive instants are, in some way or other, closely connected. This is why our world exhibits some degree of order rather than complete disorder; why it is a cosmos rather than a chaos. But we will visualize the connection between the instantaneous states not as a deterministic connection, but as something in between a deterministic block universe and chaos.

In order to get this point a little clearer, let us imagine that we have attached a film strip to a certain given instantaneous state of the world, that is, to some given 'time-slice' of the world, and that we use this film strip to represent all the past and future time-slices of the world, as well as we can represent them. (*Cp. The Open Universe: An Argument for Indeterminism,* Vol. II of the *Postscript,* section 26.)

We will assume, as a first step or first approximation, that the film strip which we have attached to the given time-slice is a Laplacean or deterministic film strip, or in other words, that it represents a deterministic block universe—the one which is determined by the given instantaneous state or time-slice; for we know that one instantaneous state or time-slice is sufficient, according to Laplace, to determine all the past and future states or time-slices of a deterministic universe. Each of the instantaneous states or time-slices of the deterministic universe we assume to be represented by one of the stills of which the film strip is composed.

Now let us further assume that we have attached a similar Laplacean or deterministic film strip to each of a considerable number of consecutive time-slices of our real, non-determinist universe.

To say that our own universe—the real universe—is non-determinist is to imply that the Laplacean film strips which we have attached to the various time-slices do *not exactly* represent our own universe; for if they did, then our real world would be Laplacean and deterministic, contrary to our assumption. No doubt the first few stills that come before and after the real time-slice to which the film is attached will be very similar to the real states or time-slices

they are supposed to represent: this we know from the success of classical physics. But minor differences will accumulate if we move further and further away from the time-slice of the real world to which the film is attached; and if we move far enough in this way, then the stills will become useless for the purposes of prediction.

Were our real world deterministic, then each film strip would represent it completely. Moreover, all the film strips would be exactly alike (since each would represent the same course of the world). But since we assume that our world is not deterministic, all, or almost all, of the imaginary film strips—the film strips which we attached in our imagination to the various time-slices of the real world—will differ from one another. Admittedly, any two of the imaginary films which are attached to neighbouring time-slices will be very similar; but they will not, as a rule, be exact replicas of each other—unless, indeed, the second of the two time-slices were *exactly* as predicted by a determinist physicist, on the basis of the first.

So far I have operated with two assumptions, and I have illustrated them with the help of our imaginary film strips (which I shall soon need for carrying the argument further): the assumption that our world is indeterministic, and the assumption that there exists a deterministic physical theory (for example 'classical physics') which is successful in the sense that its predictions are good approximations to the truth.

Now we turn for a moment from the classical extreme to the other extreme—to the assumption that the world is completely chaotic; and we try to attach another sequence of imaginary film strips to the sequence of the real time-slices, representing this assumption. How could we do it? The assumption that the world is chaotic clearly does not allow us to make any prediction. Consequently the film strips will be void of any definite information. They will leave open *all possibilities,* they will permit any state to be followed by any other (logically) possible state. Assuming that we know somehow what would be a (logically) 'possible state' of the world, each of our film strips would have to consist of stills, incorporating a catalogue of all possible states of the world, and attributing equal weight (or probability) to each possibility. Consequently all the film strips attached to the various time-slices would be exactly alike (as they would be in a determinist world); moreover, all the stills of each of the films would be exactly alike, as they would be if we were living in a world without any change in time: for there is only *one* complete catalogue of possibilities. (This catalogue can be presented in various ways, for example by 'possibility spaces'—abstract multi-dimensional spaces such that certain three-dimensional sub-spaces each represent a possible arrangement, or a configuration of particles; or that each point—or vector—represents one of the possible states of the physical system in question which, according to our assumption, is the system of the world.)

Returning to the deterministic or 'classical' film strips we may now, if we like, estimate of each still how similar it is to the time-slice it is supposed to represent. For example, we may measure the deviation of every represented detail from the real state of the world which it is supposed to represent. To this end, we should have first to agree upon some definition of an 'elementary detail' or 'event'. (It must not, roughly speaking, be too small a detail, since the assumption that classical physics is applicable down to *every* structural detail of the world may lead to trouble; consequently our classical film cannot contain a very detailed description. However, it is not necessary for our purpose to say more about this point here.) In addition, we may agree on some yes-or-no criterion by which we decide whether or not an elementary event is properly represented in our still. We can then estimate the average number of the 'yes' answers, or in other words, the chance, or the probability, of obtaining from the still in question a correct elementary prediction. This probability may then be taken as a measure of the similarity between the still and the real state of the world. The probability will be almost 1 for stills close to the time-slice to which the film strip is attached, and it will show a general tendency to decrease if we move further and further away from this instant into the 'past' or the 'future' of the film. The point at which the film becomes completely useless for predictive purposes will be reached as soon as we have to assign to a still the probability ½ or less; for this means that a random answer to any yes-no question about the world will be as good as an answer based upon our classical film. From this still on, we know that, even if the degree of correspondence between a still and a represented time-slice should be greater than ½, this can only

be due to accident; thus from this still on, the classical film will be just as useless for predictive purposes as the catalogue of possibilities which represents the other extreme, the hypothesis of a chaotic world.

Note that we could not, of course, ever *determine in advance* the probability here described of future stills: since we do not know in advance what the real world will be like, we cannot determine the number of events in which a predictive still agrees with the time-slice which it represents. We should therefore have to be content to ascribe something like generally declining probabilities to the stills, from 1 down to ½, according to their temporal distance from the time-slice to which their film is attached. The rate of this decline could be determined only by averaging over past correspondences. (There is no need, however, to go more deeply into this matter, since we shall replace those probabilities which we have assigned to our deterministic film strips by a completely different probability which will be built in, as it were, into a kind of indeterminist and non-classical film strip, to be described in the section after the next.)

I have tried to elucidate some aspects of the relationship between our indeterminist real world and its determinist or classical representation. It turned out to be necessary to operate with a sequence of film strips, or with a *multiplicity* of classical representations, ever to be renewed. This first modification of classical physics was an immediate result of an argument according to which classical ideas cannot be more than approximations to a better theory, and to the truth, since no scientific representation can completely predict an indeterministic world.

In agreement with most other interpretations, I here conceive classical physics as an approximation to quantum physics. (This is the 'correspondence argument'; *cf. Realism and the Aim of Science,* Volume I of the *Postscript,* section 15.) Yet there is a fundamental difference between this way of seeing these things and the orthodox interpretation. We take indeterminism as a cosmological fact which we do not attempt to explain. But the orthodox interpretation tries to explain this fact as due to our own interference with the physical process: as if the world would be deterministic (or more deterministic than it is now), if only there were no interfering men about; as if quanta (like some children) would behave in a more orderly or predictable fashion, if only nobody was looking. This view seems to me absurd; in order to make it more acceptable, the orthodox interpretation is forced into an idealistic or semi-idealistic attitude towards the world—into an attitude which makes it meaningless or semi-meaningless to speak of a reality which is there when nobody is looking. But no such *ad hoc* philosophical assumption is needed. The situation is as simple as it can be. I will try to show that this is so.

Bohr has always contrasted the natural, the intuitive, the immediately understandable character of classical physics with the more difficult, non-intuitive, and highly sophisticated character of quantum physics; and he has suggested that we may be able to make quantum physics more understandable to ourselves if we make use of correspondence arguments, that is to say, if we keep before our minds the transition from the quantum theoretical case to the classical case (which latter we can understand intuitively). Now I have made use here of a correspondence argument for the purpose of sketching the outlines of the indeterminist's picture of the world; yet I wish to draw the reader's attention to the fact that the resulting non-classical picture is more natural, more in keeping with ordinary experience, and less sophisticated, than the classical view of Hobbes, say, or Laplace.

For assume that we know today's time-slice, or part of it, and that we wish to predict certain events a year ahead. It is, I think, just what we should 'naturally' expect to hear if we are told that certain events, such as the flight of a bee, or the movement of a cloud, are not yet predictable—although they may become so, more or less, if we have perfect knowledge of a time-slice *very shortly ahead of them*; if we are told, further, that other events, such as solar or lunar eclipses, can be predicted far in advance; if we are told, in general, that the knowledge of a time-slice six months ahead of an event would help us to foresee some of its details a little better than knowledge of a time-slice a year ahead of that event. And this is precisely my point. What I have attempted to convey with the help of my picture is just this: that indeterminism forces us to adopt the view that there can be no theory which completely predetermines all events ahead; that therefore each time-slice yields its own predictive film which, however, soon loses its usefulness; that later and later time-slices give us a better and better idea of an event ahead of them

all; and that we therefore have to try, if we want a detailed and reliable prediction, to obtain a description of a time-slice (or part of it) of a recent date—as recent as possible.

All this is very simple; and it strikes me, intuitively, as more natural and familiar than the classical Laplacean dream of unlimited predictability. It is, in a way, too simple; and I shall soon (in the section after the next) enrich my picture by replacing my deterministic or classical film strips by others: by film strips that describe propensities. Yet even in the simple version given in the present section, our picture contains the whole story of the 'reduction of the wave packet': the transition from one basis of information to another—a later, and thus a fuller and a better one. (And if we were to believe with Heisenberg that this 'reduction' is the same as 'the element of discontinuity in quantum theory', then we should have to say that our very simple and primitive picture already incorporates quantum discontinuity, that is to say, the idea of a 'quantum jump',[1] which only shows how little this latter-day quantum jump has in common with the original idea.)

Before proceeding to replace our classical film strips by non-classical ones, I will show next how the 'reduction of the wave packet' appears here as a consequence of the hypothesis of an indeterministic world for which classical physics holds only approximately.

23. *Indeterminism and the So-Called 'Reduction of the Wave Packet'.*

The metaphysical view which I am trying to sketch seems to me in many respects similar to the orthodox view. It is for this reason important to appreciate clearly those of its tenets which differentiate it from the orthodox view. I mean its realism and its objectivism. This will be done by a further discussion of the picture which has been introduced in the foregoing section, and which consists of a sequence of time-slices of the real universe, and a sequence of imaginary classical film strips, each of which is attached to, and determined by, one of the time-slices.

The succession of actual time-slices is an objective, real process, possibly a continuous one. The film strips are of course imaginary;

[1] *Cf.* note 6 to section 13, and text.

but they are objective in the sense that each of them is logically entailed, as it were, by a complete description of the time-slice to which it belongs (in conjunction with a complete deterministic or 'classical' system of physics).

The question may be raised whether the change from one imaginary film strip to the next can be represented by a continuous function of the changing time-slices, provided the latter do change continuously. Clearly, our assumptions so far developed here do not suffice to decide this question; but the quantum theory seems to imply that one imaginary film strip will have to be replaced by another whenever some interaction has taken place, such as a collision, or an emission, or an absorption, or any other form of energy exchange. Thus in a world consisting of free particles at great distances apart, *copies of the same classical film strip might perhaps be attached even to time-slices which are removed in time from one another.* (This seems feasible in view of the validity of the classical conservation law of momentum.) But in a dense and complex world system, *new* films would have to be attached after extremely short time intervals, since new and unpredictable states would be realized with every interaction.

Now we have seen that a prediction—a prediction, say, of an event on the first of January of the year 2000—will, in general, be the more definite and the more reliable the nearer to the event in question we can choose the (total or partial) time-slice upon which our prediction is based. So if we are really interested in knowing something about this event, we shall try, from time to time, to bring our information 'up to date'; that is to say, to obtain information about a more recent time-slice, in order to base our prediction upon the latest available initial conditions. We may do this even in a deterministic world, in view of the limited precision of our measurements; but in an indeterministic world we should have to do so, even on the assumption that our information concerning the initial conditions was absolutely exact and complete.

Now every new prediction based upon new and later information of this kind will differ somewhat from the previous prediction. This does not mean that the previous prediction will have been wrongly calculated. It only means (i) that we have to consider the prediction as relative to the time-slice, or information, on which it was based,

and (ii) that it should be clear to us that the use of a later time-slice will in general improve the value of the prediction based upon it.

The logical situation here is precisely the same as that which we encountered in our analysis of the so-called 'reduction of the wave packet'. For let e be the event whose presence or absence we wish to predict, and let $s_1, s_2, \ldots$ be classical film strips attached to later and later time-slices. (All time-slices considered will be assumed to be prior to the event e.) Let

$$pred\,(e, s_1)$$

be a prediction with respect to e in the light of the appropriate still of the strip s_1. We shall then find that $pred\,(e, s_1)$ and $pred\,(e, s_2)$ do not in general agree, and that the latter will generally be preferable as a prediction to the former.

The transition from $pred\,(e, s_1)$ to $pred\,(e, s_2)$ corresponds exactly to the transition from the probability statement $p(e, s_1)$ to $p(e, s_2)$ where '$p(a, b)$' denotes the probability of a given the information b. But a transition from $p(e,s_1)$ to $p(e, s_2)$ is, as we have seen, precisely what quantum theorists have called a 'reduction of the wave packet'. (See section 8 above.) They have suggested that this reduction of the wave packet is connected with, or dependent on *(a)* the measuring *experiment* by which we obtain new information s_2, and *(b)* the realization or actualization of what so far was only potential. (Heisenberg's 'transition from the possible to the actual'; see note 3 to section 9, and sections 10 and 13 above.) These two points, *(a)* and *(b)*, are often combined in the suggestion *(c)* that it is only under the stimulus of our own interference with the physical system, only owing to our measuring experiment, that the transition from the possible to the actual takes place. In our picture, by contrast, the transition from the possible to the actual takes place whenever a new state of the world emerges; whenever a new time-slice is actualized or realized, whether observed, or measured, or not. (In fact, observations and measurements are so extremely rare that almost all 'actualizations of potentialities' happen independently of them.) As long as anything happens, as long as there is any change, it will always consist in the actualization of certain potentialities. Thus a new film strip (and with it, an opportunity for the reduction of the wave packet) appears whenever any interaction takes place. Whether or not we *know* or *observe* the new state s_2, and whether we replace *pred* (e, s_1) by *pred* (e, s_2), in our attempts to predict e, is completely incidental, and does not in any way bring about the actualization of potentialities. *The world changes without reference to us.* We can be quite sure that many interactions have occurred between our choice of strip s_1 and our choice of strip s_2, and that these interactions were not influenced by our choices.

Of course, some changes *are* due to our own experiments; and these are both practically and theoretically important to us. But it looks to me very much like a symptom of either myopia or megalomania to allow one's view of the world, or of science, to be dominated, or even coloured, by the disturbances created by one's own experiments. Transitions from the possible to the actual and quantum interactions were going on before anybody ever interfered with anything, and they will continue to go on long after we have all left off interfering.

24. *Replacing the Classical Films by Propensity Films.*

So far our modification of the classical picture has been somewhat crude: we replaced the one imaginary classical film strip by a sequence of many different film strips, each of them attached to a time-slice of the real world. If we wish to refine this picture, we may consider that so far we have made no use of the other extreme, as I called it—the representation of chaos by a catalogue of all possible states. Let us assume, then, that we possess this catalogue, and a film strip such that every still of it consists of this catalogue. Our problem is to find a sequence of film strips of a new type, somewhere in between these two extreme sequences of film strips, the classical and the chaotic type. The new type should of course give us more information than either the classical or the chaotic type.

The solution of this problem (suggested by quantum theory) is this: the new type of film strip will consist, like the chaotic type, of catalogues of possibilities; but to each of these possibilities will be ascribed a probabilistic measure or *'weight'*. This turns the catalogue of possibilities into a probabilistic distribution—a distribution of properties.

It will be remembered that in one of the two extreme cases discussed in section 22—in the case of complete chaos—we found that all the film strips became identical, each being simply a repetition of the same complete catalogue of all possibilities. But once we ascribe measures or 'weights' to the possibilities, there will be an immense number of different complete catalogues, each with a different distribution of weights over the various possibilities. It is further clear that if we consider one of these weighted catalogues that replaces, say, a classical still not far removed in time from the real time-slice to which the film is attached, then it will have to give most of the weight to those of the possible states which are very similar to the state which the classical theory predicts; consequently, it will give hardly any weight to most of the other states. But this implies that, in any of the films, the distribution of weights in consecutive 'stills' (that is, catalogues) will be closely connected; or that the distribution of weights in one moment will *determine* the distribution of weights in the next moment. As a consequence, *the succession of stills (of weighted catalogues) in each film will be of a deterministic character*—very much like the succession of stills in the deterministic or classical film strips which we considered first (in section 22). The difference will be this: laws of a determinist character (although not necessarily identical with the classical laws) now connect *catalogues of the 'weights' of all possible states of the world*, while before, the classical laws connected a representation of one state of the world with that of another state of the world.[1]

The character of the new laws will be deterministic (and thus 'quasi-classical') simply because the probability distribution of the later still, or catalogue, must depend on, or be a function of, the immediately preceding still (which is also a catalogue). And this means no more than that some of the states are never, or hardly ever, followed by some other states: even though these other states are logically possible, they are excluded by the laws—they are forbidden to succeed the preceding state. Thus the laws that determine the

[1][*(Added 1981) Since each 'still' in the filmstrip attached to a real time slice consists of a whole catalogue of weighted possible states, my proposal really involves that the predictive filmstrips split at any interaction into as many filmstrips as there are possibilities in the catalogue. In this my picture—this section was written in 1954 or 1955—greatly resembles Everett's; only that the many worlds remain mere possibilities instead of becoming real. See my discussion of Everett in the 'Introduction' above, section 5.]

weights, or the distribution of the propensities, by connecting each still (or catalogue) with the next one, will be laws of a deterministic or near-classical character (like the time-dependent Schrödinger equation).

I believe that this new picture, though perhaps a little complex, adequately represents the status of quantum theory, and its relation to classical theory.

Yet what is the advantage of this more complex picture over our first modification? What is its advantage, more especially, over our earlier procedure of attributing probabilities to the classical stills, in order to remind us of their decreasing usefulness? One advantage can be seen at once. Assume that the classical still that represents the world one week ahead has obtained, on the basis of our first procedure, a probability of 0.7 (which means here an *average rate of success* of similar predictions made one week ahead in the past), and consider *(a)* a weather forecast based upon it, and *(b)* an eclipse prediction. Clearly, the weather forecast will be less reliable and the eclipse prediction more reliable than this average rate of success indicates. Our new method can fully differentiate between these cases: a possible event—as opposed to a possible state of the world—will obtain a probability equal to the sum of the probabilities of all the possible states which contain this event. Thus if we succeed with our idea of attributing weights to all possible states, then the probability of the eclipse, or of its non-occurrence, can become much higher, and that of a certain weather situation, or of its non-occurrence, can become much lower, than the probability of 0.7, attributed to the classical still in question. This shows that the new modification may yield better results than the old one.

Now let us consider some of the problems raised by our new picture—the replacement of the classical film strip by a catalogue of weighted possibilities. The main problem is, of course, the determination of these weights or probabilities; or in other words, the discovery of the laws which allow us to connect one still—that is, one weighted catalogue, one probability distribution in possibility space—with the next. This problem sets us the task of determining transition probabilities by a generalization of the classical dynamical laws of nature.

This is the task which quantum theory attempts to carry out.

Quantum theory, as Einstein once said, 'dethroned classical physics as far as application to the case of sufficiently small masses is concerned. . .; so that today the laws of motion formulated by Galileo and Newton can be considered valid only as limiting cases'.[2] So much was clear even before 1925. The problem was, what could be preserved of the old dynamical laws? The theory of Bohr, Kramer, and Slater, of 1924, based on intuitive ideas to which my present picture is in many ways indebted, assumed that even the conservation laws of momentum and energy were only valid for statistical averages. This view was refuted by experiments of Bothe and Geiger; and so the new quantum theory of 1925–26, linked with the names of Heisenberg, Born, Jordan, Dirac, and Schrödinger, was developed, which satisfied the classical conservation laws.

Moreover, Schrödinger's theory was 'classical' in the sense that, *with respect to the propensities,* it was *prima facie* deterministic: it *determined* the distributions of the propensities; and it was time-reversible. From the present point of view this is almost to be expected. For we are faced with the following problem: (i) We have to find laws which play a part analogous to the classical dynamical laws of nature, but which connect distributions of propensities rather than actual classical states. This suggests that the laws should be, if possible, differential equations, determining changes of densities, or of weights, of continuous (multi-dimensional) fields of propensities. (ii) We have to find laws which have a high degree of simplicity or testability; but we know already that laws of the classical *prima facie* deterministic type are simpler, and thus better testable, than other laws. (iii) Our demand for *indeterminism* is satisfied by attaching different film strips to different time-slices, and by replacing the classical films by probabilistic propensity films; thus there is *no need* for the dynamic laws that are to connect the stills to be further removed from the classical type. (iv) This is also suggested by the demand that classical physics should be an approximation of quantum physics (that is, by the principle of correspondence; *cf. Realism and the Aim of Science,* Volume I of the *Postscript,* Part 1, section 15).

But let us return to the conservation laws. Their validity for individual processes suggests that non-interacting particles (and *a*

[2] A. Einstein, *Mein Weltbild,* 1934, p. 173. (*Essays in Science,* p. 8.)

fortiori if these are unobserved particles—but one of my main objects here is to brush aside all problems of observationalism) behave classically: a view held by Einstein. It is the interaction of particles—including photons—that is indeterministic, and especially the interaction between particles and particle structures such as screens, slits, grids, or crystals. These interactions preserve the conservation laws; *but the conservation laws do not suffice for determinism.* This is so because particles may be conceived, not as billiard balls, but as carriers with probabilistic propensities to interact (with various biases). Thus we assume, with Dirac, that a particle approaching a polarizer has a certain propensity to pass it, and a complementary propensity not to pass it. (It is the whole arrangement which determines these propensities, of course.) There is no need to attribute this indeterminism to a lack of definiteness or sharpness of the state of the particle, or to the indeterminacy relations: these arise, rather, as scatter relations, in consequence of the fact that deterministic interaction is replaced by propensities. This view not only replaces the mistaken belief that the indeterminacy is 'due' (or rather partly 'due') to our interference, to our measurements, etc., but it also explains it, up to a point. For every measurement is based upon the interaction of particles; and it will therefore indeed create scatter, in accordance with the distribution of the propensities. But the same happens also in innumerable cases in which there is no observer and no observation.

All this shows that the situation is closely related to the soldier's random walk (discussed above in section 10). In fact we are now in a position to construct something like a highly simplified model for quantum theoretical indeterminism, by the simple device of introducing more than one soldier.

25. *A Rough Model of Quantum Theoretical Indeterminism.*

We take a very large paddock or field, surrounded by a wall, and distribute on it a group of soldiers, consecutively numbered, each equipped with a pocket roulette-wheel with a symmetrical spinning needle whose two ends are not distinguished. Each soldier is instructed to march in a straight line with constant velocity until he comes within five steps' distance of either the wall or another

soldier. If it is the wall, he continues his movement as if he were a billiard ball reflected by it. If it is another soldier, then the one with the higher number consults his roulette-wheel; and after this consultation—without loss of time—he moves off in one of the two opposite directions defined by his roulette-wheel. His direction and speed, and also the direction and speed of the other soldier, are to be so determined that together they satisfy the laws of conservation of momentum and energy; which is always possible.

Our model shows, first, that the conservation laws do not entail determinism, but leave one of the variables open to choice, or to determination by chance. (This is not so, of course, in the case of elastic impact of macroscopic billiard balls.) Secondly, it shows that, beginning with the first encounter of two soldiers when the original predetermination of their movements is destroyed, we have to operate with catalogues of possibilities which may be graphically represented as spreading waves (*cf.* section 10 above).

A third point is this. Let us assume that in order to determine a soldier's position, it is necessary to arrange an encounter with him. Then our model shows that every determination of the position of a soldier will interfere in an unpredictable way with his momentum. Admittedly, this third point is still *far removed from Heisenberg's indeterminacy relations;* but it indicates the way in which the interference of the observer with the object may be visualized as *something of no fundamental importance*—a *consequence* of the prevailing laws of interaction, and therefore in no way capable of explaining these laws, or their indeterminist character.

A fourth point illustrated by our model is this. Let us consider one of two soldiers who are about to meet, and let us try to calculate his propensity to be located, ten minutes after that encounter, at a certain given region of the field. It is clear that this propensity will depend on the *total situation*: all soldiers so positioned that they may possibly encounter our soldier within these ten minutes will influence the result of the calculation. The changing possibilities, or propensities, of their locations will all influence the propensity which we are trying to compute, and will interact with it.

Our example is, of course, much too simple to produce a case analogous to the two-slit experiment. Yet it can be used to illustrate what is meant by saying that every state of the system is a realization of the potentialities determined by its immediately preceding state; and it may also be used to illustrate the idea that to every instantaneous state or time-slice of the system is attached an imaginary film strip whose stills are catalogues (or spaces) of weighted possibilities, and that each of these strips will be supplanted by a different one upon any interaction between the particles (or upon any realization of some of the possibilities). Of course even a supplanted film strip may still be useful for many probabilistic predictions. But if we can obtain later information, then we shall do so, and thus 'reduce our wave packet'.[1]

26. *Matter and Field.*

Einstein's most cherished dream for perhaps forty years was to construct a unified field theory—a field theory in which the dualism of matter and field was superseded, and in which particles were *explained* as resulting from the properties of the field. The object of a theory of this kind is, of course, the deduction of the *physical properties* of the particles—their stability or instability, their laws of motion, their mutual interaction, and their interaction with the rest of the field—from the assumed equations of the field. Einstein's

[1]It seems to me worth noting in this context that *Descartes's theory of free will* was based on his belief that although the 'quantity of motion' was preserved (no doubt an intuitive anticipation of the energy principle), its direction was not: this is why the discovery of the law of conservation of momentum was generally assumed to refute his view that the non-material mind may have power over the direction of motion (although not over its quantity). But if we bring in (at least) *two* particles, then the laws of conservation of momentum and of energy certainly do not fully determine their direction, although they do determine, after the direction of one of them has been chosen, the speed of both particles, and the direction of the second particle. Now what a moving animal does is precisely this: it chooses its direction. It can do this, in most cases. Its choice of speed, however, is usually limited: that is, by the available muscular energy, by the maximum acceleration it can develop indirectly, and thus, by the energy principle. (If we take the animal as one of these two particles, then the second particle whose presence makes it possible to satisfy the law of conservation of momentum is, as a rule, the earth.) I do not think that any physical law is violated here; nor that physical laws suffice to determine all animal movements—such as those of my hand guiding my pen. In other words it seems to me that Descartes's idea was fundamentally correct, although his physics, of course, needs amending. [*(Added in proofs.) I now find that Schrödinger said long ago (in 1922): '... the energy-momentum theorem provides us with only four equations, thus leaving the elementary processes to a great extent undetermined ...'; *cf. Science Theory and Man*, 1957, p. 143]

original programme does not seem to be regarded as very hopeful any longer by many physicists. This, I believe, is partly a consequence of Einstein's long adherence to determinism, and partly due to the fact that his idea of a field—which stems from Faraday—is different from the idea to which the quantum theory has given rise—the idea of fields that determine probabilities (to be interpreted, in my view, as propensities).

A Faradayan or Einsteinian field may be conceived as being just as 'real'—or almost as 'real'—as an electron, or a Johnsonian stone: such a field 'can be kicked, and it kicks back' (to quote Landé). Indeed, the present physical explanation of kicking and of being kicked back is (in first approximation), quite generally, that of two Coulomb fields kicking each other. Fields of probabilities or propensities are more abstract things. They can be created (by an experimental set-up) but they cannot be kicked, and they do not kick back.[1] However, an experimental kick may correspond to a set-up whose field determines a very high probability for a kick-back. Since it can do this, and since we have been led to assume that possibilities—virtual events—can influence one another and thus the final results, I propose to consider fields of possibilities or probabilities as 'real'—as manifolds of 'real', objective, though virtual, propensities. But since they cannot be kicked, and do not kick back, they will have to be conceived, somehow or other, as less 'real' and more 'abstract' than particles of Faradayan or Einsteinian fields.

Another aspect of more recent developments seems to point in a direction very different from that towards which Einstein aimed with his unified field theory. Einstein tried to construct a theory in which the two kinds of field considered at that time in physics—Faraday's and Maxwell's electromagnetic field and Einstein's own gravitational field—were replaced by one single field. This particular programme of unification was closely connected with what may be called the electrical theory of matter—the belief, in brief, that matter was a form of electrical energy.

In its simplest form, this theory tried to explain the mass of electrons and protons as due to the energy content of the electrosta-

[1][This view stems probably from 1954. It was later revised by Popper. See his Introduction to this volume, thesis 8. Ed.]

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tic field surrounding them; and in this form, at any rate, the theory had to be abandoned as refuted upon the first discovery of a neutral material particle, the neutron. (It has been refuted again every time a new neutral elementary particle has been discovered. Of course these discoveries constitute refutations only if we assume that these neutral particles are 'elementary', that is, non-composite.)

With the abandonment of the electrical theory of matter, the particular programme for a unified field theory which had been envisaged by Einstein lost its plausibility. Indeed, with any new kind of particle (whether neutral or charged) the present quantum theory associates a new kind of quantized field: instead of a merger of two fields into one, we now have as many different kinds of fields as there are different kinds of particles. They numbered at least sixteen in 1957 (if we always associate one kind of field with two particles—that is a particle and its anti-particle).

In any case, the fundamental idea of a unified field theory seems to me one that cannot be given up—unless, indeed, some alternative unified theory should be proposed and should lead to success. For the present state—that of a multitude of field theories—seems unsatisfactory in several respects. Admittedly, it has led to most satisfactory quantitative predictions in electrodynamics—the probabilistic theory of the interaction of an electron-positron field (an 'electron-positron assembly') with a photon field (a 'photon assembly'). But outside electrodynamics the predictions derived from the present theory are mainly qualitative; which means that the theory cannot be satisfactorily tested.

Moreover, the situation is unsatisfactory even within electrodynamics, in spite of its predictive successes. For the theory, as it stands, is not a deductive system. It is, rather, something between a deductive system and a collection of calculating procedures of somewhat *ad hoc* character. I have in mind, especially, the so-called '*method of renormalization*': at present, it involves the replacement of an expression of the form 'lim *log x-lim log y*' by the expression 'lim (*log x - log y*)'; a replacement for which no better justification is offered than that the former expression turns out to be equal to $\infty - \infty$ and therefore to be indeterminate, while the latter expression leads to excellent results (especially in the calculation of the so-called Lamb-Retherford shift). It should be possible, I think, either to find

a theoretical justification for the replacement or else to replace the physical idea of renormalization by another physical idea—one that allows us to avoid these indeterminate expressions.

But more important than shortcomings of this sort seem to me the present methods of operating with thirty or thirty-two different kinds of fundamental particles (counting the anti-particles) with which are associated fifteen or sixteen different kinds of fields.

About the unsatisfactory character of this situation there is, I think, unanimity; also about the desirability of explaining, if possible, the thirty odd particles as states of a quantized field, and their decay properties by transition probabilities (which has been partly done). It is an aim like this that makes a unitary field theory urgent. The aim is determined by a research programme which is often implicitly accepted, although it is hardly articulated.

Of course, the usual way of looking at fields and particles is essentially dualistic; and it is widely held that this dualism is an essential feature of atomic theory. This is not surprising when we consider that the idea of 'associating' a field with a particle has proved most fruitful, from Einstein's photon theory to de Broglie's and Schrödinger's theory of the electron and to Yukawa's theory of the meson. And yet, this dualism does not seem well founded as yet, however helpful it may have proved in handling physical problems. Born's statistical interpretation, for example, is an essentially monistic particle theory (*cf.* section 11 above, text to footnote 6). As Landé points out: what exists are particles; the waves merely determine the frequency with which particles assume a certain state, upon repetition of the experiment. Similarly, the more recent theories—involving a multiplicity of quantized fields associated with the various kinds of particles (mentioned earlier in this section)—are also, it must be stressed, essentially particle theories. For, these field theories are statistical theories of *particle assemblies*: what they describe are the numbers—or more precisely, the probabilities of a change in the numbers—of the particles in the various possible states; they describe the probability of 'creation' or 'destruction' of particles in these states ('creation and destruction operators'), or, with a different choice of metaphor, the probability of the transition of particles of one state to another.

Thus whether we look at the quantum theory of Heisenberg, Schrödinger, and Dirac, in Born's interpretation, or at the more recent theories of quantized fields, there really seems no basis for the assertion that quantum theory incorporates a dualism of particles and waves: in all these theories, the 'waves' play the part merely of determining the probabilities for particles to take up certain states, or to undergo transitions from certain states to certain other states.

Where however a genuine field theory is likely to come in (and a unified field theory), as opposed to a particle theory, or a theory of particle assemblies, is in the explanation of the particles themselves. This was first foreshadowed by Schrödinger in his original attempt to explain particles as wave crests (or wave packets); and though he has given up this theory he has often returned to the problem.

A little consideration of what a monistic field theory—or any other theory—may be able to do could help us here. As indicated earlier, it could explain, at best, only *the physical properties* of particles; or in other words, their *physical behaviour*; or still more precisely, their *disposition or propensity* to behave, under certain circumstances, in certain ways. No physical theory can do more than this: it can describe a physical system only by describing its propensities. Thus particles are propensities, from the point of view of physical theory; and it is only *a certain metaphysical view* which sees them differently—perhaps as regions of space which are, temporarily, packed 'full' of something, and indivisible.

I suggest that we give up this metaphysical view, and that we replace it by another, equally metaphysical one: by the view that propensities are real; that they are described by field equations; that particles can be produced by propensities; and that, at least up to a point, particles *are* propensities; so that they are, in this respect at least, what the physical theory tells us—what it can possibly tell us. We can in this way supersede the dualism of matter and field without sacrificing any advantages it may have for the treatment and solution of physical problems. For although we replace this dualism by a monism of propensities, we retain, within this monistic view, a kind of practical dualism. For propensities are, on the one hand, *potentialities*; and on the other hand, they are propensities or potentialities to *realize* something. But whatever may be realized, or may realize itself, must again be a set of propensities or potentialities to realize something else.

A physical theory of matter which treats particles in the way indicated here is not only a programme, or a wish dream, for it was achieved many years ago, as far as positive electrons are concerned. I have in mind, of course, Dirac's famous theory—one of the boldest and most ingenious parts of quantum theory. It is a theory which interprets the positive electrons as 'holes'—that is to say as unoccupied states, as open possibilities for occupation. The vacuum (the empty space) is conceived by this theory as possessing a structure: it consists of negative electrons which 'occupy' all or almost all the possible states of negative mass and energy. (The existence of such states is a consequence of Dirac's equations.) Having assumed a state of this kind, a negative electron disappears, as it were; but it remains virtually present, in so far as it may re-appear under certain circumstances if a light quantum supplies the necessary energy to lift the electron to a higher energy level. But simultaneously, a 'hole' will appear—its previous state which is now unoccupied; and the theory predicts that this 'hole' will behave exactly like an electron with a positive electric charge: it will be repelled by positive charges, attracted by negative charges, and will 'unite' with a negative electron, whereupon both the 'hole' and the negative electron will disappear. Their charges as well as their masses will be annihilated and converted into radiant energy (into a photon). The two inverse processes—the creation of a pair of positive and negative electrons out of the vacuum, and the annihilation of a positive and a negative electron—are thus explained. The explanation is monistic. There is only *one* kind of particle involved, the one which we know as the negative electron; and the two processes consist in its assumption of different states, of different energy levels. The other particle—the one we know as the positively charged electron—turns out to be a 'hole' in the vacuum: an open possibility, an unoccupied state which may be taken up by a negative electron: a *propensity*.

The particular kind of monism inherent in this approach—a monism of negative electrons—is hardly any longer attractive today. Too many different particles (*e.g.*, mesons) have been discovered, since the discovery of the positive electron, which cannot be explained in the way described. The time when there was a hope that it would be possible to explain matter in terms of the states of one or two fundamental particles has passed, it seems.

What is important here is that the existence of Dirac's formalism proves the existence of a mathematical theory which allows us to describe the presence of a material particle as equivalent to a 'hole', that is to say, to a non-particle, to a potential but 'unoccupied' state of a particle: to a sheer open possibility for interaction with other particles.

There does not seem to be any reason why these other particles should not in their turn be mathematically equivalent to open possibilities. If so, we should arrive at another kind of monism—one which interprets material particles as both the *realizations or actualizations* of certain potentialities (pair-creation realizes certain potentialities of the vacuum) *and also as potentialities for certain kinds of interaction* (such as pair annihilation, for example). A formalism which would treat 'particles' and 'holes' throughout as equivalent would, up to a point, fulfil Einstein's and Schrödinger's programme: it would explain particles of matter in terms of fields. At the same time, it would fulfil a programme which has been sketched here: a programme for a theory of change which would be monistic and dualistic at the same time; which would allow us to interpret any real state of the world as both an actualization or realization of some of the potentialities or propensities of its preceding states, and also as a field of dispositions or propensities to realize the next state. In this way, the apparent dualism of matter and field, and of particle and wave, would be shown to spring in the most natural way from the two fundamental aspects of every physical thing. I mean its two aspects as a *bearer of dispositions*—a *bearer* that can have no further testable properties beyond these *dispositions*.

A theory like this might also yield the principle that particles representing the same possibilities are identical (and with it Bose's principle).

27. *Open Problems.*

My dream programme is metaphysical. It is non-testable: it is irrefutable (and irrefutability, we should remember, is not a virtue but a vice). It is based upon the metaphysical (rather than the

'scientific') idea of indeterminism.[1] It tries to supplant, to supersede, the existing metaphysical interpretation of quantum theory in terms of 'particles', defended by Born against Schrödinger; it also tries to supplant the instrumentalist interpretation of Bohr (who renounces any attempt to go beyond the particle-wave dualism and explain it). And it tries to give a coherent view of the physical world—a physical world which is no longer a strait-jacket for its physical inhabitants, not a cage in which we are caught, but a habitat which we may make more habitable, for ourselves and for others (and which incidentally, we are about to make uninhabitable for our children by what we proudly call 'the peaceful use of atomic energy'[2]).

But if my dream is metaphysical, what is the use of it? Is there anything in it beyond, perhaps, an emotional satisfaction? Is it not utterly different from a scientific hypothesis—one in which we are mainly interested *because of its implicit claim to be considered, tentatively, as true?*

I no longer think, as I once did, that there is a difference between science and metaphysics regarding this most important point. I look upon a metaphysical theory as similar to a scientific one. It is vaguer, no doubt, and inferior in many other respects; and its irrefutability, or lack of testability, is its greatest vice. But, *as long as a metaphysical theory can be rationally criticized,* I should be inclined to take seriously its implicit claim to be considered, tentatively, as true. And I should be inclined to evaluate it, in the main, by an appraisal of this claim—considering its theoretical interest first, and taking only a secondary interest in its practical usefulness (as distinct from

[1][See *The Open Universe: An Argument for Indeterminism,* Vol. II of the *Postscript,* for the distinction between scientific and metaphysical indeterminism. Ed.]

[2]Although the topic is a digression, we might reflect here on the fact that, some forty thousand years or more after Prometheus, we have not learned to master the dangers of 'the peaceful use of fire'. People are still killed by it, peacefully and regularly, although there is no inherent difficulty in controlling it, and although a burnt child dreads it (unlike mislaid radio-active phials and other 'atomic wastage'). The building of atom bombs may be, in an unfortunate political situation, an unfortunate necessity; but can the same be said of most of the forms of 'peaceful use' of atomic energy? And is our haste to develop this 'peaceful use' free of the historicist idea that we are moving into a new age—the 'atomic age'—and of similar nonsense?

its fruitfulness as a research programme). Practical usefulness or uselessness may be considered as important mainly because it is something like a test of truth—as it may often be in connection with a scientific theory.

But is it possible rationally to appraise or evaluate an irrefutable theory? What is the point of criticizing a theory rationally if we know from the start that it is neither refutable by pure reason, nor testable by experience?

My answer is this. If a metaphysical theory is a more or less isolated assertion, no more than the product of an intuition or an insight flung at us with an implied 'take it or leave it', then it may well be impossible to discuss it rationally. But the same would be true of a 'scientific' theory. Should anybody present us with the equations of classical mechanics without first explaining to us *what the problems are* which they are meant to solve, then we should not be able to discuss them rationally—no more than *The Book of Revelation.* Even if we are presented with Newton's arguments, we may be unable to discuss them unless we hear first about the problems of Galileo and Kepler and their solutions, and about Newton's own problem of how to unify these solutions by deriving them from a more general theory. In other words, any rational theory, no matter whether scientific or metaphysical, is rational only because it ties up with something else—because it is an attempt to solve certain problems; and it can be rationally discussed only *in relation to the problem situation* with which it is tied up. Any critical discussion of it will consist, in the main, in considering how well it solves its problems; how much better it does so than various competing theories; whether it does not create greater difficulties than those which it set out to dispel; whether the solution is simple; how fruitful it is in suggesting new problems and new solutions; and whether we cannot, perhaps, refute it by empirical tests.

This last method of discussing a theory is not, of course, applicable if the theory is metaphysical. But the other methods may well be applicable. This is why rational or critical discussion of some metaphysical theories is possible. (There may, of course, be other metaphysical theories which cannot be rationally discussed.)

It is hardly necessary to give examples of this method here, for this *Postscript* is full of them. I may mention as examples my critical

discussion of idealistic and positivistic epistemologies (in Volume I, Part 1); of probabilistic epistemologies (in Volume I, Part 2); and of deterministic metaphysics—as may be found in Hobbes, Hume, Kant, and Einstein (in Volume II).

I believe that my dream can be discussed in this way—especially by comparing it with competing views which it is meant to supplant. The comparison should be in terms of simplicity, coherence with certain other theories, unifying power, intuitive appeal and, above all, fruitfulness. Without wishing to commit myself to anything like pragmatism or instrumentalism, I think I should take the question of the fruitfulness of my programme as decisive. If it does not lead to new problems or, at least, to a new evaluation of some of the great old open problems, I should discard it: as a lovely dream (or so it seems to me)—lovely, yet not to be indulged in.

I do not wish here to give a list of the well-known open problems of the theory of matter (such as the problem of the electronic charge, or the derivation of the exclusion principle) and of general cosmology which (I dream) may perhaps be attacked one day in the light of the metaphysics of change which I have proposed here. But the problem of the large and still increasing number of elementary particles is one which has to be stressed because of its extreme urgency.

It was once the programme of modern atomic theory to explain the large numbers of atoms by *only two fundamental particles*—the electron and the proton; and its moment of greatest triumph occurred when it carried out this programme—a programme which now lies in ruins. The trouble started long ago, with the *ad hoc* hypothesis of the neutrino. It was introduced *ad hoc* in order to avoid defeat—a sound procedure in the case of a highly successful theory, as long as there is any hope of obtaining one day independent evidence for the *ad hoc* assumption. But if I am not mistaken, this hypothesis is still *ad hoc,* just as it was thirty years ago.[3] The neutrino, however, is small fry compared with all those other particles which have had to be introduced since, on compelling evidence—especially the various mesons; for they shattered the

[3] [*(Added 1981) It was so when this book was being written.]

views of the structure of matter which had been the basis of quantum theory. Admittedly, these views were not part of the mathematical formalism; but they were part of physical theory none the less.

What is needed is a general theory of matter which explains the masses of all these particles, and their stability or instability, from general principles.[4] Einstein's demand for a unified theory is a necessary one, one which cannot be waved aside as a mere will o' the wisp, or as an illusion destroyed by quantum theory.[5] The theory of changing *fields of propensities* may offer a way towards unification.

Midway between atomic theory and cosmology proper there is the theory of the creation of matter due to Jordan, Gold, Hoyle, Bondi, and McCrea, which in intention at least is a scientific conjecture rather than a metaphysical theory (though it does not seem testable as yet). [*Not only has it become testable since, but it appears to have failed the test.] But assume a propensity theory of matter, as sketched in the foregoing section, and interpret space ('position' space, *topos*), following the suggestions of Leibniz and Einstein, as the field 'of the possible mutual relations of . . . bodies'[6]; that is to say, of the propensities of material particles to be positioned between other particles. (It is also, according to Dirac's theory, a field of states occupied by latent pairs of particles of matter, and capable of being polarized.) The expansion of the universe might then be interpreted as the expansion of this field of propensities. As this expansion creates new possibilities, and thus new propensities for matter to be present (some of which will be realized), it might thus explain the creation of new matter; for matter may be identified with the realization of these propensities. Thus an expanding universe might create matter merely as a function of its expansion (as perhaps suggested by the steady state theory).

In this connection, I want to draw attention to Kapp's conjecture that all material particles have a limited life-time, so that large accumulations of particles (stars, planets) would act like a 'sink' for matter (and thus, on the assumption just sketched, also for space); a

[4]A field theory which attempts to explain the masses of the particles (but not, to my knowledge, their lifetimes) has been proposed by M. Born and H. S. Green; *cp.* M. Born, *Rev. Mod. Phys.* 21, 1949, pp. 463–473.

[5]This was W. Heisenberg's thesis in *Dialectica* 2, 1948.

[6]A. Einstein, *Mein Weltbild,* p. 179; *cp. Essays in Science,* p. 14.

conjecture which, as Kapp suggests, might lead to a geometrical theory of gravitation similar to Einstein's.[7]

There are some cosmological problems which are closely connected with the idea of an indeterministic world. This idea implies, for example, the distinction between a *closed past* and an *open future,* and with it an objective direction of time. In our picture of the probabilistic or non-classical film strips each of which is attached to a time-slice of an indeterministic universe, we can say that even though the equations which determine the dynamic changes of propensities in the films may be symmetrical with respect to time, our theory is not. For our theory consists of more than these equations: it consists of these equations plus their interpretation in terms of propensities, and this interpretation is non-symmetrical with respect to time. The idea of propensity distinguishes the unrealized possibilities of the future from their realizations present and past.

This becomes clear in certain aspects of the quantum theory which I have discussed both in *The Logic of Scientific Discovery* and in this *Postscript. (Cp. L.Sc.D.,* section 73, note 5 and text, and section 16 of this volume of the *Postscript.)* As Heisenberg remarks, it is possible to calculate, with the help of certain measurements, for example of two positions of an electron, its spatio-temporal path between these two positions—*its momentum as well as its various positions*—with any desired degree of precision. Heisenberg added that it was 'a matter of personal belief whether such a calculation concerning the past history of the electron can be ascribed any physical reality or not'.[8] My own inquiry led to a different result: in my interpretation, these calculations were *necessary for testing* the Heisenberg formulae, interpreted as scatter relations, and therefore

[7]*Cp.* R. O. Kapp, *British Journal for the Philosophy of Science* 5, 1955, pp. 331 *f.*, and 6, 1955, pp. 177*ff.*, where further references are given. (To these should be added, P. Jordan, *Naturwiss.* 26, 1938, p. 417; *Astr. Nachr.* 276, 1948, p. 193.) I cannot help feeling that it would be preferable not to speak of a 'symmetry' between matter creation and matter extinction; in an expanding universe, at least, it appears that matter creation would have to keep the average density of matter constant, so that matter destruction, if any, would lead to an additional rate of matter creation, rather than to a corresponding or symmetrical rate of matter creation. Moreover the two rates, of creation and of destruction, would depend upon entirely different factors—the first upon changes of density, the second simply upon time.

[8]W. Heisenberg, *The Physical Principles of the Quantum Theory,* 1930, p. 20.

far from opposed to them. But the scatter relations do prevent similar calculations about the future. Thus we all agree that the future is open; and the fact, mentioned by Heisenberg, that we can calculate the past as determined, or closed, shows clearly that even in Heisenberg's interpretation there is an asymmetry here. In the propensity interpretation, the situation is perfectly clear. The future propensities are determined in every instant, *but only the* propensities. (This is why the scatter relations apply.) As these propensities realize themselves, in the form of particles with positions and momenta, the open possibilities become closed. Thus the calculability of the realizations, past and present, differs from that of the future. Although any one *given* time-slice does not allow us to determine the past any better than the future, sequences of *given* time-slices do determine the period between their first and last elements. *But any such sequence can only belong to the past.*

The idea of an indeterministic world bears upon, and offers a solution to, another problem which has been mentioned in this *Postscript*—Gödel's problem of the existence of closed world lines in certain cosmological solutions of Einstein's gravitational equations; that is to say, in rotating universes.[9] The history of a physical body moving on a closed world line would be repetitive, absolutely and infinitely repetitive. But this is possible only in a deterministic universe. The assumption of an indeterministic world therefore excludes this possibility; or rather, it will attribute to it the probability 0, whatever the initial conditions may be.

It is interesting from our point of view that the exclusion of this possibility by indeterminism is certainly not *ad hoc.*[10] But what seems even more interesting is the fact that, assuming Einstein's cosmological equations to be true (an assumption which seems to be compatible with our version of indeterminism), the exclusion of the solutions envisaged by Gödel amounts to the exclusion of certain ways for matter and its motion to be arranged in the world.[11] This

[9]K. Gödel, 'A Remark about the Relationship between Relativity Theory and Idealistic Philosophy', in *Albert Einstein: Philosopher-Scientist*, ed. P. A. Schilpp, pp. 555 *ff.*; and *Reviews of Modern Physics* 21, 1949, pp. 447–450. See also *The Open Universe: An Argument for Indeterminism*, Vol. II of this *Postscript*, note 2 to section 19.

[10]This fact seems to solve Gödel's problem. Gödel's suggestion that the solution lies in the rejection of realism and in the adoption of idealism, does not seem to me to offer any way out of his difficulty.

[11]K. Gödel, *op. cit.*, p. 562.

shows, surprisingly, that *a hypothesis like that of indeterminism can have structural consequences*—that the hypothesis of indeterminism may be able to exclude certain possible arrangements of matter and motion in the world. This fact is extremely interesting in view of what I have called 'Newton's Problem'. (See *Realism and the Aim of Science*, Volume I of the *Postscript*, Part 1, section 16.) It suggests that it may not be utterly impossible to make some headway in connection with certain aspects of this problem, even though it may well be insoluble in the main. But the exclusion, through the attribution of zero probability, of certain possible initial distributions of matter and motion has a further interesting consequence. It shows that in an indeterministic universe, there may be certain probabilistic principles of non-existence that exclude possibilities which from the point of view of a classical deterministic theory would appear typically contingent—possibilities of the character of initial conditions rather than of laws. (*Cp. L.Sc.D.*, Appendix *x.)

28. *Conclusion.*

The metaphysical programme of the propensity interpretation might be summed up, in the concise language of the Ionian cosmologists, by the statement: 'Everything is a propensity.' Or in the terminology of Aristotle we might say: 'To be is both to be the actualization of a prior propensity to become, and to be a propensity to become.' It is a view that unites aspects of all those metaphysical programmes listed in the first section of this epilogue (section 20), as will be seen from the following list.

1. As with Parmenides, the world is full—in the sense that the void, the vacuum, has a structure, and is itself a field of propensities which are real.

2. As with the atomists, the structure of matter is atomic, and the dualism of the full and the empty—or of matter and space or field—is, up to a point, preserved, as a distinction between the realization of a propensity, and the propensity to be realized. (See also position 10, below.)

3. As with Plato and Euclid, the emphasis upon geometry is preserved; and so is the geometrical cosmology: in this respect, non-Euclidean cosmology, as a cosmology, out-Euclids Euclid, for

geometry as such is used even to describe the distribution of matter in the world.

4. The Aristotelian view of inherent potentialities and their actualization is developed into a *relational* theory in which relational structures, instead of inhering in each material thing, may be characterized by potentialities.

5. The (Platonic) geometrical approach of the Renaissance is preserved, as well as Plato's hypothetical method, and its stress upon antecedent causes.

6. The theory of fluids (for example, heat) of the Cartesians and of Boyle is preserved in the form of the law of conservation of energy. Their action at vanishing distances is preserved in the form of the field theory.

7. The propensity theory may be described as a generalization of dynamism.

8. Central forces (which correspond to the Aristotelian inherent potentialities) give place, as with Faraday and Maxwell, to fields of potentialities of a relational character.

9. As in Einstein's and Schrödinger's programme, the dynamical laws of change of these fields of propensities are of a *prima facie* deterministic character (like the laws of a classical theory). Moreover, propensities—even those which for their geometrical representation need a multi-dimensional abstract space of possibilities—are treated as physical realities.[1] These two points, a

[1]Heisenberg (*Niels Bohr and the Development of Physics*, edited by W. Pauli, 1925, p. 24), tells us that probability waves in configuration space are not real, according to the orthodox Copenhagen interpretation which he calls 'the usual interpretation'. Thus the 'dualism of particle and wave' breaks down here: it holds only if the waves are in three-dimensional space. The passage (indeed the whole paper) is interesting, apart from other things, because of its queer suggestions, reminiscent of psycho-analysis, that the critics of Copenhagen are 'uninformed' (*cp. Realism and the Aim of Science*, Vol. I of *The Postscript*, Part I, section 18) about the esoteric teaching of the orthodoxy. 'Now Schrödinger's work', Heisenberg writes, 'first of all, contains some misunderstandings of the usual interpretation. He overlooks the fact that only the waves in configuration space . . . are probability waves in the usual interpretation, while the three-dimensional material waves or radiation waves are not. The latter . . . have just as much (and just as little) "objective reality" as particles; they have no direct connection with probability waves . . . .' I do not know whether Heisenberg intends to allow the Schrödinger waves for one and for two particles to be three-dimensional and thus 'real' (but not those for three particles), or whether he has only the 'second quantization' in mind; but in any case, Heisenberg's view seems to me to take three-dimensionality a shade

physical reality described by deterministic laws, and a reality to which the idea of field applies (so that the laws are partial differential equations), were Einstein's main ideas for a unified theory of the world. They are preserved.

10. The point of view of the orthodox interpretation of quantum theory, including its 'dualism' of field and particle, or of wave and particle, is re-interpreted and preserved in terms of the dualism of potentialities and their actualizations which in their turn are again potentialities. (This view transcends the older dualism, in a way often demanded by Einstein.) The probabilistic character of the theory, stressed especially by Born and Pauli, is also preserved.

Thus all the older programmes become approximations from the point of view of this metaphysical theory of propensities. All the programmes contribute some aspect or other to our metaphysical view.

In particular, the dispute between the last two positions on my list—between Einstein and Schrödinger on one side, and those who, like Pauli, uphold Born's essentially statistical interpretation of quantum theory on the other side—is resolved by the propensity interpretation: to Einstein and Schrödinger it offers a physical reality to which field equations of the classical deterministic type apply, and to Born it offers a probabilistic interpretation of these equations.

The propensity interpretation owes everything to Einstein. And yet I doubt very much whether he would have been prepared to accept it. What would have stood in the way was not only his determinism (*cp. The Open Universe: An Argument for Indeterminism*, Vol. II of the *Postscript*, section 26) but also his attitude towards probability, which was no doubt one of the results of his determinism. This attitude had been fundamentally subjective, and involved the usual assumption that statistical frequencies can be derived from a subjective theory. (There is a close resemblance at

too seriously (if he seriously means what he seems to say in this astonishing paper). After all, the multi-dimensional representations are only ways of putting things; and what they represent—the propensities and their laws—are just as real as what the three-dimensional waves represent. For it is clearly the same thing which is represented by the two methods. There is no difference of subject matter—not even of intended subject matter—but only between two more or less successful methods of representing it.

this point between Einstein's views and those of his opponents who, it seems, never succeeded in shaking off the eggshells of their determinist past.) When I say that the propensity interpretation owes everything to Einstein, I am thinking of his doctrine that the aim of physics is the description of physical reality in terms of field equations. At the same time, he was the first to operate with transition probabilities, that is to say, with *singular probabilities, pertaining to the single atom,* and proportional to the density of radiation.[2] At the end of the paper in which he developed this view, he stressed the task of establishing a link between Maxwell's wave theory and this new theory of his which, he said, 'leaves to chance both the instant and the direction of the elementary processes' of emission. Some time later, he connected ideas of Bose's and de Broglie's by suggesting that, just as it is possible to treat photons statistically like particles of a gas, so it must also be possible to associate with a gas a wave field.[3] In this context it is interesting to learn from a report of Bohr's that in 1920 Einstein described the situation in physics created by his photon theory (and by the refutations of Maxwell's theory implicit in the success of Bohr's model) by speaking of 'phantom fields that guide the photon';[4] the ghosts, or spectres, of the departed electromagnetic waves, were still something real (since they were still responsible for the spectra and especially for the intensity of spectral lines). But although Einstein practically anticipated the propensity theory, I do not believe that he would have liked it. (For other anticipations, see section 12 above.)

One striking advantage of the propensity interpretation is that it seems, if I may use Pauli's words, 'to open up hopes for a future development of different branches of science towards a greater unity'.[5] Pauli is here speaking of quantum theory in general, with its

[2]A. Einstein, *Physikalische Zeitschrift* 18, 1917, pp. 121–128.

[3]*Sitzungsberichte d. Preuss. Akad. d. Wissenschaften, Phys.-Math. Klasse,* 1925, pp. 3–14.

[4]Einstein's expression, '*Gespensterfelder*', is translated 'ghost waves' by Niels Bohr in 'Discussion with Einstein on Epistemological Problems in Atomic Physics', in *Albert Einstein: Philosopher-Scientist, op cit.*, p. 206.

[5]W. Pauli, 'Einstein's Contributions to Quantum Theory', in *Albert Einstein: Philosopher-Scientist*, pp. 157*f.*

probabilistic laws; and no doubt, the different branches of science he has in mind are the biological sciences (see also note 4 to section 21), and ultimately the sciences of man. I think that his hopes are justified, and even more so in view of the propensity interpretation. Biologists have always worked with propensities—even when they were prevented by mechanistic prejudices from admitting it. The propensity interpretation of physics might help to remove these prejudices, and thereby encourage further mutual fertilization between the physical and the biological sciences. Take the case of chemical affinity. We may now say, in the light of its quantum theoretical explanation, that it is a physical propensity, and that the intuitive feeling that it was in many ways akin to certain biological propensities was justified: it is not merely an analogy we face here but, more likely, an intermediate, genuinely transitional case. The term 'propensity' itself is, of course, a biological or psychological term. (It is also used in economics, in precisely the same sense as, I suggest, it should be used in physics.)

A case of special interest is the old problem of the organization of living matter in individuals—individual plants, individual animals. The individual organisms of biology with their strange character of far-reaching autarky have often been compared to crystals; and indeed, they could be compared with physical systems that are endowed with strong propensities to retain their character as relatively autarkic systems—such as crystals, in the case of which these propensities are likely to be explicable in terms of resonance; or atomic nuclei whose astonishing propensities towards autarky are illustrated by the constancy of their rate of radioactivity upon which no environmental conditions short of bombardment by nucleons make any impression. We might call these peculiar propensities towards autarky, with their surprising independence of environmental conditions, '*inherent propensities*' of the system. They are relational, as all propensities are, of course; and yet, they do resemble Aristotle's inherent potentialities of a thing more than other physical or biological propensities. (This is no accident: Aristotle was a biologist.)

Ultimately, only within a framework of an indeterministic picture of the world can we ever hope to understand the phenomenon of voluntary movement in animals. (*Cf.* the footnote at the end of section 25, above.) In biology many things happen, especially in animals, which, considered from a physical point of view, are most unexpected and improbable. That a pair of swallows, after flying long distances, repeatedly return to their old nesting place, is difficult to explain by physical laws, and the conservation laws do not seem to offer any help here. If memory can bring these things about, it must guide, somehow or other, the movements of these animals in a way similar to the way the pocket roulette-wheel guided the movements of our soldiers (*cp.* sections 10 and 25); except that the roulette-wheel is replaced by a kind of loadstone, so that the game of chance is replaced by an 'inherent propensity'. It hardly matters for our present purpose how these things function in detail. What matters is that they do function, and that they seem to function as if certain inherent propensities superimposed themselves (as in the Zeeman effect, say) upon certain other physical propensities of a more chance-like or equi-probabilistic character, thereby giving extra weight to certain possibilities: they impose, as it were, a systematic bias upon them. This seems to be the way in which so many improbable things happen in biological contexts. What we can now clearly see is that this kind of thing (I mean, the superposition of 'inherent propensities') already plays a role, in a rudimentary way, in classical physics (loaded dies, osmotic pressure, resonance); and we can therefore form an intuitive idea of how it may fit into our physical world, and yet transcend it, by superimposing upon it a hierachy of purposes—a hierarchy of systematic and increasingly purposeful biases.[6]

None of this is said in the spirit of a spiritualist's apology: man and his spirit need no apology. It is neither the conservation law of energy and momentum nor any other physical law, and not even a probability or a propensity, which has made him build the pyramids, or climb Everest; and he has reached still greater heights than this in science, in art, and in many other ways.

I have tried to present this metaphysical epilogue as what it is—a picture, a dream, rather than a testable theory. Science needs these pictures. They largely determine its problem situations. A new

[6][Compare Popper's discussions of biology in 'Of Clouds and Clocks', and 'Evolution and the Tree of Knowledge', in *Objective Knowledge*, 1972, pp. 206–84. Ed.]

picture, a new way of looking at things, a new interpretation may change the situation in science completely (as did Einstein's way of looking at the Lorentz transformations). But these pictures are not only much-needed tools of scientific discovery, or guides to it, they also help us to decide whether a scientific hypothesis is to be taken seriously; whether it is a potential discovery, and how its acceptance would affect the problem situation in science, and perhaps even the picture itself.

It is here, perhaps, that we may find a criterion of demarcation *within metaphysics,* between rationally worthless metaphysical systems, and metaphysical systems that are worth discussing, and worth thinking about. The proper aspiration of a metaphysician, I am inclined to say, is to gather all the true aspects of the world (and not merely its scientific aspects) into a unifying picture which may enlighten him and others, and which one day may become part of a still more comprehensive picture, a better picture, a truer picture. The criterion, then, will be fundamentally the same as in the sciences. Whether a picture is worth considering depends, I suggest, upon its capacity to provoke rational criticism, and to inspire attempts to supersede it by something better (rather than upon its capacity to create a fashion, to be supplanted presently by a new fashion, or upon claims to originality or finality). And this criterion, I believe, may also point to one of the characteristic differences between a work of science or of metaphysics and a work of art that aspires to be something that cannot, in its own way, be bettered.