

# Constructions of Scientific Knowledge\*

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.....given their cultural resources, only singular incompetence could have prevented members of the HEP [high energy physics] community from producing an understandable version of reality at any point in their history. And, given their extensive training in sophisticated mathematical techniques, the preponderance of mathematics in particle physicists' accounts of reality is no more hard to explain than the fondness of ethnic groups for their native language. Andrew Pickering<sup>1</sup>

The Unreasonable Effectiveness of Mathematics in the Natural Sciences. Title of a paper by Eugene Wigner<sup>2</sup>

The "science wars"<sup>3</sup> are a collection of vociferously argued academic disputes about the nature of scientific knowledge and about how that knowledge is constructed. Some of the discussion is both reductive and implausible: Natural scientists sometimes declare that their the production of knowledge within their subjects is not a social endeavor and is free from all social content or political values.<sup>4</sup> Conversely, social scientists have been known to maintain that scientific knowledge has little content beyond being a set of socially constructed and negotiated statements aimed to uphold the social status of the scientists.<sup>5</sup> Of course, most of the discussions are much more nuanced than that. Nonetheless a big gap remains between the different points of view.

In this note, I want to examine this dispute in the light of the history of theoretical physics within roughly the period from 1974 to 2002. This period is chosen because of the studies of high energy physics which touch upon 1974 as a moment of change in this scientific area.<sup>6</sup> At this time, a new consensus began to form about the nature of subatomic particles, a consensus partially based upon new experiments and equally upon a new approach to the theory of these particles. This note also touches upon developments in the area called condensed matter physics, which studies

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the behavior of liquids and solids and other materials. Looking back, one can see 1974 as a time of exceptional strength for theory both in HEP and in condensed matter.

In the last quarter century, people who study science have quite properly given more credence than heretofore to the idea that science is a social endeavor. Natural scientists have argued back, saying that a rational understanding of nature is of paramount importance in determining knowledge in their fields. In this paper, I argue that the development of physics within this period supports both sides of the argument in that negotiation, consensus formation, examination of nature, and prior ideology are all important determinants of the way theoretical physics understands what it sees. I give three examples:

Superconductivity is a very distinctive state of matter in which materials can carry electrical currents without loss of flow for extremely long periods. For more than ninety years, superconductors have been known to exist at low temperatures, below 25 degrees Kelvin. In 1974, theorists believed that they understood superconductivity pretty well based upon the then-known superconducting materials. The existing theory had been put into place starting in the late 1950s in order to explain these materials. It was widely believed that superconductors would not be found at higher temperatures. However, in 1986, two Swiss experimentalists, Johannes Bednorz and Karl Müller, unexpectedly discovered that there was a complex material which retained its superconductivity much above 25 degrees Kelvin. Other materials in that class were soon found, and their properties measured. They were seen to be, in many ways, quite different from other materials. Specifically, electrical currents tended to remain within very thin sheets in the material. Over the last fifteen years, many physicists working in an area called condensed matter physics have attempted to understand and describe these superconductors. Physicists in this area are traditionally committed to both obtaining fundamental knowledge about new classes of materials and equally to the development of materials which might be of practical use. High temperature superconductors promised to be very exciting from both points of view. They showed a quite distinctive behavior, and they might be of economic importance. Hence both experimentalists and theorists rushed in to work on them. This

scientific/technical area was defined (by a social agreement, of course) to be a central problem of condensed matter physics.

In the following years, further major discoveries about these materials were made by the experimentalists. In contrast, the theoretical work in this area remains rather disappointing. Indeed, previously accepted knowledge about superconductors and related materials had to be discarded in the light of new knowledge gained from the thin-sheet materials. This period of dissolution of theoretical certainties has continued for the past sixteen years.

Theorists in this field of science lack neither the mathematical and technical skills of other physicists, nor the social skills to negotiate and achieve consensus. But to everyone's embarrassment, both in personal terms and in terms of the prestige of the scientific area, it appears that so far theory has not begun to reach its goals. There is no theory that looks mathematically respectable and also explains a broad range of phenomena in these superconductors. Nature has not been kind, and that unkindness has hurt this particular sub-field of physics quite substantially. In contrast to the view presented in the epitome, the particular group of excellent scholars working on superconductivity theory has not reached an "understandable version of reality" within their subject. Negotiation is not enough.

Next, let us turn back to HEP. In this field people perform accelerator experiments in which high energy particles are allowed to collide with one another and the debris coming off the collision is examined<sup>7</sup>. I would argue that there was and remains a substantive agreement among the practitioners about the purpose of their area.<sup>8</sup> With a singleminded intensity they view their task as expressing the fundamental laws of nature. They expect to find these laws in the smallest and "most elementary" constituents of the universe and in the interactions between these constituents. If some collision among particles throws off a shower of pieces, they want to understand the basic forces that underlie the process rather than the details of the repeated communication of one particle to another. There was a sort of revolution in 1974 which changed the focus of high energy physics. The newer view rejects the study of the final stages of the shower and focuses instead upon the

preceding processes, which involve fewer and more exotic particles and hence are considered to be more basic. The newer view uses the theoretical approach called Lagrangian field theory as an apt description of the structure of the early stage and turns away from the some other theoretical approaches as less appropriate for the task at hand. As this newer view has become more broadly accepted, the somewhat tentative scientific consensus of 1974 turned into what is called “the standard model” of particle physics.<sup>9</sup> As if it were intended to deflate skeptics, the standard model has proved to be remarkably successful in predicting new results and explaining old ones. It is a somewhat complicated construction, containing twenty-two parameters, i.e. numbers, which must be determined by experiment. But, given those numbers, everything else predicted by the theory seems to come out in the experimental observations and to come out correctly. After very extensive study, no discrepancies between experiment and this standard model have been observed to date. So the mathematical predictions produced by this new area of physics have, in the last quarter century been borne out. This success seems to support the contention of natural scientists that “sophisticated mathematical techniques” can produce meaningful predictions about the world.

Perhaps one might argue that this apparent success is not a product of nature speaking in the language of mathematics, but is instead a demonstration of the social and political skills of the high energy physics community. Maybe they have produced this agreement by a process of negotiation, and then supported it in order to maintain their position in the world of scholarship. Not so. The very success of particle physics in solving its problems as defined in 1974 or thereabouts has been a sociological disaster for the whole field. As Kuhn<sup>10</sup> argued, the continued success of any field of “normal science” depends upon its generation of fruitful little puzzles for the practitioners to solve. The complete success of the standard model has left no room for such puzzles. It explains everything it touches. There is no room to find what is called “new physics” or even to see really new phenomena within the old physics. And so their very success<sup>11</sup> has left the community with less visible work to do, leaving it with reduced financial support and reduced prestige. The community continues to believe in its goals of producing new and fundamental knowledge. By and large, it is not willing to compromise by

studying phenomena it regards as “less fundamental”, like the production of large numbers of particles in scattering events. So by sticking to its guns, the community has seen its place in the world erode. The particle experimentalists need large accelerators to make their science, but have not gotten the accelerators and funding they want and need. Both theorists and experimentalists have found it hard to attract the graduate students needed to form the next generation of scholars. No amount of negotiation and consensus building can hide the fact that the HEP community has, in a very substantial fashion, lost out.

As HEP has lost, other areas of scholarship have gained. The experimental cosmologists, who study the behavior of the whole universe, have gained in relative prestige in the experimental world and have begun to do quite well in the funding sweepstakes. In particle theory, the most prestigious roles have passed away from the people who study the outputs of the accelerators and instead reappeared in the grasp of theoretical cosmologists and of a whole new breed of theoretical people called string theorists. The latter study phenomena in a region of energies beyond the hope of experiment, and look for much of their justification to their allies in mathematics.

My third example of the insufficiency of consensus formation is provided by string theory itself. Its subject of study lies within particle physics. It is focused upon the presumed-to-be-fundamental part of particle physics at very high energies, far beyond the reach of present-day experiments. This scientific area has attracted some of the best minds working on applications of mathematics to the physical world. These people have formed important alliances with mathematicians. Of course, all string theorists have to believe with that mathematics provides a portal to nature, and even a road to understanding truly exotic processes, far from intuition and imagination. They must expect that considerations based upon mathematics, and mathematical beauty, will provide sufficient information to permit making useful predictions about a part of the world entirely alien from experience and from experiment. Moving into this field requires a big act of faith.

Despite (or perhaps because of) this high entry fee<sup>12</sup>, the joint work of string theory and mathematicians has proven quite important. It has

revitalized an entire area of mathematical sciences. It wins some of the very best students entering both fields. Excellent minds and powerful people have concentrated in string theory, and worked to build further their prestige and power. Nonetheless, this group has not regained the previous strength of theoretical particle physics. Their subject of study remains a somewhat controversial and precarious academic endeavor. It lacks a basis in day to day experimental reality, and for this reason cannot command the unquestioned prestige of the old particle physics. Everything would be changed were string theory able to predict the twenty-two numbers required by the standard model. But, so far, string theory seems hopelessly far from generating these numbers or other experimentally testable predictions. Despite the fact that a successful prediction would improve the standing of this scientific field, no viable number-generation has been achieved by consensus, negotiation, or any other strategy. Thus in HEP, in high temperature superconductivity, and in string theory nature, as represented by experimental reality, has proved to be an important force, determining the social prestige to be conferred upon her votaries. All three fields have been hurt, respectively because nature has not supported the position of any one of the theories, or because she has not provided new puzzles, or because she stands too far away to be brought into the picture<sup>13</sup>.

In the above discussion, I have stressed the agency<sup>14</sup> of nature in the determination of the views reached by the natural scientists in the three areas considered. But there is no doubt that these views are reached by a social process which includes negotiation and consensus formation. In fact the strength of the consensus reached by physicists is quite remarkable. The great majority of people in each area agree about the content of the field, about what is known and what remains beyond reach. The content of the statements made above about the three major subfields of physics are polite, but hardly flattering. Nonetheless, I think they represent a consensus view. In my own area, which one might describe as the study of chaos and complexity, I have participated in public statements<sup>15</sup> that there is no science of complexity, and this view is accepted as a constructive contribution toward reaching a consensus about that field. Thus physicists' public statements are part of a process of negotiation, which often ends in consensus formation.

The consensus, however, is about the scientific content of the different areas. Everyone is willing to accept the practitioners' views that recent superconductivity theories have opened possibilities but not proven themselves, that the standard model has stood up to testing, or that while amazing structure has been revealed by the mathematics of string theory, it too remains unproved. There are also disagreements and border wars. They are unseemly arguments about the relative value and different values of the different areas of theoretical physics. But we physicists have mostly agreed to accept one common view of the scientific facts (or "facts").

How is it that negotiation and consensus formation<sup>16</sup> is such a fully recognized and accepted part of the world of physics? History provides a partial answer. Look back to when Boyle formed the British Royal Society<sup>17</sup>, and thereby presided at the birth of organized experimental science. Boyle was working in part to devise a method to negotiate and gain consensus for experimental "facts." These would be generated by the experiments and demonstrations performed by Hook<sup>18</sup>, and also generated from the contributions of the Society's members and correspondents. This Society's work was intended to be a model for the process of negotiation and agreement which might work in the political society of the modern, plural, state. Hobbes, on the other hand criticized this Royal Society process as necessarily divisive, and proposed a more authoritarian model as the only way of avoiding destructive discord. My discussion is intended to suggest that, at least in the operation of physics, Hobbes was wrong.<sup>19</sup>

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1. Andrew Pickering, *Constructing Quarks, a Sociological History of Particle Physics*, (University of Chicago Press, Chicago, 1984). Page 413.

2. Eugene Wigner "The Unreasonable Effectiveness of Mathematics in the Natural Sciences," in *Communications in Pure and Applied Mathematics*, vol. 13, No. 1 (February 1960).

3. There is a very extensive literature on this subject. See, for example, Ullica Segerstråle, editor, *Beyond the Science Wars* (State University of New York Press, Albany, New York, 2000), James Robert Brown, *Who Rules in Science* (Harvard University Press, Cambridge, 2001), A. Sokal and J. Bricmont, *Impostures Intellectuelles* (Editions Odile, Paris 1997) N. Koertge, editor, *A House Built Upon Sand: Exposing Postmodernist Myths about Science* (Oxford University Press, Oxford 1998), Mario Biagioli, editor *The Science Studies Reader* (Routledge, New York and London 1999). Stephen Weinberg *Facing Up, Science and Its Cultural Adversaries* (Harvard University Press London, 2001).

4. For example, "a scientific theory is....culture free and permanent " in Steven Weinberg *Looking Up* op cit. Page 135.

5. Stanley Fish in a *New York Times* piece titled "Prof Sokal's Bad Joke" (page A11, May 21, 1996) compared scientific theories to the rules of baseball. Steven Shapin and Simon Schaffer in *Leviathan and the Air-Pump* (Princeton University Press, 1985) page 344 "... it is ourselves and not reality which is responsible for what we know. Knowledge, as much as the state, is a product of human action."

6. See Andrew Pickering, reference 1 and Peter Galison *Image and Logic* (The University of Chicago Press, Chicago and London, 1997), especially chapters 5-7.

7. In this paper, I will use this definition of high energy physics. It is rather narrower than the one usually employed by the physics community which often includes within HEP the study of particles produced in the distant universe and their effects as seen on earth.

8. For an analysis of the thinking of people in physics see Margaret Wertheim, *Pythagoras' Trousers*, (Norton paperback, New York, 1997). She suggests that some of the modes of thought can best be understood as a consequence of the monastic background of the physical sciences. See

also Sharon Trawick *Beamtimes and Lifetimes, The World of High Energy Physicists* (Harvard University Press, Cambridge 1998).

9. See material cited in reference 6 and Tain Yu Cao, *Conceptual Developments of 20th Century Field Theories* (Cambridge University Press, Cambridge, paperback edition, 1998), L. Brown, L. Hoddeson, and M. Riordan, eds. *The Use of the Standard Model* (Cambridge University Press 1995), Tain Yu Cao, *Conceptual Foundations of Quantum Field Theory* (Cambridge University Press, Cambridge, 1999), see especially the papers of Weinberg (pages 241-251) and Laurie Brown (pages 252-256) and the following discussion. .

10. Thomas Kuhn *The Structure of Scientific Revolutions*, (2nd edition, Chicago University Press, Chicago and London 1970).

11. This success does not extend to the prediction of the twenty two numbers. So far, essentially no progress has been made in predicting their values and nobody sees a promising approach for doing so. And “no promising approach” and “all things successfully predicted” are equally bad for a field of science in that they leave no room for puzzle-solvers.

12. See Werthheim, reference 8.

13. The reader will notice that I studied examples which display both the successes and the failures of this area of science, and have described both cases using the same methodology. In doing this, I have followed the suggestions of David Bloor (*Knowledge and Social Imagery*, University of Chicago Press, second edition Chicago, 1991 who has defined the “strong programme” (page 7) for studying the social construction of science. Part of this program is to use the same tools to describe how people reached to correct and incorrect conclusions. Here I extend this to describe how they came to fruitful or counterproductive consensuses.

14. Agency, as used here “is the state of being in action or of exerting power”, *Random House College Dictionary First Edition* (Random House, 1975), page 25. This is the seventh meaning in this dictionary. This term is used this way in the humanities, apparently to avoid the associations contained in terms like “power” or “influence.”

15. Nigel Goldenfeld and Leo Kadanoff, *Science* **284**, 87 (1999). Leo Kadanoff, *Physics Today* **39** pages 34-39 August 2001 and also “A New Kind of Science” to be published *Physics Today* August 2002.

16. Another piece of telling evidence: At my university, the University of Chicago, nominations of the physics department for honorary degrees almost always reflect a unanimous vote and sentiment of the department.

17. See Shapin and Schaffer, *op. cit.*.

18. I don't think Boyle would have seen it quite that way.

19. *loc. cit.*. Chapter 8 and especially page 344. However, I am not sure whether Hobbes or Boyle won out in their argument about the physics of an evacuated vessel. According to today's best view Boyle was right in saying that they had gotten most of the air out; Hobbes was right in saying that what was left is replete with important and interesting physical properties.