The New Scientific Spirit

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a Bachelardian turn. What "epistemological obstacles" stood in the way (to borrow an expression from a later work with a title similar to that of the present book: *The Formation of the Scientific Spirit*)? But this is not the place to attempt an answer. It is far better to let Bachelard himself speak, to reveal what he saw clearly and what he did not. As a teacher he was beloved by his students, whose preconceptions he considered it his first duty to shake. It would be unfair to prepare you for his thrusts by giving away his strategy.

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Arthur Goldhammer

Introduction

The Essential Complexity of the Philosophy of Science: An Outline

I

Since William James it has often been repeated that every cultivated man necessarily subscribes to some system of metaphysics. To my mind it is more accurate to say that every man who attempts to learn science makes use not of one but of two metaphysical systems. Both are natural and cogent, implicit rather than explicit, and tenacious in their persistence. And one contradicts the other. For convenience let us attach provisional names to the two fundamental philosophical attitudes that coexist so peacefully in the modern scientific mind: rationalism and realism, to use the classical terminology. Is proof required that such tranquil electicism does indeed exist? Consider, then, the following proposition: "Science is a product of the human mind, a product that conforms to both the laws of thought and the outside world. Hence it has two aspects, one subjective, the other objective; and both are equally necessary, for it is as impossible to alter the laws of the mind as it is to change the laws of the Universe."[1] This rather odd metaphysical assertion can be pursued in two possible directions: the first leading to a rationalism at one remove, according to which the laws of the universe would merely reflect the laws of the mind; the second leading to a universal realism, one of whose principles would be that the laws of the mind, being instances of universal laws, must be absolutely invariable.

The philosophy of science has done nothing to purify itself since Bouty enunciated the above proposition. It would not be difficult to show that, in forming scientific judgments, the most determined rationalist daily submits to the instruction of a reality whose ultimate structure eludes him, while the most uncompromising realist does not hesitate to make simplifying assumptions just as if he believed in the principles on which the rationalist position is based. One may as well admit that, as far as the philosophy of science is concerned, there is no such thing as absolute realism or absolute rationalism, and that judgments of scientific thought should not be couched in terms of general philosophical attitudes. Sooner or later scientific thought will become the central subject of philosophical controversy; science will show philosophers how to replace intuitive, immediate systems of met-

[1] Edmond Bouty, *La vérité scientifique* (1908), p. 7.

aphysics with systems whose principles are debatable and subject to experimental validation. What does it mean to say that science can "rectify" metaphysics? As an example of what I have in mind, consider how "realism" changes, losing its naive immediacy, in its encounter with scientific skepticism. Similarly, "rationalism" need not be a closed system; *a priori* assumptions are subject to change (witness the weakening of Euclid's postulates in non-Euclidean geometry, for example). It should therefore be of some interest to take a fresh approach to the philosophy of science, to examine the subject without preconceptions and free of the straitjacket imposed by the traditional vocabulary of philosophy. Science in effect creates philosophy. Philosophy must therefore modify its language if it is to reflect the subtlety and movement of contemporary thought. It must also respect the oddly ambiguous requirement that all scientific ideas be interpreted in both realistic and rationalistic terms. For that reason perhaps we ought to take as our first object of contemplation, our first fact needing explanation, the metaphysical confusion implicit in the double meaning of the phrase *scientific proof,* which can refer either to confirmation by experiment or to demonstration by logic, to palpable reality or to the mind that reasons.

It is fairly easy, moreover, to explain why any scientific philosophy must have such a dualistic base: The very fact that the philosophy of science is a philosophy that *applies* to another discipline means that it cannot preserve the unity and purity of speculative philosophy. Any work of science, no matter what its point of departure, cannot become fully convincing until it crosses the boundary between the theoretical and the experimental: *Experimentation must give way* *to argument, and argument must have recourse to experimentation.* Every application is a form of transcendence. I intend to show that this duality exists in even the simplest scientific investigations, that is, that the phenomenology of science divides, according to one set of epistemological polarities, into two realms, that of the picturesque and that of the comprehensible (which is just another way of saying that science may be viewed in either realistic or rationalistic terms). If we could somehow place ourselves at the frontiers of scientific knowledge and there observe the psychology of the scientific mind, we would find that it has been a concern of contemporary science to overcome the contradictions of metaphysics. Yet the orientation of the epistemological "vector" seems clear. It surely points from the rational to the real and not, as all philosophers from Aristotle to Bacon professed, from the real to the general. To put it another way, the application of scientific thought seems to me to tend essentially toward reality (*nous paraît essentiellement réalisante*). Accordingly, the purpose of this book will be to demonstrate what might be called the realization of the rational or, more generally, the realization of mathematics.

Furthermore, this need of application is felt just as strongly in pure mathematics, though there it is more hidden. It introduces an element of metaphysical duality into the mathematical sciences, which appear to be purely homogeneous, and thus offers a pretext for polemics between realists and nominalists. People seduced by the marvelous epistemological gain achieved by the introduction of formal axiomatic systems into mathematics (thereby enabling mathematical notions to function, as it were, in the void) may be too quick to condemn mathematical realism. Yet if one is careful not

to abstract too hastily from the psychology of the mathematician, one quickly discovers that there is more to mathematics than formal structures, and that every pure idea is accompanied by an imagined application, an example that does duty for reality. In contemplating the work of the mathematician one also notices that such work always stems from an extension of knowledge derived from reality, and that within mathematics reality fulfills its true function: to provoke thought. In a reasonably clear-cut manner, mathematical realism (in its various functional roles) sooner or later operates to *give body* to pure thought (the French word is *corser,* which literally means to give body to wine by adding spirits — trans.); realism gives mathematical ideas psychological permanence; it parallels the spiritual activity, thereby disclosing (in mathematics as in other realms) the duality of subject and object.

Since the purpose of this book is chiefly to study the philosophy of the physical sciences, it is on the realization of the rational in physical experimentation that we must focus our attention. This realization, which has its counterpart in technological realism, is in my view one of the distinctive features of contemporary science, which in this respect differs markedly from the science of centuries past; in particular, it is quite remote from both the agnosticism of positivism and the tolerant attitudes of pragmatism and has nothing to do with traditional philosophical realism. It is rather a realism at one remove, conceived in reaction to the usual notion of reality, as a polemic against the immediate; it consists of realized reason, reason subject to experimentation. The "reality" to which this realism corresponds is not transferred into the realm of the unknowable thing-in-itself. It has a noumenal richness of quite another order. The *thing-in-itself* is a noumenon by exclusion of phenomena, whereas scientific reality, I would argue, consists in a noumenal context suitable for defining axes of experimentation. Scientific experiment is thus reason confirmed. This new philosophical view of science paves the way for the reintroduction of standards of experimental validity: Since the necessity of any given experiment is demonstrated in theory before being revealed by observation, the physicist's task is to purify phenomena sufficiently to recover the organic noumenon. Goblot has considered the use of proof by construction in mathematical argument; constructive arguments have lately made their appearance in mathematical and experimental physics. The whole doctrine of the "working hypothesis" seems to me destined to quick obsolescence. To the extent that hypotheses have been linked to experiment, they must be considered just as real as the experiments themselves. They are "realized." The time of the adaptable patchwork hypothesis is over, and so is the time of fixation on isolated experimental curiosities. Henceforth, hypothesis is synthesis.

If immediate reality has now become a mere pretext for scientific thought rather than an object of knowledge, it is time to move from descriptive *comment* to theoretical *commentary*. Prolix explication astonishes the philosopher, who would prefer to believe that explication is always limited to unfolding the complex, to demonstrating the simple within the composite. But true scientific thought is metaphysically inductive; as we shall see repeatedly, it reads the complex in the simple, states the law that covers the fact, the rule that applies to the example. We shall discover how modern

scientific generalization, for all its vast scope, is the culmination of specialized knowledge. We shall uncover a kind of polemical generalization that shifts reason from the realm of the "why?" to the realm of the "why not?" We shall make room for paralogy alongside analogy and show how the ancient philosophy of the "as if" is superseded, in the philosophy of science, by the philosophy of the "why not?" As Nietzsche says, everything crucial comes into being only "in spite." This is as true of the sphere of thought as of the sphere of action. Every new truth comes into being in spite of the evidence; every new experience is acquired in spite of immediate experience.

Thus, apart from the gradual change in scientific thought brought about by the growth of knowledge, we will discover a virtually inexhaustible wellspring of novelty in the scientific spirit, novelty of an essential, metaphysical sort. Science is like a half-renovated city, wherein the new (the non-Euclidean, say) stands side by side with the old (the Euclidean). Anyone who thinks that such diametrically opposed idioms such as these are mere means of expression, more or less convenient systems of notation, attaches precious little importance to the proliferation of new scientific tongues. I, on the contrary, shall try to show that different means of expression may be more or less expressive, more or less suggestive, and therefore lead to more or less complete "realizations"; hence considerable importance must be attached to these expanded mathematical languages. Accordingly, I shall insist that such new doctrines as non-Euclidean geometries, non-Archimedean measures, non-Newtonian mechanics (associated with the name of Einstein), non-Maxwellian physics (associated with the name of Bohr), and noncommutative (or non-Pythagorean) arithmetic are valuable precisely because they confront us with suggestive dilemmas. Then, in the philosophical conclusion of the present work, I shall try to indicate the essential features of a non-Cartesian epistemology, to my mind capable of embracing all that is novel in contemporary scientific thought.

To avoid misunderstanding, it may be worth pointing out that there is nothing automatic about these negations, and one must not hope to find a simple formula for converting the new doctrines into terms comprehensible within the framework of the old. A genuine extension of knowledge has occurred. Non-Euclidean geometry was not invented in order to contradict Euclidean geometry. It is more in the nature of an adjunct, which makes possible an extension of the idea of geometry to its logical conclusion, subsuming Euclidean and non-Euclidean alike in an overarching "pan-geometry." First constructed in the margins of Euclidean geometry, non-Euclidean geometry sheds a revealing light on the limitations of its predecessor. The same may be said of all the new varieties of scientific thought, which have time and again pointed up gaps in earlier forms of knowledge. We shall discover that the new doctrines share many of the same characteristic features, the same methods of extension, inference, induction, generalization, complementarity, synthesis, and integration — all equivalents for the idea of novelty. And the novelty in question is profound: a novelty not of discovery but of method.

In the face of this ramification of epistemology, is there any justification for continuing to speak of a remote, opaque, monolithic, and irrational Reality? To do so is to overlook the fact that what science sees as real actually stands in a

dialectical relationship with scientific reason. After centuries of dialogue between the World and the Spirit, mute experience is impossible. An experiment that purports to falsify the conclusions of a theory in a radical sense must justify its opposition to that theory. Physicists are not easily discouraged by negative experimental findings. Michelson, who believed in the existence of the ether, worked up to the moment of his death trying to refute the negative results of his own famous experiment.[2] Other physicists subtly reinterpreted Michelson's results by arguing that while negative in terms of Newton's system, they were positive in terms of Einstein's. Looked at another way, these physicists were merely applying the philosophy of "why not?" in the experimental sphere. A carefully done experiment is always positive, in other words. To say this is not to resurrect the absolute positivity of experimentation as such, for an experiment is not "careful" unless it is complete, that is, unless it is conducted according to a well-conceived plan based on a mature theory. Ultimately, experimental conditions are merely conditions of experimentation. What may seem a mere nuance of phraseology actually casts the philosophy of science in an entirely new light, by accentuating the technical difficulties involved in carrying through any preconceived theoretical project. The lessons of reality are valid only insofar as they suggest new ways to interpret theory.

[2] Albert A. Michelson (1852–1931), an American physicist who determined the speed of light to a high degree of accuracy. The "famous experiment" referred to in the text is usually known as the Michelson–Morley experiment, which failed to demonstrate any motion of the earth relative to the so-called ether, the material medium through which it was thought light propagates. — Trans.

Once one has meditated upon the nature of scientific action, it becomes clear that rationalism and realism are constantly exchanging counsels. Neither the one nor the other is in itself a sufficient philosophical basis of scientific proof. We cannot gain access to utlimate reality in the physical sciences through mere observation. Nor can any rational argument define the limits of experimental research once and for all. Hence there is a need for methodological innovation, a need that we shall have occasion to examine more closely later on; theory and experiment are so clearly related that no theoretical or experimental methodology is guaranteed to retain its validity indefinitely. To put the point even more strongly, even the most fecund of methods may eventually become sterile without the fertilizing stimulus of new problems to solve.

The epistemologist must therefore place himself at the crossroads between realism and rationalism. From this vantage he can grasp the new dynamism of those contradictory philosophies and study the dualistic process whereby science simplifies the real and complicates the rational. The gap between explicated reality and applied thought is reduced, thus circumscribing the area in which the philosopher must look to discover how standards of scientific proof are established (*la pédagogie de la preuve*). And such understanding is, as I shall point out in the final chapter of this book, the only possible psychology of the scientific mind.

More generally, it should be of some interest to examine the fundamental metaphysical question of the reality of the external world in terms of scientific practice. Why must philosophy always begin with the opposition between some

vague Nature and some unformed Spirit, thus silently confounding the pedagogy of initiation with the psychology of culture? What audacity for the philosopher to assume that, abandoning his ego for a moment, he can recreate the entire World. And in any case what authorizes him to think that he can apprehend the simple, naked ego apart from its involvement in the acquisition of objective knowledge? In order to circumvent these basic questions, we shall look not only at the problems of science but also at the psychology of the scientific mind, regarding objectivity not as a primitive given but as something that is learned with great difficulty.

And there is more: It is in science, perhaps, that one sees most clearly the two meanings of the ideal of objectivity, the social as well as the concrete value of objectification. Science, as Lalande says, does not aim solely at "reconciling things with other things but even more at reconciling minds with other minds." Without the latter reconciliation, of course, there would be no problem. Faced with the most complex reality, we would, if left to our own devices, seek knowledge of a picturesque kind, calling upon our evocative powers: *The world would be our representation*. If, on the other hand, we were entirely given over to society, we would seek knowledge in the realm of the general, the useful, the conventional: *The world would be our convention*. In fact, however, scientific truth is a prediction or, better still, a predication. By announcing the scientific truth we call for a meeting of minds; together we convey both an idea and an experience, we link thought to experience in an act of verification: *The scientific world is therefore that which we verify*. Above the *subject* and beyond the *object*, modern science is based on the *project*. In scientific thought the subject's meditation upon the object always takes the form of a project.

Yet it would be a mistake to base one's argument on the rarity of true discovery in the history of man's Promethean efforts. For even in the humblest scientific thinking one finds evidence of the indispensable theoretical preparation for discovery. In an an earlier book [*La Valeur inductive de la relativité*] I did not hesitate to write that one does not point to (*montrer*) the real, one demonstrates it (*démontrer*). This is true particularly in cases involving an organic phenomenon of some kind (*organic* is here used in the broad sense of "having systematic structure" — trans.). When the object under study takes the form of a complex system of relations, then it can only be apprehended by adopting an appropriate variety of methods. Objectivity cannot be separated from the social aspects of proof. The only way to achieve objectivity is to set forth, in a discursive and detailed manner, a method of objectification.

It is of course a striking fact about science that it incorporates belief in demonstration as the basis of all objective knowledge. Observation is governed by a "code" of precautions that must be observed; observers are admonished to think before they look, to scrutinize carefully what they first see, and invariably to doubt the results of the initial observation. Scientific observation is always polemical; it either confirms or denies a prior thesis, a preexisting model, an observational protocol. It shows as it demonstrates; it establishes a hierarchy of appearances; it transcends the immediate; it reconstructs first its own models and then reality. And once the step is taken from observation to experimentation, the polemical character of knowledge stands out even more sharply. Now phenomena must be selected, filtered, purified, shaped by instruments; indeed, it may well be the instruments that produce the phenomenon in the first place. And instruments are nothing but theories materialized. The phenomena they produce bear the stamp of theory throughout.

The dialectical relationship between the scientific phenomenon and the scientific noumenon is not leisurely and remote but rapid and strict; after a few revisions, scientific projects always tend toward effective realization of the noumenon. A truly scientific phenomenology is therefore essentially a phenomeno-technology. Its purpose is to amplify what is revealed beyond appearance. It takes its instruction from construction. Wonderworking reason designs its own miracles. Science conjures up a world, by means not of magic immanent in reality but of rational impulse immanent in mind. The first achievement of the scientific spirit was to create reason in the image of the world; modern science has moved on to the project of constructing a world in the image of reason. Scientific work makes rational entities real, in the full sense of the word.

It is perhaps by examining the "technical activity" of thought that one can best gauge the essential philosophical dichotomy, which is summarized in Renouvier's "second metaphysical dilemma" under the head "dilemma of substance." The importance of this dilemma is crucial, for all the others follow from it. Renouvier puts it as follows: Either "substance is . . . a logical subject of undefinable qualities and relations" or it "is a being-in-itself and, as such, undefinable, unknowable."[3] But it seems to me that experimental technology introduces a third term between the two horns of the dilemma, namely, the substantialized substantive. In general terms, the substantive, or logical subject, becomes substance once its qualities, regarded as a system, are unified in a role. We shall see later how scientific thought constructs entities whose unity derives from their function as key elements in a system. For example, a group of atoms in a synthesized organic substance can help us to understand the transition from "logical" chemistry to "substantialist" chemistry, that is, from the first horn of Renouvier's dilemma to the second. The dialectic of the physical sciences seems to me more instructive than the crude dialectic of traditional philosophy, because the poles between which it moves are less extreme, less heterogeneous. The study of scientific thought can actually afford the clearest insight into the psychological question of objectification.

II

To grasp the dialectic of contemporary scientific thought and show its essential novelty is the philosophical purpose of this little book. When one looks at science, what is immediately striking is that its oft-alleged unity has never been a stable condition, so that it is quite dangerous to assume a unitary epistemology. Not only does the history of science reveal a regular alternation between atomism and energetics, realism and positivism, continuity and discontinuity, rationalism and empiricism; and not only is the psychology of the scientist engaged in active research dominated one day by the unity of scientific laws and the next by the diversity of things; but even more, science is divided, in actuality as well as in principle, in all of its aspects. Hence it has not been difficult to compile chapter after chapter illuminating this dichotomy. It would even be possible to reorganize the present work along somewhat different lines, so as to exhibit scientific reality as the intersection of two converging philosophical perspectives, the precision of theory always being subject to empirical correction. To determine whether or not a given chemical substance is pure, for example, one might specify its chemical function: The more clearly defined the function, the purer the substance.

[3] Charles Renouvier, *Les dilemmes de la Métaphysique pure* (Paris: F. Alcan, 1901), p. 248.

Does the fact that the study of science forces such a dialectic upon us raise metaphysical difficulties for a philosophy that aims at synthesis? I have been unable to provide a clear answer to this question. In all controversial matters, I have tried to indicate the conditions that a synthetic view must satisfy, wherever reconciliation of an experimental or theoretical sort seemed possible. Yet in every such instance reconciliation seemed to me a compromise. A more important point, however, is the following: Reconciling contradictory points of view does not do away with the dualism that is inscribed in the history of science itself, in every conceivable approach to the teaching of science, and indeed in the very structure of thought. Apparent dualities in the immediate phenomenon might well be eradicable: They could be explained, for instance, as fleeting anomalies or momentary illusions and said to place the phenomenon in question in an entirely different category. But this way of proceeding proves unsatisfactory when the ambiguity is found to reside in the scientific phenomenon itself. What I am proposing, therefore, is a new way of looking at ambiguity, a view sufficiently supple to comprehend the recent teachings of science. The philosophy of science is, I believe, in need of genuinely new principles. One such principle is the idea that the characters of things may be essentially complementary, a sharp departure from the tacit (philosophical) belief that being always connotes unity. If being-in-itself is a principle that communicates itself to the mind (much as the concept of field defines the relation between a material particle and the space in which it moves), then being is nothing but a symbol of unity. What would be needed, then, would be an ontology of complementarity less sharply dialectical than the metaphysics of the contradictory.

III

Without pretending in any way to be laying the philosophical foundations of modern physics, I hope to suggest how common philosophical positions must be modified to accommodate reality as it is revealed in the scientific laboratory. Quite obviously the scientist can no longer be a realist or a rationalist in the manner of those philosophers who believed that it was perfectly possible to confront Being in either its outward prolixity or its inner unity. For the scientist Being can never be wholly comprehended by either experience or reason. It is therefore the task of epistemology to explain the ever-changing synthesis of reason and experience, even though achieving such a synthesis philosophically might appear to be a hopeless problem.

As for the plan of the remainder of the book, chapter 1 is devoted to a study of the origins of non-Euclidean geometry, focusing on the dialectical division of geometrical thinking and the subsequent synthesis. This chapter will be as brief as possible, since its only purpose is to show the dialectic of reason at work in its simplest and purest form.

In chapter 2 we continue the study of the dialectic by taking up the study of non-Newtonian mechanics.

In the following chapters we encounter questions that are less general in nature and at the same time more difficult to answer. We shall have occasion to consider the following series of dilemmas: matter and radiation, waves and particles, determinism and indeterminism.

We shall discover that the last of these pairs profoundly affects our concept of the real, which is made to seem strangely ambivalent. The question then arises whether Cartesian epistemology, based as it is entirely on appeals to simple ideas, is adequate to describe contemporary scientific thought. We shall find that the spirit of synthesis that animates modern science does not operate at the same depth as the Cartesian notion of *composition,* nor with the same degree of freedom. We shall then attempt to show that what is involved in the free and far-reaching synthesis of modern science is the same dialectic we discovered earlier in our study of non-Euclidean geometry. Hence this final chapter is entitled "Non-Cartesian Epistemology."

I shall take advantage of every opportunity to stress the innovative nature of the modern scientific spirit. Frequently innovation will be sufficiently apparent merely by comparing examples drawn from the physics of the eighteenth or nineteenth century with examples from the present century.

In this way we shall discover the incontestable novelty of contemporary physical science, both in detail and in the general structure of knowledge.

Chapter One

Dilemmas in the Philosophy of Geometry

In a brief and necessarily elementary chapter we cannot hope to follow the prodigious developments that have taken place in the philosophy of geometry over the course of the past century. The dialectical progress of thought is clearer in geometry than in any other science, however, and it is therefore important that we attempt to characterize the development of geometry in dialectical terms, that is, that we show first how the antithesis between Euclidean and non-Euclidean developed and second how these two "opposites" were ultimately combined in a new and higher synamounts to no more than an ephemeral moment, which hardly counts alongside periods of firm scientific accomplishment, sound explanation, and solid teaching. Yet it is in these brief moments of discovery that we must learn to grasp the decisive turns in scientific thinking. It is by making such moments live again in our teaching that we demonstrate the dynamic and dialectical nature of the scientific spirit. For it is at such times that science must suddenly confront contradictions in the evidence and doubts about its fundamental axioms; it is then that new *a priori* syntheses are proposed, syntheses that, like de Broglie's ingenious theory, create a new reality alongside the old; and it is then that we see those sudden reversals of thought of which Einstein's equivalence principle is one of the clearest examples. Meyerson's whole argument about the persistence of the substantial idea of force crumbles when confonted with a principle of this sort. If we simply recall the fact that, according to the general theory of relativity, an appropriate change of frame of reference is enough to make the gravitational force vanish, the specious character of the notion that this force is "real" becomes obvious.

It does not matter how long realism affords the mind the luxury of intellectual repose; the striking fact is that every fruitful scientific revolution has forced a profound revision of the categories of the real. What is more, realism never precipitates such crises on its own. The revolutionary impulse comes from elsewhere, from the realm of the abstract. Mathematics is the wellspring of contemporary experimental thought.

Chapter Six

Non-Cartesian Epistemology

I

Georges Urbain, a chemist who has been a leader in the careful and systematic application of the new scientific methods, is quick to assert that not even the best of methods can last forever.[1] In his view, every method eventually loses its initial fecundity. There always comes a time when scientists lose interest in searching for the new along old trails,

[1] Georges Urbain (1872–1938), chemist specializing in the study of rare earths.

when science cannot progress except by developing new methods of research. Scientific concepts themselves may lose their universality. In the words of Jean Perrin, "any concept will ultimately lose its usefulness and even its meaning as research departs more and more from the experimental conditions under which it was formulated." Concepts and methods alike depend on empirical results. A new experiment may lead to a fundamental change in scientific thinking. In science, any "discourse on method" can only be provisional; it can never hope to describe the definitive complexion of the scientific spirit.

The fact that sound methods are constantly changing in science is a fundamental feature of scientific psychology. Scientific method is always explicit. Familiarity is no guide when it comes to observation. A method and its application are intimately connected. Even pure theorists must pay constant attention to methodological issues. In the words of Dupréel, "a proven fact is supported not by intrinsic evidence but by proof."[2]

The question therefore arises whether the psychology of science cannot simply be reduced to a matter of *conscious methodology*. It would then be almost a normative psychology, a set of pedagogical principles distinct from scientific knowledge itself. In a more positive light, the essence of scientific psychology would then lie in the reflection whereby experimental *laws* are transformed into *rules* for discovering new facts. This is how laws become coordinated with one another and how deductive thinking is introduced

[2] Dupréel, "De la nécessité," p. 13.

into inductive science. Scientific knowledge accumulates, one might say, without taking up additional room in the mind, and this is one difference between *scientific* knowledge and empirical erudition: Science uses tested methods to filter the facts. This normative element is of course most visible in the psychology of the mathematician, for even though his thoughts are always in some sense "correct," there is a fundamental psychological distinction to be made between what he knows to be true and what he simply conjectures or intuits. But a normative component can also be sensed in the essentially organic conception of things that encrusts logical thought in the world. In any case, experimental testing always begins with what one believes to be logical. Hence the failure of an experiment sooner or later entails a change of logic, a deep change in our thinking. Everything stored up in memory must be reorganized along with the mathematical framework of science itself. There is constant interchange between the psychology of mathematics and the psychology of experimentation. Little by little the dialectics of mathematical thought enters into the empirical realm. Methodological change follows the contours of mathematical argument.

But to get back to Urbain's point, are there methods that are exempt from obsolescence? The answer would seem to be no, provided we judge the matter from the standpoint of scientific research, that is, from the realm in which the assimilation of the irrational by reason never fails to bring about a reciprocal reorganization of the domain of rationality. It is often said, accordingly, that work in the laboratory never follows the prescriptions laid down by Bacon or J. S. Mill. Carrying this one step further, I maintain that there are also reasons to doubt the usefulness of Descartes's methodological dicta. The next section will develop this point.

II

The foundation upon which Descartes erects objective thought is too narrow to accommodate the phenomena of physics. The Cartesian method is *reductive* rather than *inductive*. But reduction distorts analysis and hinders the extension of objective thought, and without extension there is no such thing as objective thought or objectification. What I shall show is that Cartesian method, so useful a tool for *explicating* the world, is inadequate when it comes to *complicating* experience — the true function of *objective research*.

Let me begin by asking what justifies the initial separation of simple natures? To take an example whose generality is especially telling, recall that in microphysics it is objectively impossible to distinguish between object and motion. De Broglie stresses the same point: "Early in the development of modern science, Descartes argued that it was important to explain natural phenomena in terms of figures and motions. The uncertainty relations express the fact that a rigorous description of this kind is impossible, since we can never know both the figure and its motion at the same time."[3] Hence the uncertainty relations should be interpreted as impediments to absolute analysis. In other words, we must de-

[3] Louis de Broglie, *Théorie de la quantification dans la nouvelle mécanique*, p. 31.

fine the basic notions of physics in terms of relations, just as we define mathematical objects by stating the axioms that determine how they relate to one another. Parallel lines do not exist *prior* to Euclid's postulate; they are *ulterior*. Similarly, in microphysics, objects do not exist *prior* to a method of detecting them. Definitions depend on methodology: Tell me how to find you and I will tell you what you are. Simple always means simplified. We cannot use simple concepts correctly until we understand the process of simplification from which they are derived. Unless we are willing to make this difficult epistemological reversal, we cannot hope to understand the real point of mathematization.

We have already seen several times that the phenomena of microphysics are in essence complex. While it was logical for scientists influenced by Descartes to construct the complex out of the simple, modern science tries to read the real complexity of things beneath the simple appearance; it seeks diversity beneath identity and tries to go beyond superficial and summary views. But the opportunity to achieve these goals does not arise of its own accord; it does not lie on the surface or beckon to the observer. Scientists must delve into the very heart of matter, into the fabric of its attributes. It is theory that guides research. What feats of pure thought, what faith in the realism of algebra, was necessary to bring together matter and motion, space and time, matter and radiation! Descartes was able to deny the essential diversity of matter and motion, but modern science, which has found the way to bring matter and energy into collision, has discovered that diversity is everywhere: Simply by changing the parameters of the colliding particles, it is possible to produce different colors, different kinds of radiation, different amounts of heat. Matter is no longer just an obstacle that repels moving particles. It transforms them and is itself transformed. The smaller the "grain" of matter, the greater its substantial reality; as we observe smaller and smaller volumes, the structure of matter becomes deeper and deeper.

Hence in theoretical physics even more than in experimental physics there is a need for synthetic *a priori* judgements. An organic conception of microphysical phenomena, bringing together at a deep level all of the fundamental notions of the subject, has therefore become increasingly essential. The main goal of contemporary physics, as we have seen, has been to effect a synthesis of matter and radiation. Underlying this physical synthesis is a metaphysical synthesis of object and motion. The synthetic judgment involved is quite difficult to formulate, because it runs strongly counter to the analytical habits formed in the course of ordinary experience, for which there is no difficulty at all in dividing phenomenology into two separate categories, one static (the phenomenology of the object), the other dynamic (the phenomenology of motion). But it has now become essential not to isolate the various aspects of a phenomenon; in particular, it is important to do away with the notion of an object *at rest*. In microphysics, matter at rest is an absurdity, because matter exists for us only as energy and sends us messages only by way of radiation. What, then, is an *object* that can never be examined in its static condition? Nothing suitable for physical analysis. The physicist seeks instead to combine position and motion in his equations, in which the variables describing the one and the other are said to be *conjugate* (the term was taken originally from the Hamiltonian formal-

ism for classical mechanics — trans.). Of course his use of these conjugate variables is still guided by ordinary intuition, and this might lead one to believe that all that is going on is that two simple notions are being combined. But a closer look at mathematical physics gives the lie to this erroneous assumption. For it becomes clear that the conjugate variables enter into the calculations in an indirect manner, and that what is called momentum need not correspond to our original, intuitive idea of momentum. The key parameters are actually derived from a general mathematical formalism. In other words, the usual concrete description of the problem is replaced by an abstract mathematical description. This mathematical description is not clear from its elementary constituents but only from the final equation, whose synthetic value is manifest.

In raising the idea of a non-Cartesian epistemology, my intention is not to condemn Cartesian physics or mechanism (whose spirit remained Cartesian) but rather to criticize the doctrine of simple and absolute natures. The new scientific spirit has profoundly altered our understanding of intuition. Intuition is no longer direct and prior to understanding; rather, it is preceded by extended study, the result of which is to develop a fundamental duality in our thinking. All our elementary ideas are doubled, ranged alongside complementary ideas. Intuition must henceforth involve a choice. Hence scientific description cannot help being fundamentally ambiguous, and this fact raises problems for the immediacy of intuition on which Cartesian judgment relies. Descartes believed not merely that absolutes exist in the objective world but also that they are wholly and directly known to us. Our clearest ideas, in fact, pertain to these absolutes, in Descartes's view. This is so because simple things are indivisible. We see them whole because we see them as separate objects. Clear and distinct ideas are wholly free of doubt, and by the same token simple objects are wholly free of relations with other objects. Nothing could be more anti-Cartesian than the slow change that has been brought about in our thinking by the progress of empirical science, which has revealed a wealth of information never suspected in our first intuition. Einstein's discovery of relativity, for example, has revealed a world of such richness and complexity that the inadequacy of Newtonian physics soon became apparent. Similarly, de Broglie's wave mechanics *completes* (in the full sense of the word) both classical and relativistic mechanics.

But let us assume for a moment, with Descartes, that the elements of the real are truly and integrally given. Can we at least say that the Cartesian construction that unites them is truly synthetic in form? The answer, it seems to me, is that the Cartesian inspiration remains analytical even here, for Descartes believed that no construction is clear to the mind unless the mind knows how to take it apart. He tells us that when we confront something composed of many elements, we must always look for the simple parts, the basic components of the system. Never is a composite idea comprehended in terms of its synthetic value. Descartes never pays heed to the reality of the complex, to the emergence of qualities in the whole not evident in the parts. Rather than accept the complex idea of energy, for example, he prefers to go against the intuition of his own senses and favors a thoroughly reductionist account developed by "intellectual intuition." Nor would he accept that curved trajectories are

truly elementary. The only true motion, for Descartes, was simple, uniform, rectilinear motion, the only motion of which the mind can form a clear idea. In thinking about motion along an inclined plane, he was reluctant to assume that the speed of the moving object varies continuously, because speeds in his view should be separate entities, simple and distinct *elements* of a well-defined fall.

Once again, let me compare these tenets of Cartesian epistemology with the contemporary scientific ideal of complexity. Modern science begins with synthesis. Its basis is a complex amalgam of geometry, mechanics, and electricity. It develops its arguments in the context of space-time. It puts forward innumerable sets of axioms. Epistemologically, it relies for clarity on combining ideas rather than on trying to understand individual objects in isolation. In other words, instead of intrinsic clarity it relies on what I shall call operational clarity. Relations do not exemplify objects; objects exemplify relations.

Of course the fact that the epistemology of contemporary science is non-Cartesian should not blind us to the importance of Cartesian thought, any more than non-Euclidean geometry should blind us to the limpid organization of Euclidean thought. But these different ways of organizing our ideas should suggest a general truth about systems of thought that lay claim to totality. *Completeness* should henceforth be regarded as a *de jure* rather than a *de facto* issue. Whether a system is or is not complete can be judged in ways that have nothing to do with the simple enumeration of possibilities. Contemporary science does not enumerate, it theorizes. It does not count up its wealth but proposes new ways of amassing further riches. We must pay close attention to whether or not our knowledge is complete and remain on the lookout for occasions to extend it, to follow one or another avenue of dialectical development. We want to be sure that we have listed all the variables relevant to any particular phenomenon. When we ask ourselves if we have in fact discovered all the degrees of freedom of a system, we are obviously asking a theoretical and not an empirical question: Has anything been left out of the theory? We may find that something was overlooked in our original intuition. It is *theoretical oversights* that we fear, for it is obvious that physicists and mathematicians don't *forget* anything — they simply leave things out.

What we discover when we study physics is that experiments, however important as methods of proof, are always simplifications, selections, examples, or extensions of theory. Intuitive ideas are made clear in a discursive manner, by progressive illumination, by illustration in a series of examples that bring one or another notion into clearer focus. Dupréel elaborates on this point as follows:

> If, by an intellectual act, I posit a simple truth, a second act is needed in order to see what it is good for (*s'en rendre compte*). Generalizing this observation, we see the error of those who believe that necessary and unconditional truths, duly regarded as such, can both at the same time be usefully applied. Once an axiom is posited, a second act is always necessary to establish its application, that is, to establish the circumstances under which it may be invoked. How was it that Descartes and all the other champions of self-evident necessity failed to perceive that the crucial moment comes not when one hangs a hook on a wall,

> no matter how solid that hook may be, but when one finally attaches the first link of a chain of deductions to that hook? However irrefutable your *cogito* may be, I am waiting for you to draw from it some [concrete] conclusion.[4]

It is impossible to give a better statement of the discursive nature of clarity and of the identity that exists between evidence and varied application. In order to measure the epistemological value of a fundamental idea, we must always look to induction and synthesis. For it is there that we discover the importance of the dialectical process that discloses variety within identity and that clarifies by completing our initial intuition.

III

The reader may be willing to grant that Descartes's rules of thought do not answer the multifarious requirements of research in either theoretical or experimental science and yet still object that those rules retain some pedagogical value. Here again, however, I must insist on the gap between the true spirit of modern science and the simple impulse to order and classify. If I may draw an analogy between science and religion, what is needed is a distinction between "regular" and "secular" science (in Catholicism, the regular clergy are bound by the *regula* or rule of an order and live together in communities, whereas the secular clergy work in the world, or saeculum, ministering to their flock — trans.): Regular science is the science practiced in the research laboratory,

[4] Dupréel, "De la nécessité," p. 14.

while secular science finds its disciples among the philosophers. If the issue is one of teaching students the importance of taking orderly notes, writing clear reports, making precise conceptual distinctions and thorough observations, then nothing is more valuable than the teachings of Descartes. They amply instruct the student in the use of those careful, objective methods that give historical or literary accounts their rightful authority, even as the mathematical and physical sciences are expressing themselves with greater reserve. And in any case it is scarcely conceivable that a physicist could ever violate Descartes's rules. None of the major revolutions in physics has resulted from correcting an error of this kind.

Furthermore, it is apparent that in the context of modern culture there is nothing very startling about Descartes's rules. Not one reader in a hundred of Descartes's *Discourse on Method* has an intensely personal intellectual reaction to that once path-breaking work. Forget the historical charm of the *Discourse*, forget its winning tone of innocent and naive abstraction, and the book has little to offer but common-sense advice — a rather dogmatic, if uncontroversial, set of rules to guide the intellectual life. To the physicist it will all seem obvious. Descartes's rules say nothing about what precautions to take in order to ensure that a measurement will be precise; they offer nothing to calm the anxieties of contemporary scientists. On the other hand, they do prevent teachers from introducing the simple paradoxes that can be so useful in teaching even elementary science. Based on my own experience of teaching basic physics and philosophy, I can say that it isn't easy to interest young minds in the Cartesian method. The problem is that we are now

at a stage in human intellectual development where we face a real, and useful, crisis in our understanding of the physical world, but there is no corresponding crisis in our philosophical culture.

Cartesian skepticism, which should be the point of departure for any attempt to teach metaphysics, is not easy to get across. As Frost says, it is a far too solemn affair (*ein sehr feierliche Gebärde*).[5] It is quite difficult to concentrate a youngster's attention long enough to make him perceive the value of doubt. Suspension of judgment prior to objective scientific proof (which is characteristic of the scientific spirit) and clear understanding of the axiomatic meaning of the principles of mathematics (which is characteristic of the mathematical spirit) reflect a less pervasive skepticism than Descartes's, but a skepticism that for that very reason is clearer and more robust. Psychologically speaking, this more limited skepticism, a prerequisite of all scientific research, is still useful. It is an essential, not a transitory, feature of the structure of science.

IV

The time has now come to leave these generalities of method behind and to attempt, by examining several specific scientific problems, to show the new epistemological relationship between simple and composite ideas.

There are no simple phenomena; every phenomenon is a fabric of relations. There is no such thing as a simple *nature*, a simple substance; a substance is a web of attri-

[5] Walter Frost, *Bacon und die Naturphilosophie* (Munich, 1927), p. 65.

butes. And there is no such thing as a simple idea, for as Dupréel has pointed out, no idea can be understood until it has been incorporated into a complex system of thoughts and experiences. Application is complication. Simple ideas are working hypotheses or concepts, which must undergo revision before they can assume their proper epistemological role. Simple ideas are not the ultimate basis of knowledge; after a complete theory is available, it will be apparent that simple ideas are in fact simplifications of more complex truths. If one wants to understand the dialectics of the simple and the complete, nothing is more instructive than to consider experimental and theoretical work on atomic structure and spectra, an almost inexhaustible source of epistemological paradoxes. To take an example that we shall soon be studying in some detail, it is fair to say that in certain respects an atom with several electrons is simpler than an atom with only one electron: the more complex the organization, the more organic its complexion. The curious quantum-theoretical notion of degenerate states sheds a new light on certain supposedly simple phenomena. The epistemological implications of such problems are worth pursuing.

Let us consider, therefore, how physicists approached the problem of atomic spectra. The first spectrum that scientists were able to decipher was of course that of the hydrogen atom. The lines of the hydrogen spectrum are more clearly organized into distinct series than the lines of any other spectrum, and it was a part of the hydrogen spectrum that was first described in mathematical terms, in the formula for the so-called Balmer series. What is more, the hydrogen atom itself was seen as having a particularly simple structure, consisting of a single electron revolving around a single pro-

ton. Thus the problem of hydrogen was thought to be simple in two respects: (1) A simple mathematical formula describes its spectrum, and (2) it was possible to give a simple intuitive picture of its structure (the so-called Bohr model — trans.).

Physicists then tried to understand more complex atoms in terms of information derived from the study of hydrogen, which constituted a kind of "working phenomenology." Here, science adhered to the classical Cartesian ideal. The mathematical and intuitive approaches to the study of complex atoms are therefore interesting to consider.

Mathematically, it became clear that, allowing for a suitable correction coefficient, an analogue of the Balmer series can be found in the spectra of other atoms besides hydrogen. The coefficient turned out to be the square of the atomic number (i.e., the number of protons in the nucleus of the atom — trans.). This coefficient did not appear explicitly in Balmer's original formula because the atomic number of hydrogen is one. For a time, then, the Balmer formula was held to be valid for all atoms: It was, physicists believed, the simple and general law of atomic spectra.

Improvement in the techniques of spectroscopic measurement eventually led to corrections in certain parameters of the formula, however. These spoiled the elegant simplicity of the original mathematics. Yet these corrections, essentially empirical in origin, appeared to leave the functional role of the various terms in the formula intact, so that the rational basis of the theory still seemed valid. The empirical results could be explained, it was believed, as *perturbations* of a general law. Two-stage processes of this sort are actually rather common in the history of science In the first, more feverish stage, the general law is established. This is followed by a second, more relaxed stage, in which various complexities, at first ignored, are interpreted as perturbations of the simple, general law. This second stage generally lasts for some time. The whole process is a fundamental feature of a characteristic psychological structure, in which there is a sharp division between the clarity of theory and the inevitable murkiness of reality, between what is lawful behavior and what is irregular, and indeed, as is all too readily assumed, between what is rational and what is irrational. These oppositions mark the boundary between intellectual courage and intellectual torpor. Isn't the theorist's work done, some will say, when the broad outlines of a phenomenon have been worked out? Do details, subtleties, fluctuations, really matter? Isn't it enough to "interpret" them in terms of the law or to declare them to be marginal disturbances? A strange dialectic, and a strange peace of mind.

The temptation to achieve a clear result in a short period of time is sometimes so great, however, that the scientist will persist in applying a theoretical model inappropriate to the problem at hand. If we imagine that we see a unicorn in the clouds, the wind may blow for a long while, stretching the fantastic animal this way and that without eliminating it from view altogether; but if someone should interrupt our reverie, the unicorn is likely to vanish in an instant. If enough perturbations accumulate, it may become necessary to take a fresh look at a complex problem. This was precisely what took place in the mathematical theory of spectra: The introduction of matrices made it easier to deal with a large number of terms. I shall have a bit more to say about the

complicated mathematics involved in a moment. But first I want to show that there was a parallel increase in complexity of the associated atomic "models."

What happened to the mathematical formulas also happened to the "images" that illustrated those formulas. Again, an effort was made to replace the electron trajectories in the original models with more complex, perturbed trajectories. Discrepancies quickly emerged, however, because the helium atom — simple as it is with its two electrons and its nucleus — proved to be quite difficult to analyze. Research therefore focused on what was called the hydrogenoid character of certain elements in either the normal or the ionized state. And investigation did indeed turn up Balmer-type series in the spectra of ionized helium, alkaline metals, and ionized alkaline-earth metals (all of which possess a single electron in the outer shell, this being the property referred to as hydrogenoid — trans.), from which it was inferred that the spectral picture is basically the same as for hydrogen, so long as there is a single electron revolving around a more or less complex nucleus. In other words, the spectrum of an atom depends almost exclusively on the outer electron. It appeared, then, that the original simple picture was finally vindicated, and that the general law was now understood.

But complexity would ultimately exact its revenge: Not only was it a mistake to look for an artificial hydrogenoid character in other elements, but it also turned out that this so-called hydrogenoid character was not really a simple character at all. Indeed, if I may anticipate the ultimate results, hydrogen turns out to be no simpler than the hydrogenoids, and its pseudosimplicity is actually a misleading sign that obscures what is really going on. Paradoxical as it may seem, it is nonetheless true that if we really want to understand the hydrogen spectrum, we should first study the more complex hydrogenoid elements. In short, the only way to form a correct idea of the simple is first to study the complex in depth.

The problem is that for quantum-theoretical purposes the hydrogen atom doesn't know how to count: Bohr's model seems to allow room for only a single quantum number. Léon Bloch puts it quite well: "The spectrum of hydrogen is in fact a *degenerate* alkaline spectrum, that is, a spectrum that fails to reflect differences in the value of *l*,[6] where *l* is of course the "azimuthal" quantum number (corresponding to classical angular momentum — trans.), which in the non-degenerate case gives rise to the double periodicity observed in the spectra of alkaline metals. And that is not the end of the story. It is possible to assign still a third quantum number to the so-called optical electron of the alkaline metals, and this leads to the prediction of still a third periodicity in the corresponding spectra. "It is interesting," writes Bloch,

> to investigate whether traces of this triple periodicity exist in the hydrogen atom itself when we regard it as a degenerate instance of the alkaline metal case. There are, predictably, substantial difficulties in carrying out experiments to do this. For in lithium, the first true alkaline metal, the doublet structure is so subtle that it took very special conditions to render it visible. For hydrogen the expected doublet structure is finer still. Nevertheless, the resolving power of present-day interference spectroscopes is so great that it has been possible to detect, beyond any doubt, the fine structure of the lines of the Balmer series, and in particular the red line Hα. . . . The decomposition of the lines of H I and He II into extremely tight multiplets, whose structure is the same as that of the alkaline multiplets, shows that there is no essential difference between the hydrogen and hydrogenoid spectra. . . . Thus we see that the simplest of atoms is already a complicated system.

[6] Léon Bloch, "Structures des spectres et structure des atomes," *Conférences d'Actualités scientifiques et industrielles*, 1929, pp. 200, 202.

A possible objection to this line of argument is the following: If Peter resembles Paul, then Paul resembles Peter, and if hydrogen is spectroscopically similar to the alkaline metals, then the reverse is also true. But this objection misses the point that a shift has taken place in the *basic image* of the phenomenon, a shift that entails a total transformation of the underlying phenomenology. Careful examination of the sequence of events leads to the following conclusions: The hydrogenoid image is not imposed upon the alkaline metals, but rather the reverse. At the Cartesian stage of research, when investigators were still trying to work from the simple to the complex, the results were stated in the following form: The alkaline metals have a hydrogenoid spectrum. But at the non-Cartesian stage, by which time research had achieved a complete picture of the phenomenon and turned back to the case of hydrogen as a simplified or degenerate instance, the results could be summed up as follows: The hydrogen spectrum is an alkalinoid spectrum. In order to give a detailed account of the spectroscopic data, the more complicated spectrum (here that of the alkaline metals) had to be treated first. It was this spectrum that first opened experimenters' eyes to the fine structure of the spectral lines. No one would ever have looked for the doubling of lines in the hydrogen spectrum had such doubling not already been found in the alkaline spectra.

A similar problem arises, as we shall see shortly, in connection with the hyper-fine structure of the hydrogen spectrum. It is clear that study of the hydrogen spectrum alone would never suggest the need for still more careful investigation. The Balmer formula has no clues to offer experimentalists. Nor does the Bohr model suggest possible theoretical reasons for additional spectral periodicities. The only reason to consider the angular momenta of the nucleus and the orbital electron in hydrogen is that this procedure proved successful in dealing with more complex, that is, more organic, atoms.

It is not simply in regard to theory and intuition that the hydrogen atom turns out to be deficient: It also leaves much to be desired from the experimental point of view. Powerful instruments and extreme precision are required to detect, in the relatively unsophisticated hydrogen atom, certain phenomena that are more easily detected elsewhere. The most apparent features are not always the most characteristic, moreover; we must be careful not to fall into the trap of a "positivism of the first glance." If we fail to heed this caution, we risk mistaking a degenerate case for an essential truth.

Thus, even though it is historically true that the study of the hydrogen atom was instrumental in the development of spectroscopy, hydrogen is far from being the best starting place for further inductive theorizing. The theory of alkaline spectra is an inductive extension of the theory of the hydrogen spectrum. But then the consequences of the new theory for the hydrogen atom have to be deduced from the results for the alkaline metals. Then further induction is necessary — induction is always necessary. And in this case it leads to the discovery of additional structure in the hydrogen spectrum; or, better still, it leads to the *production* of additional structure by powerful, artificial means.

Thus far, we have followed the interplay of the simple and the complex in only one case, that of the hydrogen-hydrogenoid spectrum. But if the hydrogen model proved insufficient, it seems reasonable to expect that the hydrogenoid model too will ultimately be found to be an artificial simplification rather than a definitive theoretical model. And indeed, the model does prove less and less adequate as one moves from the first to the eighth period of Mendeleev's periodic table. The spectra of bismuth and lead bear little resemblance to hydrogenoid spectra, and the spectrum of iron is completely indecipherable in terms of the hydrogenoid pattern.

How does science react to such a failure? By declaring that reality is hopelessly complex and fundamentally irrational? Such an answer, presuming as it does acceptance of defeat, hardly does justice to the courage of modern science. The scientist's response is rather to pursue his education in the theoretical and experimental study of complex phenomena. Theoretically, there is reason to hope that wave mechanics will provide the means to calculate, *a priori*, the terms of spectral series unrelated to the Balmer series (though numerous approximations may be necessary). On the experimental side, clarity should come from study of the hyper-fine structure. Just as the study of the fine structure of the alkaline spectra helped to clear up the degenerate structure of hydrogen, so, too, may the hyper-fine structure of complex spectra such as that of bismuth help to introduce new models into general spectroscopy. In Bloch's words, "the more spectral analysis was refined, the more allegedly simple structures seemed to decompose. The hyper-fine structure, like the fine structure, may prove to be not the exception but the rule."[7] It would be impossible to overestimate the importance of this observation. It is indicative, I think, of nothing less than a Copernican revolution in empiricism. The very idea of perturbation seems destined for eventual elimination. No longer will there be simple laws and perturbations but complex, organic laws, occasionally affected by various "viscosities" or "vanishing terms." The old ideal of the simple law was reduced to nothing more than the simplicity of an example, a truncated truth, a schematic outline, a blackboard sketch. These simplified images still have their usefulness for teaching purposes, because historically teaching has relied on suggestive, enticing examples. But the ease of teaching by means of such devices, our easy confidence in the known, our tranquil acceptance of established systems, has been dearly bought: The easy way is always dearly bought. The danger is that the student will mistake the scaffolding for the finished edifice. Profound knowledge is finished knowledge, and the finished structure of the old physics lay in the details of perturbation theory, in the sophisticated approximations to exact solutions. That was

[7] Ibid., p. 207.

where the equation between noumenon and phenomenon became real, and where the noumenon revealed the technical factors that impelled its further development. But now the static duality of the rational and the irrational has been supplanted by the dialectics of active rationalization. Theory complements experience. Exceptions are eliminated from above, as it were, by adding attributes and functions to account for the accumulation of accidental facts.

That complete theory should take precedence over sophisticated observation is strikingly clear when we look at an experimental observation such as the Zeeman effect, which was responsible for a long series of theoretical developments. The Zeeman effect involves the splitting of spectral lines under the influence of a magnetic field. This observation raises the following question: "Might not such a separation exist in latent form in the absence of a magnetic field?"[8] This is just another way of saying that the real structure of the atom should be interpreted in terms of various "principles of possibility"; it reflects the scientist's confidence that *compossible* phenomena [phenomena that vary together] are the first, and eminently rational, sign of something real. Thus we begin to think in terms of *prior* structure (prior, that is, to experiment — whatever it is in the atom that interacts with the magnetic field to produce the Zeeman effect is a "real" part of its structure — trans.); we begin to think in terms of *projects* for constructing the structure of the atom, *plans* for getting at its reality. In other words, we begin to fashion theoretical molds to shape our experimental technique.

[8] Ibid.

In a similar vein, we may ask if it really is as absurd as it may seem to wonder what role the Pauli exclusion principle might play in the hydrogen atom. What exactly does this question mean? The Pauli principle is absolutely general. It states that no two electrons in the same atom can have identical quantum numbers. How is this principle to be interpreted in the case of hydrogen, which has only one electron? We can of course opt for simplicity, rejecting the lesson of the Pauli principle (which is based on the study of more complex atoms) on the grounds that there is only one opportunity for quantification. This choice leads to a simplified picture of hydrogen and limits the experimental possibilities. Should we instead imagine phantom electrons that would provide opportunities for more complex quantum-mechanical analysis? Again, the problem is the same: how to count with a defective abacus, how to interpret the law of large numbers when we are dealing with numbers that are small, how to recognize the rule and all its exceptions when dealing with but a single, and manifestly exceptional case? Stated in more general terms, the question is this: How can the simple illustrate the full? Here we have the hydrogen atom, stoichiometrically the simplest element of all, just as the amphioxus is the simplest vertebrate. There is no doubt that hydrogen combines the two electrically simplest forms of matter, the positive and the negative charge. How are we to untangle the skein? Why not complete the tangle by pushing matter's powers of composition to the limit? Won't various functions be clarified once we have seen their full scope? What holds matter together may become all the more obvious once we have created a tightly woven fabric of particles, increasing to the maximum possible extent the

number of relations, functions, and interactions involved. The free electron teaches us less than the bound electron, the atom less than the molecule. Yet we must take care not to push such composition too far. We must remain in the realm where composition is still organic if we wish to understand the equation of the complex and the complete.

We have in fact just recently entered the "age of the molecule," after long years devoted to the theory of the atom. To persuade ourselves of the importance of this new age we have only to think back one hundred years: The artificial nature of the old concept of molecule then becomes quite evident. The definition of *molecule* common at the time depended on an obviously artificial distinction between physical and chemical phenomena. A molecule of a particular substance was the smallest unit of that substance that could be separated out by physical means, whereas an atom could be separated from a molecule by chemical means. Viewed as a composite, a molecule was little more than an amalgam of atoms. All the chemical functions were thought to be inherent in the elements, that is, in the constituent atoms. In keeping with the teachings of realist metaphysics, scientists believed that the categorical attribution of properties to elementary substances had explanatory value. Little by little, however, people began to realize that it was not at all obvious why all properties must inhere in the simple elements, and that some properties might be attributes of the molecules themselves. Take one example: the concept of valence. Chemical valence, a scientific concept that was originally a rationalization of the latent substantialist notion of affinity, is now judged to be something that has no precise meaning apart from actual compounds. As Cabrera says, "valence is something more complex than once thought, which has to do with the stability of the new dynamical configurations of the outer electrons resulting from the mutual perturbations of atoms in contact with one another. It is clear that the details of this configuration and the degree to which it is stable depend on the structure of the atoms involved, so that, strictly speaking, valence is not a property of each isolated element but of all the atoms in a compound."[9] In other words, affinity depends upon communion. To enter into a compound is to "come to terms" with all the other elements in that compound. It is misleading to think that an element is so "different" that it cannot possibly enter into an association with other elements — just as misleading as to think of "difference" as an impediment to human associations. Hence there is no reason to pursue the study of the simple element as such, the being-in-itself, since properties are a product of the compound, of the relation.

The propositions that I am setting forth may seem dangerous in that they run counter to the usual practice of dogmatically asserting the basic notions of a discipline. But the very idea that such basic notions exist is in some respects contradictory. Empirical notions derived from ordinary experience have to be revised and modified repeatedly before they can be of any use to microphysics, which defines reality by *inference* rather than *discovery*. Non-Cartesian epistemolgy is thus by essence and not by accident in a constant state of crisis. Let me try once again, therefore, to demonstrate the way in which modern science attempts,

[9] Cabrera, "Paramagnétisme et structure des atomes combinés," *Activation et structure des molecules*, 1928, p. 246.

by focusing on complex cases, to fashion its preliminary ideas into solid, organic definitions.

In the nineteenth century scientists believed, just as firmly as Descartes two centuries earlier, that the rational foundations of mechanics were unshakeable. Even obscure notions like force were thought to designate definite objects, without conceptual mediation. Other notions, such as work and energy, were then derived from these elementary notions: Work (or energy), for example, was defined as the product of a force and the distance thorugh which it operates. This manner of construing energy conformed to the then prevalent analytical and Cartesian ideal. Note in passing that the absolute separation of space and time lent itself to such an intuitive, analytical approach, even though a good many philosophical difficulties persisted, for example, the discrepancy between the static and dynamic conceptions of force. In exploring such difficulties, scientists discovered any number of obscure points in the original definitions of such concepts as force, work, energy, and power and realized that various ambiguities had been carried over from the prescientific era. Indeed, it was realized that there can be no precise definition that fails to relate force to one of its fundamental properties, which is the ability to do work. From a modern standpoint, the essential relationship between the two notions, force and work, is obvious. The reciprocal relationship between force and energy has become increasingly apparent. What will the basic notion ultimately be? It is of course too early to give an answer to this question. Quantum theory may yet end the debate in an unexpected way by disclosing new principles on which to base the mathematical definitions of hitherto empirical notions. In fact, if we look carefully at London and Heitler's ideas concerning the possible interactions of two hydrogen atoms, it becomes clear that in "microenergetics" the tendency is to view force as a derivative notion, a secondary phenomenon a kind of conventional special case. Without defining energy in terms of more or less hypothetical forces, Heitler and London begin by writing energy equations for the two-atom system. By then applying the Pauli exclusion principle to the system, they discover that there are two possible energy states. If when the distance between the two nuclei is decreased the energy of the system increases, then we *say* that the nuclei repel one another; if, on the other hand, the energy of the system decreases, then we *say* that the nuclei attract. Thus what had seemed to be clearly phenomenal characteristics of the problem, such as whether the nuclei attract or repel one another, here become matters of definition. The idea of force has no absolute basis, and in this analysis it ceases to be a primitive notion. Let us carry this line of argument one step further. It is clear that attraction can occur only between two atoms differentiated according to the Pauli principle, and that the possibility of elastic collision, which used to be explained as the result of a repulsive force inherent in the two nuclei, is an attribute of the system in which the two atoms are not differentiated according to the Pauli principle. Thus it seems that attraction occurs between "different sets of quantum numbers" and repulsion between "like sets of quantum numbers." The force that results from this mathematical analysis is a mere phantom of the force that realist metaphysics imagined as the basis of the notion of energy. On this view, mechanical force becomes as much of a metaphor as the force of sympathy or

antipathy. It is a property of a composite, not of the elements that enter into that composite. Mathematical intuition, with its concern for completeness, replaces empirical intuition with its arbitrary simplifications.

I believe, in short, that science is beginning to base its reasoning on tentative, complex models, and that the idea of simplicity is reserved for specific, and always provisional, purposes. The desire to leave the canons of explanation open reveals the receptive psychology of modern science. Some new combination of experiments may lead to alteration of the fundamental postulates. As Cabrera wrote in 1928, "we are not yet . . . in a position to say whether quantum mechanics, created to interpret the radiation of isolated atoms, is an adequate tool to explore the far more complicated problem of molecular dynamics. It is possible and, as I believe, highly probable that a new assumption will have to be added to those already in place. In any case, we must keep our minds open to this possibility."[10] Thus mathematical physics is subject to the same anxiety as geometry: There is always the fear that a new postulate may split the science in two. The attitude that we must remain always in doubt of knowledge that had seemed certain in the past extends and indeed transcends Cartesian precaution and is truly worthy of being called non-Cartesian, as long as we remember that non-Cartesian philosophy complements Cartesian philosophy without contradicting it.

Similarly, as I have tried to show in my book *Le pluralisme cohérent de la Chimie moderne (The Coherent Pluralism of Modern Chemistry)*, chemistry has finally succeeded in es-

[10] Ibid., p. 247.

tablishing itself on a firm rational and mathematical footing by systematically extending its pluralist outlook. The way to rationalize the world of matter, it turns out, is to complete it.

Thus it is awareness of totality that animates work in both mathematical physics and pure mathematics. This accounts for the importance of group theory in both disciplines. The mind cannot rest easy until group theory has placed its seal of completeness on any particular construction. In an essay on the work of Laguerre,[11] Poincaré alludes to its novel non-Cartesian character. When Laguerre published his first paper in 1853, analytical geometry, according to Poincaré,

> was making itself over . . . by means of a revolution in some sense the opposite of the Cartesian reform. Before Descartes, chance alone, or genius, made possible the solution of a geometric problem. After Descartes we were in possession of infallible rules for obtaining results. To be a geometer it was enough to be patient. But a purely mechanical method that asks nothing of the mind by way of invention cannot be really fruitful. A new reform was therefore necessary. Poncelet and Chasles were its initiators. Thanks to them, we no longer need rely on either tireless patience or a stroke of good fortune for the solution to a problem, but rather on profound knowledge of the mathematical facts and their intimate relations.[12]

The method of Poncelet, Chasles, and Laguerre was thus a method of discovery more than a method of solution. It was clearly synthetic in nature and, as Poincaré points out, op-

[11] Edmond Laguerre (1834–1886), French mathematician.
[12] Poincaré, *Savants et ecrivains*, p. 86.

posite in intent to the Cartesian reform. Thus in some respects it marks the end of Cartesian thought in mathematics.

V

When one truly appreciates how far modern mathematics has surpassed the primitive science of spatial measurement and how much the "science of relations" has grown in recent years, it becomes clear that mathematical physics is each day opening new avenues of scientific objectification. Only after we have realized this does the stylized nature that scientists, with the aid of mathematics, create in the laboratory begin to seem less opaque than the nature that offers itself immediately to our observation. Conversely, once objective thought has been educated through the study of an organic nature of some sort, it reveals itself to be remarkably deep, for the simple reason that it is perfectible, correctable, and suggestive of new theories. The best way for a thinking subject to deepen its thoughts is still by meditating upon an object (as Decartes by his hearth meditated, in the *Discourse on Method,* upon his piece of wax — trans.). But rather than follow the example of the metaphysician who sits down at his hearth, one would do well to follow the mathematician who heads for his laboratory. Before long, every physics and chemistry laboratory may bear over its entrance the Platonic caveat, Let no one enter here who is not a geometer.

By way of illustration, let us compare Descartes's observation of his piece of wax with what I shall call the wax-drop experiment (an imaginary experiment that Bachelard invents to illustrate the use of careful modern techniques — trans.).

For Descartes the ball of wax was a clear symbol of the fleeting character of material properties. Simply by placing it near the fire he was able to alter or transform its consistency, its shape, its color, its feel, and its smell — an experiment that to us may seem rather crude but to Descartes proved the ambiguity of so-called objective qualities. Here is a lesson in doubt. Its point is to alienate the mind from experimental knowledge of substances, which are harder to know, Descartes tells us, than the soul. If the understanding were unable to find within itself the basis for the science of extended substances, then the substance of the ball of wax would evaporate along with the reveries of the imagination. It is only *intelligible* extension that subsists in the wax, since even its size is subject to increase or decrease depending on the circumstances. Descartes' refusal to base thought on experience is in fact final, even though he does ultimately return to the study of extended substance. From the first, however, he rules out any possibility of what I shall call progressive experimentation, any means of classifying or measuring the diversity of what is observed, any way of fixing the variables of the phenomenon in order to distinguish one from another. Descartes's desire was to apprehend directly the object's simplicity, unity, and constancy, and at the first sign of failure he was plunged immediately into doubt of *everything*. He failed to see the coordinating possibilities in directed experimentation and did not recognize how theory combined with experiment might restore the organic, and hence entire and complete, character of the phenomenon. What is more, by refusing to submit docilely to the lessons of experience, he condemned himself to overlook the fact that the variability of objective observation is immediately reflected in a corresponding

mobility of subjective experience. If the wax changes, I change; I change with my sensation, which is, in the moment I conceive of it, the entire content of my thought; for to feel is to think in the broad sense that Descartes attaches to the *cogito*. But Descartes has a secret confidence in the reality of the soul as substance. Dazzled by the sudden light of the *cogito*, he never doubts the permanence of the *I* that is the subject of *I think*. Why is it the same being who feels first the hard wax and then the soft wax, when it is not the same wax that is felt on the two occasions? If the *cogito* were recast in the passive voice as *cogitatur ergo est*, would the active subject vanish along with the inconstancy and vagueness of its impressions?

This Cartesian partiality in favor of subjective experience will be all the more evident, perhaps, when we learn to bring more fervor to objective scientific experiment, to measure precisely the limits of our thoughts, and to match, in a strict and rigorous manner, thought to experiment, noumenon to phenomenon, rather than allow ourselves to be misled by the deceptive appearance of substances both subjective and objective.

Let us look, therefore, at how modern science goes about its business of progressive objectification. A modern physicist working with a ball of wax would not start with bees' wax straight from the hive but with chemically pure wax produced by careful purification techniques. The wax used is therefore in one sense a specific *moment* of a "method of objectification." It retains no trace of the fragrance of the flowers that entered into its composition, but it does bear the marks of the careful process of purification to which it was subjected. It is, in a manner of speaking, the product of artificial experience. Without an artificial experience of this kind, such a ball of wax — pure wax, not in its natural form — would never have come into existence.

The physicist would then melt this wax in a crucible and resolidify it in a slow, methodical manner. He can precisely control the rate of melting and solidification by using a small electric oven whose temperature can be regulated by adjusting the supply of electrical power. Thus the physicist gains *control of time*, the time during which the action of the heat affects the composition of the wax. In this way he can obtain a wax "droplet" whose shape and surface composition can be precisely controlled. Now that the "book of the microcosmos" is engraved, as it were, it remains to be read.

In order to study the surface of the wax, the physicist might expose it to a monochromatic beam of X-rays; he would do this in a very careful way, of course, and would never think of using "natural" white light, which in prescientific ages was thought to be of a simple nature. Thanks to the slow cooling of the ball of wax, the surface molecules will be oriented in a precise way relative to the surface of the drop. This orientation will determine the diffraction pattern of the X-rays and yield spectrograms similar to those obtained by Debye and Bragg for crystals. As is well known, crystal spectrograms, whose existence was predicted by von Laue, have given new life to the discipline of crystallography by enabling scientists to deduce the internal structure of various crystals. Similarly, our study of the wax drop may give us new knowledge of the surface structure of this form of matter. This new way of "reading" matter can be highly instructive. As Trillat points out, "orientation phenomena . . . are responsible for many surface properties, such as capillarity, oiliness, adherence, adsorption, and catalysis."[13] The outer film of a substance determines its relations with the outside world, a whole new realm for physicial chemistry to explore. It is by attending to this new realm that the metaphysician can best understand the influence of structure. We can examine the orientation of the molecule at various depths below the surface of the wax droplet. It turns out that the orientation gradually disappears as we go deeper and deeper into the interior; the microcrystals become less and less sensitive to the surface action, and beyond a certain depth there is complete statistical disorder. In the orientation zone, however, we observe an interesting set of phenomena having to do with the discontinuity of the molecular fields at the separation surface — the zone of material dialectic, as it were. In this intermediate region, various interesting experiments reveal an interplay between the physical and chemical properties of the wax and enable the physicist to alter its *chemical nature*. Trillat, for example, has reported on experiments pertaining to the stretching of colloidal gels. Using purely mechanical means to stretch the gel can cause marked differences in the X-ray diffraction patterns. Trillat's conclusion is as follows: "This pertains to the mechanical properties (of the gel) and also to the adsorption of dyes, according as the matter is oriented by traction or not: this may be an unsuspected way of affecting chemical activity."[14]

To affect chemical activity by mechanical means is in certain respects in keeping with the Cartesian ideal. But here

[13] Jean Trillat, "Etude au moyen des rayons X des phénomènes d'orientation moléculaire dans les composés organiques," *Activation et structure des molecules*, 1928, p. 461.
[14] Ibid., p. 456.

the artificial, constructive intent of the experiment, its impulse to greater and greater complexity, is so clear that it can only be regarded as yet another proof of the way science has extended our possibilities of experience, and as yet another instance of the non-Cartesian dialectic.

Is it even clear that crystallization can take place in the absence of ambient fields? The idea that crystallization is essentially the result of internal forces stemming from the substance itself and that it is possible to neglect outside influences is based on realist presuppositions. Indeed, it is striking to discover that surface crystallization depends on field discontinuities to such an extent that one can speak of substances that are superficially crystallized in the direction perpendicular to the surface while remaining amorphous in the direction parallel to the surface. The resulting structure is rather grasslike in structure, with the "blades" of grass implanted in the amorphous body of the substance in a definite manner. This new type of crystalline "growth" has already yielded considerable information about molecular structures.[15]

Anyone who is willing to recognize the importance of the new techniques, hypotheses, and mathematical theories that are involved in our proposed study of the drop of wax cannot fail to acknowledge that Descartes's metaphysical critiques have lost their edge. What is fleeting is not, as Descartes thought, the properties of the wax but the haphazard circumstances surrounding his observation of it; modern science coordinates its observations in its search for the qualities of matter. In nature the conditions of observation are *confused*, and all one has to do is put some order into the process in order to bring organization to the real. For science, then, the qualities of reality are functions of our rational methods. In order to establish a scientific fact, it is necessary to implement a coherent technique. Scientific work is essentially complex. Science is a discipline of active empiricism, which, rather than rely on whatever clear truths happen to lie ready to hand, actively seek its complex truths by artificial means. Innate truths naturally have no place in science. Reason has to be shaped in the same way as experience.

[15] See Jean Thibaud, "Etudes aux rayons X du polymorphisme des acides gras," *Activation et structure des molecules*, pp. 410ff.

"Objective meditation" in the laboratory commits us to a path of progressive objectification that gives reality to both a new form of experience and a new form of thought. Subjective meditation is bent on attaining clear and definitive knowledge; objective meditation differs from this by the very fact that it makes progress, by its intrinsic need always to go further, to extend the limits of the known. The scientist, when he has done with his days' objective meditation, has his program of research for the following day in hand, and at the end of each working day he repeats the following article of faith: Tomorrow I shall know the truth.

VI

Looking now at the problem of scientific innovation from the psychological standpoint, it seems certain that the revolutionary character of modern science will have profound effects on the structure of the scientific spirit. Now, to say that the structure of the scientific spirit changes is just another way of saying that knowledge has a history. Human history, with all its passions and prejudices and its dependence on impulses of the moment, may well be a theater of eternal recurrence. But as history moves forward, there are some ideas that are not simply repeated; these are ideas that have been rectified, enlarged, completed, ideas that have definitively outstripped the limited and shaky principles on which they may once have been based. Now, the scientific spirit is essentially a way of rectifying knowledge, a way of broadening the horizon of what is known. Sitting in judgment, it condemns its historic past. Its structure is its awareness of its historical errors. For science, truth is nothing other than a historical corrective to a persistent error, and experience is a corrective for common and primary illusions. The intellectual life of science depends dialectically on this differential of knowledge at the frontier of the unknown. The very essence of reflection is to understand that one did not understand before. The non-Baconian, non-Euclidean, and non-Cartesian philosophies are historical dialectics that grew out of the correction of an error, the extension of a system, or the completion of an idea.

In order for the new scientific spirit to take on the same formative value as a new economic policy,[16] all that is needed is a little social life, a little human sympathy. For many scientists, who passionately lead the dispassionate life, the resolution of today's scientific problems will determine the future of reason itself. Reichenbach has spoken, rightly I think, of a generational conflict over the deep meaning of science.[17] While visiting J. J. Thomson at Cambridge, Karl Compton happened to meet G. P. Thomson, the elderly

[16] The French makes clear that Bachelard is here alluding to the Soviet Union's New Economic Policy. — Trans.
[17] Reichenbach, *La philosophie scientifique*, pp. 23–24.

physicist's son, who had come up for the weekend. The three men spent some time examining photographs taken by means of electron waves, about which Compton made the following remark: "It was truly a dramatic event to see the grand old man of science, who had spent the best years of his life arguing that the electron is a particle, full of enthusiasm for the work of his son, which revealed that electrons in motion are in fact waves."[18] The distance traveled from father to son is a measure of the philosophical revolution entailed by the abandonment of the notion that the electron is a thing. The intellectual courage required for such a revision in our realist principles commands our admiration. Physicists have been obliged, three or four times in the past twenty years, not simply to change their minds but, intellectually speaking, to make a totally fresh start.

When, moreover, we realize what an incomplete state modern science is in, we begin to gain some intimate idea of the meaning of "open-minded rationalism" (*le rationalisme ouvert*). To be rational and yet open-minded is to experience genuine surprise at the implications of theoretical speculation. Juvet puts it quite well: "The surprise created by a new idea or association of ideas is surely the most important element in the progress of the physical sciences, for it is astonishment that excites logic, which is always rather cold, and that forces scientists to make new connections. But the ultimate cause of progress, the reason for our surprise itself, has to be sought in the force fields that new associations of ideas set up in our minds, fields whose strength measures

[18] Hans Reichenbach, *Scientifische Monatschrift*, 1929, vol. 28, p. 301. Cited by Haissinsky, p. 348.

the good fortune of the scientists lucky enough to bring those ideas together."[19]

Confronted with the surprising principles of the new quantum mechanics, even Emile Meyerson, who expended such vast quantities of meditation and erudition to prove that relativity was in fact classical physics, has suddenly been gripped by doubt. There is reason to doubt that anyone will ever write a *Quantum Deduction* to complete the proof that Meyerson began in his *Relativistic Deduction*. As he himself confesses, "compared with the theories I have examined in my books, quantum theory admittedly occupies a place apart, and it does not seem possible, in my view, to attempt for it what I believe I have accomplished for the theory of relativity."[20] For Meyerson, quantum theory is essentially an aberration, and he is not far from claiming that the "arithmetization of the possible" is really an irrational doctrine. My view is quite the contrary: It is that the quantum theory extends, in a positive sense, our conception of the real and marks a triumph of the new reason over irrationalism. The mind must be made ready to receive the quantum idea, and this can be done only by systematically expanding the scientific spirit.

I further believe that relativity already marks the triumph of an eminently inductive theory, and that the success of efforts to deduce, for pedagogical purposes, certain consequences of relativity theory in no way diminishes the brilliance of Einstein's achievement or makes it any less surprising. The genius of de Broglie in founding wave mechanics and of Heisenberg in founding matrix mechanics has been

[19] Gustave Juvet, *La structure* (Paris: F. Alcan, 1933), p. 105.
[20] Meyerson, *Le cheminement de la pensée*, vol. 1, p. 67.

no less astonishing and no less historically unprecedented. These discoveries have relegated both classical and relativistic mechanics to the past, as two more or less crude approximations to more subtle and complete theories.

Will some still more general theory swallow up all these astonishing advances and establish itself as the immutable truth? Will yet another advance bring order to chaos and rule the universe? Doubtless it is Meyerson's profound wish that such will prove to be the case. When Meyerson shows how modes of thought endure for centuries and demonstrates the persistence of primitive ways of thinking in the most modern of minds, he draws the conclusion that the brain cannot evolve any more rapidly than any other organ. This argument of Meyerson's is obviously the argument of caution, and it would be hard to gainsay him without venturing the riskiest of speculations. Still, I shall try. For isn't the brain the true center of human evolution, the terminal bud of the vital spirit? With its manifold connections, is it not the organ of innumerable possibilities? When Juvet suggestively alludes to the "force fields" created in the mind by bringing together two different ideas, he encourages us to interpret the traditional association of ideas in a more dynamic light and to give to Fouillée's notion of an idea force an almost physical interpretation. An evolving idea is an organic center that swells to greater and greater proportions. A static brain would be a brain that never drew any new conclusions. In order to prove the continuity of the mind, are there no better arguments than commonplace, effortless thoughts, the thoughts that control our muscles and by so doing merge with what no longer evolves? If so, then everything is already complete: the soul, the body, the world itself, the world as it is given to us with its grand and noble features. The philosophers, for their part, hold out to us the idea of communion with an all-enveloping reality, to which the scientist can hope for nothing better than to return, as to a philosophy original and true. But if we really want to understand our intellectual evolution, wouldn't we do better instead to pay heed to the anxiety of thought, to its quest for an object, to its search for dialectical opportunities to escape from itself, for opportunities to burst free of its own limits? In a word, wouldn't we do better to focus on thought in the process of objectification? For if we do, we can hardly fail to conclude that such thought is creative.

The psychological advance brought about by mathematical physics has been described by Juvet. He points out that the boldest and most fruitful ideas have been the work of very young scientists: "Heisenberg and his pupil Jordan were born with the century. In England, an astonishing genius . . . Dirac developed a new and original method and discovered the deep theoretical reasons for what has been called the spin of the electron; he was only twenty-five years old at the time. If we recall also that Bohr was very young in 1913 when he proposed his model of the atom, and that Einstein was twenty-five when he discovered special relativity and shortly thereafter proposed for the first time an explanation of the laws of radiation for quanta of light . . . it seems reasonable to conclude that the twentieth century has witnessed a mutation in man's brain or mind of a sort apt to help him unravel the laws of nature, much as the precocity of the Abels, Jacobis, Galois, and Hermites of the previous century may have been due to a mutation of the mind apt to further the understanding of mathematical objects."[21]

Each of us can relive these intellectual mutations by thinking back to the turmoil and emotion that the new doctrines caused us personally when we first learned of them. They required such effort of learning that they did not seem natural. But *natura naturans* (to use Spinoza's term — trans.) is at work in our very souls; at some point we each realized that we had understood. By what light do we recognize the importance of these sudden syntheses? By an ineffable light that brings security and happiness to our minds. This intellectual happiness is the first sign of progress. This is the moment to recall, with Jean Hering, the phenomenologist, "that the most advanced person will always be able, thanks to his broader horizon, to understand his inferiors . . . whereas the reverse is impossible.[22] Understanding has a dynamic dimension; it is a spiritual élan, a vital élan. Einsteinian mechanics added to our understanding of Newtonian concepts. De Broglie's mechanics is adding to our comprehension of the concepts of classical optics and mechanics, which the new physics is molding into a new synthesis that extends and completes the epistemology of Descartes. If only we were capable of immersing ourselves in scientific research with all our strength and of studying our psychological development as we study other aspects of our cultural history, we would be able to feel the sudden animation that has been given to the soul by the creative syntheses of mathematical physics.

[21] Juvet, *La structure*, p. 134.
[22] Jean Hering, *Phénoménologie et philosophie religieuses* (Strasbourg, 1925), p. 126.