



Type: Book Chapter

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Source: *By Study and Also By Faith, Volume 2*

Editor(s): John M. Lundquist and Stephen D. Ricks

Published: Provo, UT/Salt Lake City; Foundation for Ancient Research and
Mormon Studies/Deseret Book, 1990

Page(s): 438-455



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Is There a Cure for Authoritarianism in Science?¹

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It is a commonplace that "in our time . . . the sciences, physical and social, will be to an increasing degree the accepted point of reference with respect to which the validity (Truth) of all knowledge is gauged."² Yet, as Professor Nibley and others have warned, it would be a grave mistake to accept without reservations the hegemony of the sciences in the house of intellect.³ The widely held notion that science has delivered us an absolutely authoritative source of knowledge simply cannot withstand close scrutiny.

Nowhere is this more apparent than in the history of novel theories and experiments in science. Scientists with radically new ideas have difficulty getting an audience among their more orthodox brethren. Sometimes they are ignored or rejected because of personal animosities or simple inertia. In other cases, the rejection seems to violate the canons of open-minded scientific inquiry. Through the whole spectrum of the sciences, one can document an astonishing disregard for facts which contradict fashionable theories, stereotyping of acceptable approaches to

This essay originally appeared in a slightly different form in the unpublished "Tinkling Cymbals: Essays in Honor of Hugh Nibley," John W. Welch, ed., 1978.

problems and theories, and the waving of academic credentials and ritual invocation of the specialist's mystique to discourage criticism from "outsiders."⁴ In these instances, the intellectual conservatism of the scientific community appears to be *authoritarian* rather than *authoritative* in character.

The occurrence of authoritarian behavior patterns appears at first glance to be completely pathological in view of our idealization of science as an objective inquiry after "stubborn irreducible facts." But the personal vanities and insecurities of individual scientists cannot reasonably be invoked to explain widespread authoritarianism in science. Moreover, since the stigmata of rigidity and dogmatism are observable in physics as well as archaeology, the problem cannot arise simply from the peculiarities of individual disciplines, but must be connected with general features of science.

The difficulty lies with the presumed objectivity of scientific investigation.⁵ For *facts* are not normative in science – the *consensus* is. To achieve that consensus, the community of science is often forced to make subjective judgments about the relative weight to be given to data, methodology, theoretical elegance, and the credentials of scientists. This sense of the community may be either imposed in authoritarian fashion, or proposed on defensible scientific – and hence authoritative – grounds. But science will always be torn between loyalty to the discipline as it exists and to the ideal of progress, between the desire to *possess* the truth and the striving to *discover* it. Therefore, even though this fundamental tension may on occasion lead to authoritarian behavior, it cannot be eliminated without destroying an essential mechanism of scientific activity.

Theory as the Source of Facts

Almost any science textbook contains a statement to the effect that "experiment, rather than preconceived

ideas, are the ultimate authority in science."⁶ Because we have been convinced that "an hypothesis will be rejected if even a single known fact is at variance with it,"⁷ we tend to view the subordination of fact to theory as *prima facie* evidence of authoritarian, even antiscientific, attitudes.

Such a naive view grossly oversimplifies matters of fact. For all experimental data are, in N. R. Hansen's felicitous phrase, "theory-laden." Two people may experience the same photochemical reaction at the surface of the retina but *see* entirely different things.⁸ Thus, as Einstein said, "It is the theory which decides what we can observe."⁹ This is true even in so-called "crucial experiments" – which are supposed unambiguously to reject or falsify a given hypothesis, and thus give an authoritative *denial* of a theory. Philosophers and historians of science disagree sharply about the problems of defining such experiments.¹⁰ But in practice, the results of such an experiment are unlikely to win easy acceptance if they fail to match previous expectations.

The genesis of the special theory of relativity provides an instructive case history. In 1864, in his "Dynamical Theory of the Electromagnetic Field," James Clerk Maxwell proposed that electromagnetic waves were transmitted as "vibrations of an aethereal medium filling space and permeating matter."¹¹ However, the mechanical properties required of this "lumeniferous ether" were an embarrassment; worse yet, it defied all attempts even to verify its existence.

Finally, the American physicist Michelson devised an experiment which, it was hoped, would settle the issue once and for all. The basic idea was to measure the speed of light in two mutually perpendicular directions – parallel and perpendicular to the trajectory of the earth's orbit. The ether theory predicted that the two measurements would show a slight discrepancy (on the order of one part in a hundred million). By 1887, Michelson had perfected an

interferometer capable of measuring the anticipated effect.¹² But a series of extraordinarily careful measurements showed no detectable difference in the speed of light in the two directions.

Now if a crucial experiment were in fact an unambiguously authoritative way to resolve scientific controversy, Michelson and everyone else should have abandoned the ether theory. Instead, his reaction was that "since the result of the original experiment was negative, the problem is still demanding a solution."¹³ Most physicists agreed with him. Numerous hypotheses were put forward to explain the null result of the experiment without abandoning the ether, although they were never more than *ad hoc* proposals which could not be connected with more general principles.¹⁴

Then, in 1905, the most famous patent clerk in history proposed the special theory of relativity, which began with the *postulate* that the speed of light is constant in all frames of reference, thus neatly "explaining" the Michelson result. Furthermore, starting from this and two other similarly general postulates, Einstein was able to remove some mathematical inconsistencies in Maxwell's theory of moving charges and to cast into unified form the transformation equations of particle mechanics and the electromagnetic field.

Nevertheless, Einstein's paper was received skeptically rather than gratefully.¹⁵ And it would be a mistake to label this negative response as simple authoritarianism. On the contrary, it effectively demonstrates the impossibility of settling a scientific controversy by means of a single fact. The ether was only one facet of a theory of mechanics which had successfully explained everything from universal gravitation to the motion of a spinning top. And implicit in the modest form and tone of Einstein's paper was a demand for the drastic revision of the classical con-

cepts of space and time—a great weight to hang on the result of a single experiment.

Once special relativity was accepted as authoritative, however, physicists willingly based its validity solely on the null result of the Michelson-Morley experiments.¹⁶ In fact, when a later experimental test of the ether drift appeared to invalidate special relativity, H. A. Lorentz hastened to assure physicists that the experimental results only “indicate the existence of some unknown cause which it will be very important to discover . . . but I think . . . that relativity will be quite safe.”¹⁷ Eventually the results were found to arise from a systematic error—thus confirming Eddington’s dictum that “It is also a good rule not to put overmuch confidence in the observational results . . . until they are confirmed by theory.”¹⁸

Problems with Paradigms

During periods of “normal science,”¹⁹ the authoritative standard of scientific truth is not data, but the *paradigm*—a framework of validated theories, concepts, and methods of attacking problems which both guides the course of experiment and embodies the data it produces. But, as with the ether-drift experiment, even when a paradigm fails, it will not be torn down until a new one can be constructed. The new consensus is usually not achieved by gathering more data, or by “multiplying existing hypotheses beyond necessity,” but by finding a new way to see *existing* theory and experimental experience. This process of “scientific revolution” is, in Polanyi’s words, “the classical case of Poe’s *Purloined Letter*, of the momentous document lying casually in front of everybody, and hence overlooked by all.”²⁰

The difficulties of constructing a new paradigm are illustrated nicely by the quantum theory of light. In 1887, Heinrich Hertz found experimental confirmation for Maxwell’s conjecture that light was an electromagnetic wave.

Ironically, he also observed what we now know as one manifestation of the photoelectric effect—in which light rays eject electrons from the surfaces of some materials. Over a period of almost two decades, other experimenters after Hertz reported similar phenomena. But the data could not be explained by Maxwell's theory, nor, it seemed, by any other reasonable scheme, so the experiments were mostly ignored by theorists.²¹

In another of his famous trio of 1905 papers, Einstein proposed an heuristic explanation of the result, assuming for purposes of calculation that light waves behaved as particles when interacting with matter.²² However, he was not taken seriously, because experiments done early in the nineteenth century by Young, Fresnel, and Foucault had convinced physicists that light consisted of waves, not of particles.²³ Gradually, though, Einstein satisfied himself that the wave or particle character of light is not determined *a priori*, but is contingent upon the way in which the light is observed. And although he showed how the photoelectric effect and related phenomena could be explained by his theory, Einstein's arguments rested primarily on his emerging view of a fundamental duality in Nature—between waves and particles, matter and energy.²⁴ To Robert A. Millikan, accustomed to thinking of waves and particles as mutually exclusive entities, this duality

seemed completely unreasonable because it *apparently* ignored and indeed seemed to contradict all the manifold facts of interference and thus to be a straight return to the corpuscular theory of light. . . . I spent ten years of my life testing that 1905 equation of Einstein's, and contrary to all my expectations I was compelled in 1915 to assert its unambiguous experimental verification in spite of its unreasonableness.²⁵

However, even "unambiguous experimental verification" was not sufficient to establish the dual nature of light as a new paradigm. In 1916, for instance, Max Planck (who

had suggested the concept of energy quanta in 1900) nominated Einstein for membership in the Prussian Academy of Science with the caveat:

That he may sometimes have missed the mark in his speculations, as for example in his hypothesis of light quanta, cannot really be held too much against him. For it is not possible to introduce fundamentally new ideas, even in the most exact sciences, without occasionally taking a risk.²⁶

Not until 1923, when Compton showed that the scattering of x-rays from electrons could be treated simply as a collision between particles, did the wave-particle duality in light begin to find unreserved acceptance by physicists.²⁷ From there it was a short step to de Broglie's hypothesis of wavelike behavior in particles—and what had been a nicely compartmentalized world of particles and waves dissolved almost overnight into a hash of “wavicles.”

The tortuous evolution of the wave-particle paradigm is *not* evidence for authoritarian resistance to the concept. Instead, it shows plainly that neither the content nor the internal logic of paradigms furnishes authoritative standards for judging experimental results of a completely novel type. The photoelectric effect was simply incommensurable with existing concepts of the nature of light. It was not predicted by existing paradigms in advance, and, after Hertz's accidental discovery, there was no way to “save the appearances” by grafting new hypotheses onto Maxwell's theory, even in an *ad hoc* fashion. Thus this work was an authentic case of *premature discovery*—today's anomaly or embarrassment which turns out to be the kernel of tomorrow's paradigm.²⁸ Unfortunately, one can seldom judge accurately *today* whether the result is humbug or the makings of a Nobel Prize.

Thus, the prototypical controversy about new paradigms appears to be a struggle with *language*. “In the be-

ginning of the investigations," writes Heisenberg, ". . . the words are connected with old concepts; the new ones do not exist yet."²⁹ Thus, the solution of the controversy cannot come from the rules which relate the paradigms to one another, but from a higher level of thought which comprehends the paradigms as special cases—just as a dispute about grammar cannot be resolved by the rules of spelling.³⁰

It is tempting to say that questions about paradigms must be settled by metaphysical arguments, but physicists are more easily swayed by elegance than by metaphysics. "It is more important," wrote Dirac, "to have beauty in one's equations than to have them fit experiment," because whatever discrepancies exist "may well be due to minor features . . . that will get cleared up with further developments of the theory."³¹ Certainly, the final acceptance of Einstein's quantum theory of light seems to have resulted as much from the strong aesthetic appeal of his conceptualization as from its explanation of the photoelectric effect.

Community, Certifiability, and Quality Control

If neither the data nor the paradigms of science are absolutely authoritative, we are left in a precarious position. The responsibility for adjudicating conflicting claims rests *ipso facto* on the community of science—but the rules of evidence admit not only objective criteria but also such subjective considerations as the aesthetic qualities of theories.³²

A set of subjective standards may be internalized by the formation of a "school" of scientific thought, but such enterprises have not enjoyed spectacular success.³³ Hence, one may legitimately wonder if there is some way for the community of science to define itself and its patterns of growth so that controversial theories and experiments will always be examined on intellectual merit alone.

But two contrasting idealizations of scientific identity make this an extraordinarily problematical task. On one hand, there is a view of the community of science traceable to Sir Francis Bacon, which stresses the inductive, experimental character of its work; the formal, public apparatus of consensus—journals, societies, and conferences; lengthy schooling and socialization of scientists during which they acquire loyalties to, and are in turn certified by, the community; team research; the elaboration and extension of existing paradigms; and the progressive vanquishing of ignorance across a broad front. On the other hand, there is a tradition personified by René Descartes, which emphasizes the deductive, theoretical side of science—the informal networks of information which spring up among those of like temperament and interest, individual research, imaginative, unorthodox approaches to problems, and the breakthrough to new discoveries in problematical areas of inquiry.³⁴

The temper of the average modern scientist is predominantly Baconian, and he senses an enduring and inescapable conflict between himself and the solitary Cartesian genius who periodically shakes the foundations of science. A classic example comes from the history of thermodynamics, one of the frontiers of physics during the early nineteenth century. In 1845, James Waterston explained some of the thermodynamical properties of gases by assuming a gas to consist of “hard-sphere” molecules, moving in random directions with some distribution of velocities. In terms of his model, for instance, the pressure of a gas on the walls of its container would be caused by the aggregate force of all the molecules colliding with the wall. Although Waterston was not a Fellow of the Royal Society, the Society’s rules would have permitted his paper to be read before the Society and to be published in its *Philosophical Transactions*. However, the referees did not like Waterston’s work, and though it was read in 1846, it was

not published by the Society. Due to a technicality in the Society's rules, however, the only copy was retained in its archives, so that Waterston was unable to publish it elsewhere.³⁵

In 1892, Lord Rayleigh discovered the paper in the Royal Society Archives and had it printed. In his preface, he remarked that "highly speculative investigations, especially by an unknown author, are best brought before the world through some other channel than a scientific society," and that someone in Waterston's position should establish a reputation "by work whose scope is limited, and whose value is easily judged, before embarking on higher flights."³⁶

Hindsight is always cruel, and particularly so in this case, since James Joule won ready acceptance for essentially the same theory about twenty years later.³⁷ But one ought not to judge Lord Rayleigh's authoritarian *pronunciamento* too harshly. If one is committed to the search for truth, one must also be wary of being led astray. One must leave no stone unturned, but one must also be careful how much time and energy one spends turning over stones that have only the same beetles underneath. The scientist knows from experience that revolutions are infrequent and genius is rare. He is therefore properly skeptical of the paper which proposes "a general theory of Space, Time, Matter and Radiation – an attempt to outdo Quantum Theory and Relativity, Cosmology and the Theory of Elementary Particles in one splendid stroke."³⁸ He is also likely to be wary of strangers and young upstarts, and to mistrust work which does not appear in his own literature, which he can usually be sure is competently reviewed before getting into print.³⁹

On the frontiers of science, his standards may be relaxed somewhat, as a concession to emotional equilibrium in areas where controversy is rife. Recently, for example, the prestigious *Physical Review Letters* simply quit refereeing

papers in high-energy physics – a “hot” field where priority and the chance to publish controversial results are highly esteemed.⁴⁰ But as long as the community of science is struggling with the conflicting aims and standards of the Baconian and Cartesian ideals, there will always be people like Waterston who are denied their due.

One can argue, of course, that the danger of rejecting possible new insights outweighs the danger of letting uncertified and possibly incompetent persons into the discussion. But science would thereby lose the strength of consensus, which establishes a body of knowledge upon which all can agree for discussion. Take that away, and one has not science, but a collection of warring factions, busily anathematizing one another. If you prefer that, the Baconian would argue, better that you should form your own scientific society!⁴¹

Prognosis

We began with the question of a cure for authoritarianism in science – implying the existence of an illness in the body scientific. Some believe the illness to be acute, and have called for radical therapy.⁴² But such a pessimistic diagnosis is almost certainly unwarranted. “Under small perturbations,” as they say in physics, the values of the community of science provide authoritative standards for balancing the competing ideals of scientific practice. These normative structures break down primarily when the data fall far outside expectations, when paradigms are incommensurable with experience, or when new methods, new languages, or infant disciplines are struggling through their early development, a circumstance exacerbated by the specialization of science.⁴³ Thus one may properly speak of “essential” rather than “acute” authoritarianism: The patient cannot be cured – in fact, the symptoms can be removed only at the cost of his life. But the symptoms can be controlled by making the patient aware of his lim-

itations and moving him to a more salubrious climate. This analysis suggests the following course of treatment.

First, scientists – and those who would like to be – need constant reminding that the intellectual and emotional state of a field of inquiry is a sensitive and complicated function of the quality of available data, the complexity and generality of models, and of the patterns of growth in that area. Hence, they must be prepared to change interests and amphoras as the field matures through successive stages: from collecting and classifying data, through the embedding of heuristic schemes into more comprehensive and elegant theories, and finally to the stage where the foundations and interconnections of theories are of paramount interest.⁴⁴ To ignore the question of what activities are fruitful for a field at a given stage in its development is to risk carrying on a mere parody of science.⁴⁵

Second, diligent effort is prescribed both in defining paradigms and in exploring their practical and conceptual limitations, to avoid the situation where theories are dismissed without adequate analysis. For sciences where paradigms are as yet not well established, this means a frank recognition that a discipline cannot be a science until a paradigmatic consensus is achieved, no matter how narrow its boundaries.⁴⁶ For the “harder” sciences, this medicine contains, in addition, a liberal dose of pessimism about the durability of paradigms. Physicists have lived to see an astonishingly successful and long-lived paradigm – that of classical mechanics – altered almost beyond recognition by experimental and theoretical developments in the early twentieth century. It may well be time for younger sciences to stop mimicking the outworn mechanistic determinism of nineteenth-century physics and to consider how their own discipline’s evolution and their efforts will reshape existing paradigms.⁴⁷

The most important part of the cure is fresh air. If

science is about real problems, there must be solutions which cannot yet be described, and which cannot be discovered in any formally prescribed way. The patient, careful work of the Baconian scientist – deeply specialized, intimately familiar with his paradigm – is absolutely essential to the conduct of science. But precisely because of his faithful adherence to the prevailing consensus, he is unlikely to foresee the outlines of those solutions. The germ of a new paradigm is more likely to be brought into the discussion by the Cartesian doubter, the amateur, or the generalist.⁴⁸ The formal approaches of the consensus scientist “are certainly beneficial,” wrote Einstein, “when one is trying decisively to formulate an already discovered truth, but they almost always fail as heuristic aids.”⁴⁹ What is needed for revolutions in science is clear vision, and as Bohr often said, “Clarity is gained through breadth.”⁵⁰

The overall goal of this cure is not to change the dependence of science on consensus, but to ensure that it is achieved in a healthy way. Where a scientific consensus is established by certifying some given set of data, methods, or credentials as authoritative, that consensus will be enforced, paradoxically but inevitably, in authoritarian fashion. As Popper observes, the setting up of such standards is based on the false assumption that “knowledge may legitimize itself by its pedigree.”⁵¹ But if, instead, a consensus is sought without fixed norms for resolving controversies, but with the stipulation that debate continue as long as the participants show good faith, one can avoid the destructive authoritarianism which vitiates scientific inquiry by preventing the free flow of ideas.

It thus becomes desirable to give up the view of rationality as the search for universal, absolute truth in science by means of some specified (and infinitely debatable) set of logical procedures. In its place, we may adopt “the Socratic idea of rationality as a process of conflict between universality and specificity, . . . to wit, rationality as So-

cratic dialectic."⁵² To do so is to accept the quintessentially tentative nature of scientific inquiry, and to be content, if necessary, with the modest task of finding errors in our knowledge by means of civilized and critical discussion.

Professor Nibley has given an appealing sketch of this ideal of science, not as rational *explanation*, but as rational *dialogue*: The method of science, he writes, is "to talk about the material at hand, hoping that in the course of the discussion every participant will privately and inwardly form, reform, change, or abandon his opinions . . . and thereby move in the direction of greater light and knowledge."⁵³

Notes

1. This paper was written for and presented at a symposium in honor of Professor Nibley's sixty-fifth birthday in March 1975 and is thus something of a period piece (though not necessarily dated on that account). Updated citations to recent editions have been added. I am grateful to Professor Dietrich Schroerer of the University of North Carolina for helpful discussions during the first writing.

2. George A. Lundberg, *Can Science Save Us?* 2nd ed. repr. (New York: David McKay, 1971), 43.

3. Hugh Nibley, "A New Look at the Pearl of Great Price," *Improvement Era* 71 (January 1968): 20-22; Nibley, "Archaeology and Our Religion," unpublished manuscript; see also Jacques Barzun, *Science: The Glorious Entertainment* (New York: Harper and Row, 1964), chap. 1.

4. Donald G. Miller, "Ignored Intellect: Pierre Duhem," *Physics Today* 19 (December 1966): 47; Bernard Barber, "Resistance by Scientists to Scientific Discovery," *Science* 134 (1 September 1961): 596. A bitter remark by Max Planck illustrates the distress this situation causes for the individual scientist: "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die. . . ." Max Planck, *Scientific Autobiography*, tr. F. Gaynor (New York: Philosophical Library, 1949), 33-34.

5. The failure of objectivity at the frontiers of science is conceded by many scientists. But historians of science are now challenging the ideal of objectivity even in "normal science." See Stephen G. Brush, "Should the History of Science be Rated X?" *Science* 183 (22

March 1974): 1164. On the importance of consensus, see J. M. Ziman, *Public Knowledge* (Cambridge: Cambridge University Press, 1968).

6. Robert L. Sproull, *Modern Physics*, 2nd ed. (New York: John Wiley, 1963), 81. A counterexample from the heroic age of physics is Newton's manipulation of data on the velocity of sound to make it fit his theory. See Richard S. Westfall, "Newton and the Fudge Factor," *Science* 179 (1973): 751.

7. Franklin Miller, Jr., *College Physics*, 3rd ed. (New York: Harcourt, Brace, Jovanovich, 1972), 2.

8. Norwood R. Hanson, *Patterns of Discovery* (Cambridge: Cambridge University Press, 1958), chap. 1.

9. As reported by Werner Heisenberg, *Physics and Beyond* (New York: Harper and Row, 1971), 63.

10. Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in *Criticism and the Growth of Scientific Knowledge* (Cambridge: Cambridge University Press, 1970), 91-196.

11. As quoted in R. A. R. Tricker, *The Contributions of Faraday and Maxwell to Electrical Science* (London: Pergamon Press, 1966), 108.

12. The interferometer remains one of the most precise instruments available to the physicist. Its operating principles are simple: by means of half-silvered mirrors, a beam of light is split in two, and then reflected along two perpendicular paths of differing lengths. The split beams are then recombined and the location of the "fringes" produced by the interfering waves is measured. See R. S. Shankland, "The Michelson-Morley Experiment," *Scientific American* 211 (November 1964): 107.

13. As quoted in Gerald Holton, *Thematic Origins of Scientific Thought*, rev. ed. (Cambridge, MA: Harvard University Press, 1988), 284.

14. *Ibid.*, 322-34.

15. Ronald W. Clark, *Einstein: The Life and Times* (New York: World, 1971), 107-10.

16. As Holton points out (*Thematic Origins*, 306-15), scientists have generally assumed that Einstein's main concern was the Michelson experiment. Both direct and indirect evidence suggests the contrary: that his overriding concern was the resolution of what he saw as intolerable formal and physical inconsistencies in the transformation theories of mechanics and electrodynamics. Additional experimental evidence for the theory became available in 1909, with the observation of an increased mass of electrons traveling at velocities comparable to that of light. See Robert M. Eisberg, *Fundamentals of Modern Physics* (New York: John Wiley, 1961), 37-38.

17. H. A. Lorentz, *Collected Papers*, 9 vols. (The Hague: Martinus Nijhoff, 1935), 8:415.

18. As quoted in Brush, "Should the History of Science Be Rated X?" 1171-72, n. 35.

19. The use of the terms "normal science," "scientific revolution," and "paradigm" follows Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed., enlarged (Chicago: University of Chicago Press, 1970).

20. Michael Polanyi, *The Tacit Dimension* (New York: Doubleday-Anchor, 1967), 22.

21. Max Jammer, *The Conceptual Development of Quantum Mechanics* (New York: McGraw-Hill, 1966), 33ff.

22. A useful introduction to this work, containing Einstein's first two papers on the topic, with references to the early experimental work, is Armin Hermann, *Die Hypothese der Lichtquanten* (Stuttgart: Ernst Battenberg, 1965).

23. The character of these experiments and the discarding of Newton's corpuscular theory of light is handled at length by Norwood R. Hansen, *The Concept of the Positron* (Cambridge: Cambridge University Press, 1963), chap. 1.

24. Some insight into the significance of symmetry concepts in Einstein's thinking is given by Holton, *Thematic Origins*, 362-67.

25. Robert A. Millikan, "Albert Einstein on His Seventieth Birthday," *Reviews of Modern Physics* 21 (1949): 344-45.

26. Quoted by Jammer, *Conceptual Development of Quantum Mechanics*, 44.

27. The history of this change in attitude is recorded in *ibid.*, 160-65.

28. Gunther S. Stent, "Prematurity and Uniqueness in Scientific Discovery," *Scientific American* 227 (December 1972): 84.

29. Werner Heisenberg, "Tradition in Science," *Bulletin of the Atomic Scientists* 29 (December 1973): 4.

30. Polanyi, *The Tacit Dimension*, 33-37.

31. P. A. M. Dirac, "The Evolution of the Physicist's Picture of Nature," *Scientific American* 208 (May 1963): 45.

32. Jerome R. Ravetz, *Scientific Knowledge and Its Social Problems* (Oxford: Oxford University Press, 1971), 223-33.

33. Joseph Haberer, *Politics and the Community of Science* (New York: Van Nostrand Reinhold, 1969), chaps. 3-4. For further analyses of the character of scientific work, see Ravetz, "Science as Craftsman's Work," *Scientific Knowledge*, chap. 3; and Warren Hagstrom, "Social Control in Science," *The Scientific Community* (New York: Basic Books, 1965), chap. 1.

34. As exemplified by Ziman's remark that "Only the crank – or his cousin the rare genius – decides to find the explanation of Gravitation" (*Public Knowledge*, 60).

35. Stephen G. Brush, *Kinetic Theory*, 3 vols. (London: Pergamon Press, 1968), 1:17-19.

36. *Ibid.*, 1:18

37. Ziman, *Public Knowledge*, 113.

38. *Ibid.*, 114.

39. On the sociology of refereeing in physics, see Harriet Zuckerman and Robert K. Merton, "Sociology of Refereeing," *Physics Today* 24 (July 1971): 28.

40. S. A. Goudsmit, "Editorial: A Drastic Change in Policy," *Physical Review Letters* 13 (20 July 1964): 79; S. A. Goudsmit, "Important Announcement Regarding Papers about Fundamental Theories," *Physical Review* 80 (18 July 1973): 357.

41. That is precisely the origin of the British Association for the Advancement of Science (Brush, *Kinetic Theory*, 1:17-19).

42. Gerald Holton, "On Being Caught between Dionysians and Apollonians," *Daedalus* 103/3 (1974): 65.

43. Hagstrom, *The Scientific Community*, 256ff.

44. See the discussion of stages of science in C. H. Waddington, *Behind Appearances* (Cambridge, MA: M. I. T. Press, 1970), 1-6.

45. The social sciences have a particularly difficult time in this connection. See Dankwart A. Rustow, "Relevance in Social Science, or the Proper Study of Mankind," *American Scholar* 40 (Summer 1971): 487.

46. Ravetz, "Immature and Ineffective Fields of Inquiry," *Scientific Knowledge*, chap. 14.

47. Gunther S. Stent, "Limits of the Scientific Understanding of Man," *Science* 187 (1975): 1052.

48. An instructive example from geophysics is discussed by A. Hallam, "Alfred Wegener and the Hypothesis of Continental Drift," *Scientific American* 232 (February 1975): 91.

49. As quoted in Hermann, *Die Hypothese der Lichtquanten*, 15.

50. As quoted in Heisenberg, *Physics and Beyond*, 276.

51. Karl Popper, *Conjectures and Refutations* (New York: Harper Torchbooks, 1968), 25.

52. Joseph Agassi, "Unity and Diversity in Science," in R. S. Cohen and M. W. Wartofsky, eds., *Boston Studies in the Philosophy of Science*, vol. 80 (Dordrecht: Reidel, 1969), 405.

53. Hugh Nibley, *Since Cumorah*, vol. 7, *The Collected Works of Hugh Nibley*, 2nd ed. (Salt Lake City: Deseret Book and F.A.R.M.S.,

1988), xiv. As this paper goes to press, the development of theories describing the dynamics of complex systems – “chaos theory,” in the vernacular – are forcing us to question even the postulated connection between determinism and predictability that lies at the heart of the concept of causality in classical mechanics. As this field has developed, all of the issues of authoritative *vs.* authoritarian criticism of new science have been starkly outlined; readers with interests in the history of science may find James Gleick’s bestselling *Chaos: Making a New Science* (New York: Penguin, 1987) especially instructive and entertaining. While the philosophical implications of chaos theory are by no means all worked out, it is clear that we shall be forced to confront the habits and mode of scientific discourse in profound new ways. All so much the better for both science and society, of course.