PHYSICS, COMMUNITY AND THE CRISIS IN PHYSICAL THEORY

We are in the midst of a restructuring of the physical sciences. Internally, they are stratifying into independent levels with stable basic principles; externally, budgets are shrinking and political objectives are changing.

Silvan S. Schweber

A deep sense of unease permeates the physical sciences. We are in a time of great change: The end of the cold war has ushered in an era of shrinking budgets, painful restructuring and changing objectives. At the same time, the underlying assumptions of physics research have shifted. Traditionally, physics has been highly reductionist, analyzing nature in terms of smaller and smaller building blocks and revealing underlying, unifying fundamental laws. In the past this grand vision has bound the subdisciplines together. Now, however, the reductionist approach that has been the hallmark of theoretical physics in the 20th century is being superseded by the investigation of emergent phenomena, the study of the properties of complexes whose "elementary" constituents and their interactions are known. Physics, it could be said, is becoming like chemistry.

Such observations, of course, are not new. In 1929, in the wake of the enormous success of nonrelativistic quantum mechanics in explaining atomic and molecular structure and interactions, Dirac, one of the main contributors to those developments, asserted in a famous quotation that "the general theory of quantum mechanics is now almost complete."[1] Whatever imperfections still remained were believed to be connected with the synthesis of the theory with the special theory of relativity. But these were "of no importance in the consideration of atomic and molecular structure and ordinary chemical reactions. . . . The underlying physical laws necessary for the mathematical theory of a large part of physics and the whole of chemistry are thus completely known, and the difficulty is only that the exact application of these laws leads to equations much too complicated to be soluble."

Forty years later, the head of the theory division at CERN, Léon van Hove, made similar comments. In an after-dinner speech entitled "The Changing Face of Physics," delivered during a Battelle Colloquium on phase transitions and critical phenomena held in Geneva in September 1970, he said[2] that it seemed that

> physics now looks more like chemistry in the sense that, in percentage, a much larger fraction of the total research activity deals with complex systems, structures and processes, as against a smaller fraction concerned with the fundamental laws of motion and interaction. This colloquium is a good example. Surely, we all believe that the fundamentals of classical mechanics, of the electromagnetic interaction, and of statistical mechanics dominate the multifarious transitions and phenomena you discuss this week; and I presume that none of you expects his work on such problems to lead to modifications of these laws. You know the equations better than the phenomena. . . . You are after the missing link between them, i.e., the intermediate concepts . . . which should allow a quantitative understanding and prediction of phenomena.

Van Hove was pointing to an important transformation that had taken place in physics: As had previously happened in chemistry, an ever larger fraction of the efforts in the field were being devoted to the study of novelty rather than to the elucidation of fundamental laws and interactions. Van Hove could have elaborated his picture of chemistry further by pointing to its predominantly applied and utilitarian concerns and to the close ties that academic chemistry has traditionally had with industry. The same was becoming true for physics, and in fact for most of the sciences.

The present situation is in part a consequence of the enormous success of quantum mechanics and quantum field theory and of the theoretical physics based on them since World War II.

World War II was a watershed for physics. It gave physicists the opportunity to display their powers. They emerged from the war transformed, with the state recognizing their value for its security and its power.[3] The enormous expenditures for research and development

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during World War II and the lucrative support of science by the government after the war brought about a revolution in the physical sciences. The revolution was driven principally by novel techniques and instruments: microwave generators and detectors; nuclear reactors; cyclotrons, synchrotrons and linear accelerators; transistorized electronics and computers; nuclear magnetic resonance devices; cryostats; masers and lasers. All these changed the practice of physics and chemistry.

Building on the base laid by quantum mechanics, physicists also transformed the theoretical foundations. That transformation culminated in a new understanding of condensed matter physics and the establishment in high-energy physics of what is now called the standard model. These conceptual developments in fundamental physics have revealed a hierarchical structure of the physical world. Each layer of the hierarchy is successfully represented while remaining largely decoupled from other layers.[4] These advances have supported the notion of the existence of objective emergent properties and have challenged the reductionist approach. They have also given credence to the notion that to a high degree of accuracy our theoretical understandings of some of these domains have been stabilized, since the foundational aspects are considered known. Let me amplify these remarks.

The reductionist approach that has been the hallmark of theoretical physics in the 20th century is being superseded by the study of emergent phenomena

Reduction and unification

The revolutionary achievements in physics during the period from 1925 to 1927 stemmed from the confluence of a theoretical understanding, the representation of the dynamics of microscopic particles by quantum mechanics, and the apperception of an approximately stable ontology (embodying the notion that all atoms, molecules, solids and so on are built out of electrons and nuclei). By "approximately stable" I mean that these particles (electrons and nuclei) could be treated as *ahistoric* objects, whose physical characteristics were seemingly independent of their mode of production and whose lifetimes could be considered as essentially infinite. One could assume these entities to be essentially "elementary" point-like objects, each species specified by its mass, spin, statistics (bosonic or fermionic) and electromagnetic properties such as its charge and magnetic moment.

The success of quantum mechanics at the atomic level immediately made it clear to the more perspicacious physicists that the laws behind the phenomena had been apprehended, that they could therefore control the behavior of simple microscopic systems and, more importantly, that they could create new structures, new objects and new phenomena.[5] Thus already in 1928 John Slater had a vision of a molecular engineering based on quantum mechanics. Condensed matter physics has indeed become the study of systems that have never before existed. Phenomena such as superconductivity are genuine novelties in the universe.

Quantum mechanics reasserted that the physical world presented itself hierarchically. The world was not carved up into terrestrial, planetary and celestial spheres but layered by virtue of certain constants of nature. As Dirac emphasized in the first edition of *Principles of Quantum Mechanics*, Planck's constant allows us to parse the world into microscopic and macroscopic realms, or more precisely into the atomic and molecular domains and the macroscopic domains composed of atoms and molecules. The story repeated itself with the carving out of the nuclear domain: quasistable entities—neutrons and protons—could be regarded as the building blocks of nuclei, and phenomenological theories could account for many properties and interactions of nuclei.

During the 1970s the establishment of the standard model description of the entities that populate the subnuclear worlds—quarks, gluons and leptons—marked the attainment of another stage in the attempt to give a unified description of the forces of nature. The program traces its origins to Newton and his realization of the universality of gravitation. Different aspects of the program were anchored during the 19th century. Oersted and Faraday gave credibility to the quest by providing the first experimental indications that the program of unification had validity. Maxwell constructed a model for a unified theory of electricity and magnetism and provided a mathematical formulation that explained many of the observed phenomena and predicted new ones. Joseph von Fraunhofer and others demonstrated that the laws of physics discovered here on Earth also apply to stellar objects. With Einstein the vision became all-encompassing. Einstein advocated unification coupled to a radical form of theory reductionism. In 1918 he said, "The supreme test of the physicist is to arrive at those universal elementary laws from which the cosmos can be built up by pure deduction."

Unification and reduction are the two tenets that have dominated fundamental theoretical physics during the present century. One characterizes the hope of giving a unified description for all physical phenomena; the other the aspiration to reduce the number of independent concepts necessary to formulate the fundamental laws. The conceptual or idealistic component in reductionism takes precedence over the materialistic one of reducing the number of so-called elementary particles. The impressive success of the enterprise since the beginning of the century has deeply affected the evolution of all the physical sciences as well as that of molecular biology.

But ironically, the very concepts that helped physi-

cists achieve the important advances that led to the standard model have eroded these foundational tenets upon which the program is based. The ideas of symmetry breaking, the renormalization group and decoupling suggest a picture of the physical world that is hierarchically layered into quasiautonomous domains, with the ontology and dynamics of each layer essentially quasistable and virtually immune to whatever happens in other layers. At the height of its success, the privileged standing of high-energy physics and the reductionism that permeated the field were attacked.

Anderson's attack

In 1972 Philip Anderson, one of the foremost condensed matter physicists, challenged the radical theory reductionist view held by the majority of elementary-particle physicists. His criticism was not only an attack on the philosophical position of the high-energy physicists; it also challenged their dominance within the physics community and within the councils of state. In an article entitled "More is Different,"[6] Anderson asserted that

> the reductionist hypothesis does not by any means imply a "constructionist" one: The ability to reduce everything to simple fundamental laws does not imply the ability to start from those laws and reconstruct the universe. In fact, the more the elementary-particle physicists tell us about the nature of the fundamental laws, the less relevance they seem to have to the very real problems of the rest of science, much less to those of society. The constructionist hypothesis breaks down when confronted with the twin difficulties of scale and complexity.

Anderson believes in emergent laws. He holds the view that each level has its own "fundamental" laws and its own ontology. Translated into the language of particle physicists, Anderson would say each level has its effective Lagrangian and its set of quasistable particles. In each level the effective Lagrangian—the "fundamental" description at that level—is the best we can do. But it is not enough to know the "fundamental" laws at a given level. It is the *solutions* to equations, not the equations themselves, that provide a mathematical description of the physical phenomena. "Emergence" refers to properties of the solutions—in particular, the properties that are not readily apparent from the equations.

Moreover, the behavior of a large and complex aggregate of "elementary" entities is not to be understood "in terms of a simple extrapolation of the properties of a few particles."[6] Although there may be suggestive indications of how to relate one level to another, it is next to impossible to *deduce* the complexity and novelty that can emerge through composition. The study of the new behavior at each level of complexity requires research that Anderson believes "to be as fundamental in its nature as any other." Although one may array the sciences in a roughly linear hierarchy according to the notion that the elementary entities of science X obey the laws of science Y one step lower, it does not follow that science X is "just applied science Y." The elementary entities of condensed matter physics obey the laws of elementary-particle physics, but condensed matter physics is not just "applied elementary-particle physics," nor is chemistry applied many-body physics, *pace* Dirac. In his article Anderson sketched how the theory of broken symmetry helps explain the shift from quantitative to qualitative differentiation in condensed matter physics and why the constructionist converse of reductionism breaks down.

Developments in quantum field theory and in the use of renormalization-group methods have given strong support to Anderson's views. The insights from the renormalization-group approach and from the effective field theory method have also greatly clarified why the description at any one level is so stable. In fact, renormalization-group methods have changed Anderson's remark "the more the elementary-particle physicists tell us about the nature of the fundamental laws, the less relevance they seem to have to the very real problems of the rest of science," from a folk theorem into an almost rigorously proved assertion.

Effective field theories

When renormalization was first developed, after World War II, it was regarded as a technical device to get rid of the divergences in perturbation theory and seemed to be a remarkably effective way of sweeping the problems under the rug. Renormalizability became a criterion for theory selection.[7] Only renormalizable, local, relativistic quantum field theories were adopted for the representation of the interactions of what were then thought to be the "elementary particles." Note the cunning of reason at work: The divergences that had previously been considered a disastrous liability now became a valuable asset. However, the question of why nature should be described by renormalizable theories was not addressed. Such theories were simply the only ones in which calculations could be done.

'The ability to reduce everything to simple fundamental laws does not imply the ability to start from those laws and reconstruct the universe'—Philip Anderson

The impressive advances of the late 1960s and early 1970s that culminated in the standard model were a triumph of renormalization theory. In the lecture he delivered in 1979 in Stockholm when he received the Nobel Prize in Physics, Steven Weinberg stressed that "to a remarkable degree, our present detailed theories of

elementary-particle interactions can be understood deductively, as consequences of symmetry principles and of the principle of renormalizability which is invoked to deal with the infinities."[8] Relativistic local fields were once again the "fundamental" entities, but now gauge symmetry and renormalizability, together with the insights derived from spontaneously broken symmetries of the Goldstone–Higgs variety, were the guiding principles.

But by the end of the 1970s the cunning of reason once again had partially undermined the triumph. In the early 1950s Ernst Stueckelberg, Andreas Petermann, Murray Gell-Mann and Francis Low[9] had made a fundamental observation regarding the breakdown (due to renormalization) of naive dimensional analysis in quantum field theory. The importance of that observation was not realized by the community until the early 1970s, when Kenneth Wilson made manifest the implications of Gell-Mann and Low's paper. Wilson had derived important insights from the work of condensed matter physicists attempting to explain phase transitions—in particular the research of Lars Onsager, Benjamin Widom, Leo Kadanoff and Michael Fisher—and from the work of axiomatic field theorists who had elucidated the mathematical problems encountered in analyzing the properties of products of field operators. Wilson's work made clear that renormalization was not a technical device to eliminate the divergences "but rather is an expression of the variation of the structure of physical interactions with changes in the scale of the phenomena being probed."[10]

The renormalization-group method made it clear that one must take seriously the energy cutoff Λ that is introduced in the regularization method that renders a relativistic quantum theory meaningful. It was then realized that the physics that is observed at accessible energies (of order E) can be described, neglecting terms of order $(E/\Lambda)^2$, by an "effective" field theory in which the interaction terms are finite in number and limited in complexity. In the effective field theory—the one that essentially any theory reduces to at sufficiently low energies[11]—the nonrenormalizable interaction terms (which are consistent with the symmetries of the interactions) are all suppressed by powers of 1 over the cutoff, and hence negligible. Now once one accepts nonrenormalizable interactions, the key issue in addressing a theory like quantum electrodynamics "is not whether it is or is not renormalizable, but rather how renormalizable is it?"[12] How large are the nonrenormalizable interactions in the "effective" theory? What range of energy scales are well described by the degrees of freedom that one keeps in the theory? Renormalizability, it turns out, has to do with the range in energy over which the theory is valid.

In condensed matter physics the notions of an effective description and an effective interaction predated the renormalization-group approach. Landau had emphasized those concepts in areas as diverse as the quasiparticle theory of helium and the Landau–Ginzburg–Abrikosov theory of superconductors. Anderson took the same point of view in his work. John Bardeen, Leon Cooper and Robert Schrieffer showed how an effective Hamiltonian approach could explain the behavior of superconductors. All this was known to well-trained condensed matter physicists by 1960.

'To a remarkable degree, our present detailed theories of elementary-particle interactions can be understood deductively, as consequences of symmetry principles and renormalizability'—Steven Weinberg

The concept of "universality"—the related notion that the long-wavelength behavior is independent of small-distance behavior—predates the publication of Wilson's work on the renormalization group. A. A. Migdal and Alexander Polyakov used the term to describe the results of field theoretic calculations in the late 1960s. The idea played a major role in the phenomenological explorations of critical phenomena in the 1960s and was present in some form in the work of Cyril Domb, Fisher and their associates at King's College, and it was explicitly referred to in the 1967 review paper[13] by Kadanoff and coworkers. In the late 1960s most workers in critical phenomena knew and accepted the view that much of the macroscopic behavior was quite independent of the microscopic forces. Wilson's brilliant insights systematized the approach. In condensed matter physics, renormalization-group methods were not the source of universality; in fact, the field provided the examples that were responsible for the further development of the methods.[14]

Decoupling

Renormalization-group methods in condensed matter physics gave new insights into why the details of the physics of matter at microscopic length scales and high energy are inconsequential for critical phenomena. What is important is the symmetry involved, the conservation laws that hold, the dimensionality of space and the range of the interactions. The method is not restricted to critical phenomena. It has been extended to show that for a many-body system one can, by integrating out the short-wavelength, high-frequency modes (which are associated with the atomic and molecular constitution), arrive at a hydrodynamical description that is valid for a large

class of fluids, and which is insensitive to the details of the atomic composition of the fluid. The particulars of the short-wave (atomic) physics are amalgamated into parameters that appear in the hydrodynamic description. Those parameters, such as density and viscosity, encapsulate the ignorance of the short-distance behavior. The physics at atomic lengths—and *a fortiori* high-energy physics—has become decoupled. (David Nelson emphasized these points in a roundtable discussion with Weinberg on "What is fundamental physics?" that was held in the department of the history of science at Harvard University in May 1992.)

High-energy physics and condensed matter physics have become essentially decoupled in the sense that the existence of a top quark, or any new heavy particle discovered at CERN or elsewhere, is irrelevant to the concerns of condensed matter physicists—no matter how great their intellectual interest in it may be. The same is true to some extent in nuclear physics. This fragmentation has resulted in the exploration and conceptualization of the novelty capable of expression in the aggregation of entities in each level—novelty that is evidently contained in the "fundamental laws" (the effective Lagrangian) at that level and that does not challenge their "fundamental" character. The challenge is how to conceptualize this novelty: That is what Anderson meant by "More is different."[6]

The statement that condensed matter and high-energy physics have become decoupled refers to the ontology that is used: Electrons and nuclei are the elementary particles of condensed matter physics, and the relevant features of the internal constitution of a nucleus is embodied in the (empirically determined) parameters stating its spin, magnetic moment, electric quadrupole moment and so on. Further detail is irrelevant for describing (at the usual level of accuracy) the phenomena probed by condensed matter physics.

There is, however, a great deal of exchange of ideas between the two fields. More than anything else, it was the importance of symmetry breaking, and in particular the beauty of the Higgs–Anderson mechanism of generating masses in massless gauge theories, that made the particle physicists recognize the significance of the insights and techniques of condensed matter physicists. Then Wilson's work on the renormalization group brought home to both communities the importance of mutual education. And indeed the cross-fertilization has been of great value and mutual benefit to both subdisciplines. The common pastures include the topics of scaling and renormalization, topological defects, two-dimensional models, Monte Carlo techniques, nonlinear σ models and much else.

The commonality of theoretical techniques used to address problems in what were different fields is a general phenomenon. Something similar is happening in areas of chemistry, meteorology and ecology, where the mathematics of dynamical systems has given deep new insights into such diverse phenomena as oscillating chemical reactions, the onset of turbulence and population dynamics. The computer has been central in this development. It has made "modeling" a new form of theoretical understanding and has allowed "visualization" of systems where nonlinearity is an essential feature. We can now deal with many phenomena that were not amenable to the analytical methods fashioned until World War II.

The interdisciplinary nature of the new communities studying these phenomena is also striking. The communities are held together not by paradigms but by tools: renormalization-group methods, nmr machines, lasers, neural networks, computers and so on. It is also noticeable that despite the apparent increase in specialization, the interconnectedness of science is becoming more prominent. Tools and concepts are constantly being carried from one field to another in ways that are difficult to anticipate by any logical and structural analysis.[15]

We need to reconceptualize the growth of scientific knowledge. The Kuhnian model will no longer do. The new model will have to take into account that something important has happened. A hierarchical arraying of parts of the physical universe has been *stabilized*, each part with its quasistable ontology and quasistable effective theory, and the partitioning is fairly well understood. For the energy scales that are experimentally probed in atomic, molecular and condensed matter physics the irrelevance (to a very high degree of accuracy) of the domains at much shorter wavelengths has been justified. Effectively a kind of "finalization" has taken place in these domains. The date when finalization occurred for nonrelativistic quantum mechanics can be taken to be 1957, when Bardeen, Cooper and Schrieffer explained superconductivity. (Until then, there was the nagging possibility that quantum mechanics might break down at distances above about 150 Å.)

Condensed matter physics provided the examples that were responsible for the further development of renormalization-group methods

Stabilization and community

The internal advances within physics that I have sketched have altered the traditional relationships among the various branches of physics. The shared commitment to unification and reduction that earlier had bound the various subdisciplines together has been substantially weakened. Moreover, these developments have taken place at a time when the various sub-branches confront severe problems. Together these two trends have produced a deep sense of unease that has permeated the discipline, and particularly the high-energy physics community.[16]

In the past the great successes of the program of reductionism and unification were taken as confirmation of the existence of causal connections between different layers in the physical world. Those successes also provided a justification for claiming the fundamental nature of high-energy physics activities and furnished a basis for arguing its relevance to other areas of science. But decoupling theorems and the effective field theory viewpoint have challenged these assumptions.

The sense of crisis in the high-energy community has been amplified by the disparity between the time scales for constructing new accelerators and detectors and for creating novel theories. There have essentially been no

major new experimental results since the early 1980s, when the discovery of the W's and the Z_0 corroborated the electroweak theory. Thus the empirical basis for new theoretical advances has been absent. The cancellation of the SSC implies that this may be the state of affairs for a long time to come. The termination of that project spells the end of an era for high-energy physics in the US.

Meanwhile, particle physics theorists have divided themselves into various camps: phenomenologists, effective field theorists, string theorists. String theorists represent the tradition of seeking unifying theories, and many of them share Dirac's view that theories ought to be beautiful. And everyone seems to believe that some of the most exciting aspects of high-energy physics are to be found in astrophysics. Nor has the condensed matter community been unaffected. The excitement generated by the solution of the problem of phase transitions has abated. The problems of explaining high-temperature superconductivity have proven more refractory than initially anticipated. The departure of a number of distinguished practitioners to such fields as biophysics and neural networks has not escaped notice.

The end of the cold war in the late 1980s exacerbated the unease. During the cold war, national prestige and national security could be invoked to justify the large outlays connected with experimental high-energy physics and other "fundamental" areas. But the new realities have called into question the assumption that society will continue to give high-energy physicists, cosmologists and astrophysicists generous support simply because of their ability to produce exciting new knowledge that will enhance national prestige.

The sense of crisis has been widely noted. In the fall of 1992 *The New York Times* reported that some 800 applications had been received for a single tenure-track position in the physics department at Amherst College. PHYSICS TODAY in March 1992 (page 55) described the widespread concern about the tightening job market for physicists, in particular for new PhDs and postdocs. And the roundtable discussion featured in the February 1993 issue of PHYSICS TODAY (page 36) suggests that physics faces a situation as difficult as that during the early 1930s, in the depths of the Depression. Nor is the situation likely to improve in the foreseeable future. Universities are undergoing a structural change and are planning to cut the size of their tenured faculties. The industrial sector is cutting back its research activities. Government funding is slackening, and its emphasis is shifting to applied research that will enhance competitiveness and productivity. It is probable that the discipline will shrink sharply over the next decade or so.

The crisis is particularly acute for particle physics and cosmology. Those fields claim to provide, in the reductionist sense, an ultimate basis for our understanding of the physical world. The crisis manifests itself both at the cognitive and at the social level. Fundamental research in physics is driven by a passion to reveal the secrets of nature by probing deeper and deeper into the physical world. This passionate internal dynamic, which has constantly propelled physics toward an intellectual mastery beyond the creation of technologically exploitable knowledge, is a deeply imbedded social practice that has ancient roots in the religious sphere. But the modern pursuit is conducted within a society whose dominant conception of rationality follows the doctrine of instrumentalism: Truth is valued less than usefulness. The justification of a reductionist pursuit within such a society depends, by and large, on the relevance of such resource-expensive research (expensive in terms of both capital and talent) to the goals set by the society.

The conceptual dimension of the crisis has its roots in the seeming failure of the reductionist approach, in particular its difficulties accounting for the existence of objective emergent properties. Thus the formulation of the strong interaction as a non-Abelian gauge theory of quarks and gluons can be considered a "fundamental" description. But it has proven very difficult to derive from quantum chromodynamics—*without invoking any empirical data*—an effective chiral Lagrangian describing low-energy pion–nucleon scattering, or to deduce from QCD the binding energy of the deuteron and explain why it is so small. Judging from the results obtained in the mathematical description of phase transitions in various dimensions, ascertaining the properties of the solutions of the "fundamental" equations (to say nothing of obtaining actual solutions) is an extremely difficult mathematical task involving delicate limiting procedures. The expectations that had seemed firmly established by previous successes, namely further reductionism and further unification, have not been met thus far. In fact, this lack of success has, in some quarters, called into question unification and reductionism as a strategy for the community—and thus has undermined an important component of the value system shared by the community.

We need to reconceptualize the growth of scientific knowledge. The Kuhnian model will no longer do

At the social level, the cutbacks in Federal funding and the contraction of job opportunities in universities, national laboratories and industrial laboratories are demanding a deep and painful restructuring of the community. And for us in the United States this reassessment comes at a time when we have to face the cost of waging the cold war, a conflict that has left us almost bankrupt economically and in need of finding new bonds to hold the nation together.

Where we are

The huge success and the stability of the theoretical descriptions given by quantum mechanics and quantum field theory imply that most scientists are no longer testing the foundational aspects of the domains they work in. Therefore the goals of most of the scientific enterprise are no longer solely determined internally; other interests come into play. The scientific enterprise is now largely involved in the creation of novelty—in the design of objects that never existed before and in the creation of conceptual frameworks to understand the complexity and novelty that can emerge from the *known* foundations and ontologies. And precisely because we create those objects and representations we must assume moral responsibility for them.

I emphasize the act of creation to make it clear that

Modern research is conducted within a society whose dominant conception of rationality follows the doctrine of instrumentalism: Truth is valued less than usefulness

science as a social practice has much in common with other human practices. But there are important differences: In the physical sciences, nature places strong constraints on our experiments and means of observation and plays the role of ultimate arbiter. Stated slightly differently, the stabilities of the natural order studied by the physical sciences have a time scale that is "quasi-infinite" compared with most other time scales.

I believe that in the reconstruction we are engaged in we must accept that the separation between the moral sphere and the scientific sphere cannot be maintained. The history of the present century makes clear that we must reject instrumental rationality, the notion that control and usefulness should be the overriding criteria guiding our behavior. Similarly, the relativism of the postmodernist position poses dangers by allowing everyone to have his or her own criteria. Yet the stubborn question remains: How shall we determine the universal criteria that I believe we must have and commit ourselves to, criteria that transcendental philosophy had postulated as *a priori*? Part of the answer surely must come from the lessons we have learned from Darwin. On the one hand we have a moral responsibility for the future of our species—and in fact for all life forms on Earth—and on the other hand we have a moral responsibility to leave the future open—this in the face of having acquired the knowledge of how to affect our own evolution and evolution in general.

Let me conclude by coming back to fundamental physics. I believe that elementary-particle physics has a privileged position, in that the ontology of its domain and the order manifested by that domain refer to the building blocks of the higher levels. But though the domain may have a privileged status, the community investigating it is part of the collective human enterprise. That community must confront the implications of all-encompassing visions such as Einstein's and recognize their danger. Yet it must also recognize the potency of those visions and accommodate them to a more human scale.

I also believe that fundamental physics has a special role to play precisely because of its remoteness from everyday phenomena and its seeming lack of relevance to utilitarian matters—what its proponents after World War II called its purity. There ought to be a part of the scientific enterprise that does not respond easily to the demand for relevance. It has become clear that that demand can easily become a source of corruption of the scientific process. Elementary-particle physics, astrophysics and cosmology are among the few remaining areas of science whose advancement is determined internally, based on experimental findings within the field and on its own intrinsic conceptual structure. Particle physics and cosmology have not been "stabilized" and may never be. Scientists engaged in fundamental physics have a special role—and a special responsibility—as a *community* committed to the visions of Bohr and Charles Sanders Peirce. (For Bohr, the practice of science exhibited a commitment to an underlying moral order; Peirce's vision was of a community determining truth through consensus and asymptotically approaching "Truth.") Scientists constitute a model of what Jürgen Habermas has called a communicative community: one that exists under the constraint of cooperation, trust and truthfulness, and that is uncoerced in setting its goals and agenda.[17] That community is a guarantor that one of the most exalted of human aspirations—"to be a member of a society which is free but not anarchical"[18]—can indeed be satisfied.

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For the past few years I have collaborated closely with Tian Yu Cao and have had the benefit of his sharp critical faculties, vast erudition and impressive technical knowledge. This paper is a testimony of our constant dialogue.

References

1. P. A. M. Dirac, Proc. R. Soc. London, Ser. A **126**, 114 (1929).
2. Quoted in C. Domb, Contemp. Phys. **26**(1), 49 (1985).
3. For an entry into the enormous body of works on the physicists and World War II, see M. Fortun, S. S. Schweber, Social Studies of Science, Fall 1993, in press.
4. For a historical overview, see T. Y. Cao, S. S. Schweber, Synthèse, Fall 1993, in press.
5. S. S. Schweber, Hist. Studies Phys. Biol. Sci. **10**(2), 339 (1990). J. D. Bernal, *The World, the Flesh and the Devil,* Dutton, New York (1929).
6. P. W. Anderson, Science **177**, 393 (1972).
7. F. J. Dyson, report to the Oldstone Conf., April 1949; see S. S. Schweber, *QED and the Men Who Made It,* Princeton U. P., Princeton, N. J. (1994), p. 554.
8. S. Weinberg. Rev. Mod. Phys. **52**, 515 (1980).
9. E. C. G. Stueckelberg, A. Petermann, Helv. Phys. Acta. **26**, 499 (1953). M. Gell-Mann, F. Low, Phys. Rev. **95**, 1300 (1954). See also S. Weinberg, in *Asymptotic Realms of Physics: Essays in Honor of Francis E. Low,* A. H. Guth, K. Huang, R. L. Jaffe, eds., MIT P., Cambridge, Mass. (1983), p. 1.
10. D. Gross, in *Recent Developments in Quantum Field Theory,* J. Ambjorn, B. J. Durhuus, J. L. Petersen, eds., Elsevier, New York (1985), p. 151. For an overview of Wilson's contributions, see his Nobel lecture: K. Wilson, Rev. Mod. Phys. **55**, 583 (1983).
11. H. Georgi, in *The New Physics,* P. Davies, ed., Cambridge U. P., Cambridge, England (1989), p. 446.
12. P. G. Lepage, in *From Action to Answers,* Proc. 1989 Adv. Inst. in Elementary Particle Physics, T. DeGrand, T. Toussaint, eds., World Scientific, Singapore (1990), p. 483.
13. L. P. Kadanoff, W. Götze, D. Hamblen, R. Hecht, E. A. S. Lewis, V. V. Palciauskas, M. Rayl, J. Swift, D. Aspnes, J. Kane, Rev. Mod. Phys. **39**, 395 (1967).
14. A referee of this article stressed the points made in this and the previous paragraph. See also M. Dresden, in *Physical Reality and Mathematical Description,* C. P. Enz, J. Mehra, eds., Reidel, Dordrecht, The Netherlands (1985), p.133.
15. This point was made by Harvey Brooks already in 1973; see H. Brooks, Am. Sci. **75**, 513 (1987). B. L. R. Smith, *American Science Policy since World War II,* Brookings Inst., Washington, D. C. (1990).
16. S. S. Schweber, in *The Rise of the Standard Model,* Proc. June 1992 SLAC Conf., L. Brown, L. Hoddeson, M. Riordan, eds., Cambridge U. P., Cambridge, England, to appear in 1994. I also draw on a draft for a proposed workshop on the crisis in physics prepared by T. Y. Cao and S. S. Schweber.
17. R. J. Bernstein, ed., *Habermas and Modernity,* MIT P., Cambridge, Mass. (1985).
18. I. I. Rabi, in *The Scientific Endeavor: Centennial Celebration of the National Academy of Sciences,* Rockefeller Inst. P., New York (1963), p. 303. ■