STEPHEN G. BRUSH

SUGGESTIONS FOR THE STUDY OF SCIENCE

Recently, an education columnist in the *Washington Post* wrote that students should be given some idea of "how the various disciplines fit together (the history of science, the mathematics of sport…)".[1] This reminded me once again of the great potential audience for our field. In an age when education seems to be dominated by relentless specialization and the testing of factual knowledge, many teachers, parents and other citizens are fascinated by the Big Questions: What is the origin and structure of the universe? Are science and religion compatible? Did humans evolve from simpler organisms? Is human behavior determined by genes or culture? Why did European civilization come to dominate the world after the fifeteenth century? Do science and society influence each other?

If historians of science do not give intelligible answers to these questions, someone else will. In fact, others already have done so. In the general science section of any comprehensive bookstore you will find many books that seem to use the history of science to tell fascinating stories about how we arrived at our present understanding of the world and the lively controversies along the way. Plays about physicists and mathematicians (*Copenhagen*, *QED*, *Proof*) have been popular. The authors of these works are often very good writers and some of them even read our publications. However, few of them are historians of science in the modern sense. They repeat old myths and stereotypes about the history of science without making the effort to study original sources and do serious research in archives.

Historians of science often write more accurate and interesting accounts than the traditional stories but their language should appeal more effectively to students and the public. For many years, historians of science were reluctant to write textbooks and popularizations, perhaps because they realized how much research needed to be done to get past the myths or because they feared that addressing issues of current interest would legitimize the much-maligned "whig" interpretation of history. Recently however, there has been a revival of good expository writing for a wide audience: several comprehensive textbooks and short monographs, readable and reliable, are now available.

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1 Karin Chenoweth, "Homeroom: Taking the Measure of Magnet's Attractions," *Washington Post, Prince George's Extra* (15 November 2001), p. 6.

The first four paragraphs of this article are taken from "A Wider Audience for History of Science" in the *American Institute of Physics Center for History of Physics Newsletter*, 34, no. 1 (Spring 2002), p. 4, reprinted by permission of the American Institute of Physics. I have also incorporated material from my Keynote Lecture, "Different Directions in the History of Physics in the 1990s" presented at the Joint Atlantic Seminar in the History of the Physical Sciences, College Park, Maryland, 17 September 1999.

K. Gavroglu and J. Renn (eds.), Positioning the History of Science, 13–25.

In science education, the historical approach can no longer be considered just a distraction that takes time away from learning "real science". On the contrary, research done on the Project Physics course for high schools showed that this historically-oriented text, in combination with simulations of the experiments done by Galileo and other great scientists, enhanced students' understanding of the nature of science while preparing them to do as well on standardized tests of subject-matter as students taking a traditional course.[2]

Nor is there necessarily any conflict for a historian of science, between research and educational or popularizing activities. At least in my own case, the effort to present an intelligible and accurate view of science to undergraduate students inspired me to undertake new research projects, and the results of those projects were directly incorporated into my teaching.

The purpose of this essay is not to persuade historians to put more effort into teaching undergraduates or to write more books and articles for the public. Instead, it is to argue that more attention to the historical questions that interest non-historians would stimulate research and analysis that is beneficial to us as professional historians of science. The problems we have been addressing during the last two or three decades are important and worth studying, but it is time to look at other kinds of problems that have been neglected.

I am not alone in my dissatisfaction with the present state of the discipline but others have rather different reasons for being dissatisfed. Let us begin with a recent assessment of the state of our field, as seen by one eminent practitioner, in a review of Jan Golinski's book *Making Natural Knowledge*:[3]

> "The place of the history of science in the academy (in the United States as well as elsewhere, save perhaps for Holland) is appalling. Only a few universities have free-standing departments; where these are lacking, history departments may employ one or two professors in this area. Historians, by trade, know "nothing about science." Thus, although we have learned quite a lot about women and workers, wars, political movements and other important aspects of ordinary life, science – the muscle of twentieth-century North America – has been understudied and poorly understood.
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> And for a number of reasons. Chief among them is a prevailing epistemology that has lent privileged status to science as pure and objective, largely unsullied by the mess of human subjectivities. Jan Golinski explains how constructivism, which he defines as a methodology that "directs attention to the role of human beings, as social actors, in the making of scientific knowledge" (p. 6), has exploded this foundational belief. Constructivism has historicized science and in so doing has called for analysis of all its associated categories: discovery, evidence, argument, experiment, expert, laboratory,

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2 See my article "History of Science and Science Education," in *Scientific Literacy Papers: A Journal of Research in Science, Education and the Public* (Oxford), Summer 1987, pp. 75–87; reprinted in *Teaching the History of Science*, edited by M. Shortland & A. Warwick (Oxford: The British Society for the History of Science/Blackwell, 1989), 54–66 and in *Interchange: A Quarterly Review of Education* (Toronto), 20, no. 2 (1989): 60–70. The success of the journal *Science & Education: Contributions from History, Philosophy and Sociology of Science and Education* (Kluwer, volume 12 published in 2003) documents the widespread international interest and activity in this enterprise.

3 Review by Londa Schiebinger in *American Historical Review* 103 (1998): 1554–1555.

> instrument, image, replication and law. The heat of the current "science wars" – those unproductive tussles between scientists and their critics – reflects perhaps the success of the last thirty years of science studies.

This quotation raises some interesting questions. Is the pitiful state of history of science worse than it was 30 years ago, and is this despite or because of the "success" of constructivist science studies? Is constructivism the only acceptable way to "historicize science," and is it in fact the dominant trend in history of science at present? If the two sides in the science wars are identified as "scientists and their critics" does that mean that constructivists are really anti-science, as some scientists claim and many constructivists indignantly deny?

Whatever may be the state of history of science as a whole, research in the history of *physical* science is flourishing and highly regarded by scientists. One reason for this relative success is that physicists, chemists, astronomers and geologists have strongly supported historical research through their societies (for example, the Center for History of Physics, financed partly from the revenues of physics journals) and journals (one can publish historical articles not only in the relatively new *Physics in Perspective* but also in the well-established *Physics Today* and *Reviews of Modern Physics*). Historical sessions at meetings of these societies attract large audiences. Authors whose primary training is in science publish in professional history of science journals, hold professorships in university departments of history or history of science and win prizes offered by history of science societies. Thus, the premise that there is some inherent hostility between scientists and historians is certainly not universally valid.

I begin by describing some trends in research on the history of science; only one of them, and not the most popular in the 1990s, is constructivism. Two other approaches, which I call "modernism" and "contextualism," dominate the publications I am familiar with.

Next, I propose a couple of unsolved problems that should, in my opinion, provide fruitful research opportunities in the twenty-first century (although historians of science now seem reluctant to tackle them): explaining the Scientific Revolution and elucidating the "nature of science". Finally, I mention a topic we already know a lot about but have not made into a coherent theme: the role of mathematics in the introduction of new ideas about the physical world.

MODERNISM, CONTEXTUALISM AND CONSTRUCTIVISM

Thirty or forty years ago one could clearly distinguish between publications by scientists, which were generally internalist and whiggish, and works by historians, which were more likely to be externalist and contextual. In fact, historians proclaimed their rejection of the "whig interpretation of the history of science" to demonstrate their independence from the scientific community they were studying, while scientists simply ignored what historians were writing about them. Since then the two groups have moved much closer together and their approaches can be regarded as complementary rather than antagonistic. At the same time, scholars from other disciplines – sociology, women's studies and literary criticism to mention only three – have become interested in the history of science from their own perspectives and their work has greatly enriched the history of science by introducing new questions, methods and sources.

Lacking a generally-accepted term, I have used the term "Modernist" for the successor to the old whig internalist history; it may be considered "presentist" in its *choice* of subjects, but is no longer whiggish in its treatment of those subjects. The Modernist is still interested in long-term trends, revolutions and the role of ideas like continuity, atoms, force, progress, etc., in early as well as modern science. However, the focus is on the development of the science itself, with the technical details explained in a way that engages the attention of experts as well as general readers. Sam Schweber's magnificent history of quantum electrodynamics (*QED*, 1994) is a good example of a Modernist history, although he has also written in the Contextualist mode.

"Contextual" is a familiar term for the analysis of science in relation to other factors (social, institutional, economic, political, psychological, etc.) pertaining to a particular time and place; it is the successor to the old "externalist" approach, having been enriched by much greater attention to the technical aspects of the science. However, it is more limited to particular times and places (thus giving rise to the complaint that the "Big Picture" is ignored). Contextualism is not the same as "Social Construction," though there is obviously some overlap between the two: both may use the same kind of evidence but interpret it with different assumptions. The Contextualist, like the Modernist, assumes that scientists are discovering facts and laws that correspond at least approximately to some reality in the physical world; the Social Constructionist does not. Among other approaches are the "Artistic" (studies that emphasize the role of visual presentation, musical harmony or aesthetic factors in the development of science) and the "Feminist/postcolonial" (studies that discuss the development of science from the perspective of disadvantaged groups such as women, ethnic or racial minorities and third world populations). I am especially interested in "Philosophical" approaches that analyse phenomena such as the acceptance or rejection of theories in terms of philosophical criteria (e.g., testing of novel predictions).

Most professional research in the history of science in the past couple of decades has been done in the Modernist or Contextualist mode. Social Construction, despite the large amount of publicity it has received and its apparently widespread influence within the larger community of Science and Technology Studies (STS), is found in only a small number of publications. This may reflect its faddish character: by now most of the founders of Social Constructionism have either rejected or substantially modified their original radical positions.[4] The other three approaches

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4 Thomas S. Kuhn, whose famous *Structure of Scientific Revolutions* inspired many of the Social Constructionists, explicitly rejected their work in "Reflections on Receiving the John Desmond Bernal Award," *4S Review* 1, no. 4 (1983): 26–30 and in *The Trouble with the Historical Philosophy of Science* (Cambridge, MA: Department of the History of Science, Harvard University, 1992). Bruno Latour and Steve Woolgar, whose book *Laboratory Life: The Social Construction of Scientific*

(artistic, feminist/postcolonial and philosophical) are also sparsely represented in the general history of science journals, though they flourish in specialized journals.

Another dimension is the subject-matter studied by historians of science. A glance at the contents of a journal like *Isis* shows that "science" does not usually include mathematics, technology or medicine. (By contrast, the scope of *Isis* in its early years or of *Social Studies of Science* currently, seems much broader.) I think this contraction of our field has been umfortunate; a historian of science should not have to seek out specialized journals on history of mathematics, technology or medicine to learn about the relevance of those subjects to physics, chemistry and biology.

EXPLAINING THE SCIENTIFIC REVOLUTION

To me the most important question in the history of science is "why did the Scientific Revolution happen in Europe in the 17th century"? It is also one that undergraduates find fascinating, judging by class discussions and their choice of topics for an assigned essay.

Many factors have been proposed: social/economic/religious conditions in Europe in the 15th and sixteenth centuries, recovery of ancient Greek science and mathematics, Humanism, the "natural law" concept, geographical discoveries, etc. But how can we determine which of these factors is important, necessary or sufficient

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Facts (Beverly Hills, CA: Sage, 1979) is still a canonical text of the movement, pointedly omitted the word "social" in the subtitle of their second edition (Princeton, NJ: Princeton University Press, 1986) and Latour himself elaborated his view that STS based on Social Constructionism is obsolete, in "One More Turn after the Social Turn," in *The Social Dimensions of Science*, edited by E. McMullin, pp. 272–94 (Notre Dame, IN: University of Notre Dame Press, 1992). Harry M. Collins first reduced his "relativism" from an ontological to a methodological position [compare "Stages in the Empirical Programme of Relativism," *Social Studies of Science* 11 (1981): 3–10 on p. 3 with "Son of Seven Sexes: The Social Destruction of a Physical Phenomenon," *ibid.* 11 (1981): 33–62, on p. 54]; he now seems to have abandoned it completely, in his article with Robert Evans, "The Third Wave of Science Studies: Studies of Expertise and Experience," *ibid.* 32 (2002): 235–96, on pp. 239, 240. David Bloor, founder of the social constructionist "Strong Program," later admitted that this program seems to have been forgotten ["Remember the Strong Program?" *Science, Technology & Human Values* 22 (1997): 373–85] and, with his colleagues, explicitly rejected the radical anti-realism of other sociologists [Barry Barnes, David Bloor & John Henry, *Scientific Knowledge: A Sociological Analysis*, Chicago: University of Chicago Press, 1996, pp. 76–77, 87]. Andrew Pickering, in response to severe criticism by philosophers of his book *Constructing Quarks: A Sociological History of Particle Physics* (Chicago: University of Chicago Press, 1984), did not defend it but changed his position in a way that seems to me to water down Social Constructionism ["Knowledge, Practice, and Mere Construction," *Social Studies of Science* 20 (1990): 682–729; *The Mangle of Practice: Time, Agency, and Science* (Chicago: University of Chicago Press, 1995)].

Challenged by Stephen Cole to give just one example in which established knowledge had been socially constructed, Bloor cited Andrew Warwick's study of the reception of relativity theory at Cambridge University; however, this is not very convincing since Warwick covered only the period up to 1911, when the theory had not yet become established knowledge and (as often happens in research at the frontiers) there are different views about what the theory actually means. See S. G. Brush, "Why Was Relativity Accepted"? *Physics in Perspective* 1 (1999): 184–214.

on the basis of only one historical event? To do that we must analyze other situations where some but not all of those factors were present, just as Conrad Russell tried to eliminate proposed causes of the English Revolution by studying an earlier period in English history when that Revolution *didn't* happen.[5]

In particular, you cannot plausibly explain why the Scientific Revolution *did* happen in Europe in the seventeenth century – what has been called "The Grand Question" – unless you try to explain why it *didn't* happen in other places where a very high level of science (and technology) had been reached earlier. The leading candidates are Islam and China. As Raymond Martin argued, we can learn something about historical causation by studying counterexamples.[6] But only a handful of historians of science – notably Joseph Needham and H. Floris Cohen – have seriously considered the question in this way.

One must deal with a set of questions that many historians do not want to discuss, for two reasons. First, they tend to rule out hypothetical questions (why something did not happen) as a matter of principle – "that's not history". Cohen complains that "quite a few scholars have indeed denied flatly" that the question "makes sense."[7] Second, historians deem it offensive to ask why another civilization "failed" to achieve what the West did. Doesn't that presume that the West succeeded and the others somehow took a wrong turn?

The biologist Jared Diamond dared to tackle the larger question: why did European civilization dominate the rest of the world after the fifeteenth century? In so doing, of course, he was careful not to insult the people who lost power, wealth and their lives to the Europeans. The commercial success of his book *Guns, Germs and Steel*[8] suggests that there is a popular demand for explanations of major events in history. However, in this case the excuse "that's not history" is unconvincing, since general historians (unlike historians of science) do regard this as a legitimate question, suitable for discussion in a professional journal as well as in a magazine edited for a broader audience.[9]

There is a small amount of historical analysis directed toward The Grand Question; some of it is summarized in Floris Cohen's historiographic book on *The Scientific Revolution*. But when I decided to include the topic in my undergraduate course, I could not find any general books by historians of science suitable for students. In fact, it is shunned by the handful of good textbooks on science and

5 Conrad Russell, *The Causes of the English Civil War* (Oxford University Press, 1990). See my article "Why Did (or Didn't) it Happen? "*Historically Speaking* 4, no. 5 (June 2003): 20–21, and the letter to the editor about this article by Roger L. Williams, with my reply, *ibid* 5, no. 1 (September 2003), 49–50.

6 Raymond Martin, "Historical Counterexamples and Sufficient Causes," *Mind* 88 (1979): 59–73.

7 H. Floris Cohen, *The Scientific Revolution: A Historiographical Inquiry* (University of Chicago Press, 1994), 381.

8 Jared Diamond, *Guns, Germs and Steel: The Fates of Human Societies* (New York: Norton, 1998).

9 Gale Stokes, "The Fates of Human Societies: A Review of Recent Macrohistories," *American Historical Review* 106 (2001): 508–25; "Why the West? The Unsettled Question of Europe's Ascendancy," *Lingua Franca* 11, no. 8 (November 2001): 30–38.

technology in world history, as well as by books on the Scientific Revolution. One of the best such books in the first category calls the question "illicit" – "foreign to the historical enterprise and not one subject to historical analysis." So I have had to use a book by a sociologist, Toby Huff's *The Rise of Early Modern Science: Islam, China, and the West* (Cambridge University Press, 1993), which is useful but apparently not based on research using original sources.[10]

The challenge to historians of science is: if you do not provide a satisfactory explanation of why the Scientific Revolution happened in seventeenth century Europe but not at another time and place, someone else will do it.[11] My thesis is that if you do undertake to explain why an event happened by invoking certain causes, you should be willing to back up your argument by discussing counterexamples – other situations in which some of those causes were present but the event did not happen. Otherwise you cannot claim that your explanation is satisfactory. Although the task may involve more theoretical reasoning than historians find congenial, it does not mean that the historian has to be *scientific*, either in the sense of Popper (making predictions of future events) or in the sense of modern physics (developing general laws and mathematical theories to explain or predict empirical facts). In fact, given the eagerness of many historians of science to emulate what they consider to be the methods of "general" historians, it is ironic that my colleagues seem to avoid the kind of causal questions that specialists in, say, the seventeenth century English Revolution, find worthy of serious research and debate.

THE NATURE OF SCIENCE

In the 1970s there was a brief flirtation between historians and philosophers of science; each group thought it might learn something useful from the other. Philosophers of science were tired of arguing with each other about how science *should* work and decided they should take some account of how science actually *does* work, now and in the past. Historians of science welcomed this movement at first because it promised to fill a perceived need for some *theory* to explain or at least rationalize the large amount of descriptive data they had collected on the behavior of scientists. If it were possible to establish a philosophically-respectable theory of the nature of science by historical work, one might even be able to predict how science would develop in the future.

10 James McClellan and Harold Dorn, *Science and Technology in World History: An Introduction* (Johns Hopkins University Press, 1999), p. 137; see also pp. 115, 139. Toby E. Huff, *The Rise of Early Modern Science: Islam, China, and the West* (New York: Cambridge University Press, 1999; 2nd ed., 2003)

11 One way to avoid the question is to deny that there *was* a Scientific Revolution in seventeenth century Europe. Judging by the ever-increasing demand for and supply of books about the supposedly nonexistent event, I would say that strategy has not yet been successful.

Although the flirtation gave birth to some academic programs, books and journals (one of which, *Studies in History and Philosophy of Science*, has been quite successful), it did not lead to a stable marriage. Historians decided "science" is not a well-defined entity that persists unchanged through time, hence it cannot have a unique "nature" and there is no need for a theory of its historical development. Philosophers still believe that science does have a nature but decided the best way to determine that nature is by logical analysis, rather than tedious archival research on past science. Science educators also want to know the nature of science because they think that's what they should be teaching in the classroom, not just the results of scientific research.[12]

Nevertheless, a few problems on the borderline of history and philosophy are still generating research and discussion within both disciplines. In particular, philosophical analysis of theory-confirmation has interacted with historical studies of the reception of theories. For example, I claim that the following is an important historical question, even though it is usually discussed only by philosophers, not by historians: in deciding whether to accept a proposed theory, do scientists (now and in the past) give more weight to the successful *prediction* of *new* facts than to the successful *explanation* of *known* facts? Historians who study the reception of scientific theories are best able to answer this question because they have the evidence right in front of them; but unless they recognize the importance of the question they may simply report *who* accepted or rejected the theory without investigating *why*. We have here another causality issue, this time on the level of individual scientists rather than entire socities or nations.

Historians should not simply point the philosophers in the direction of the archives of scientific writings (published and unpublished) because philosophers are not looking for the kind of answer that would be useful to historians. The philosopher is likely to be an absolutist who wants *the* answer, valid at all times and places. The historian would suspect, rightly I believe, that the answer varies from one field of science to another, and within each field may change from one time to another. It is precisely the way in which it changes – whether, for example, nineteenth-century physicists are more or less likely to judge theories by their successful predictions than seventeenth-century physicists or twentieth-century biologists – that tell us something worth knowing about the history of science.

This particular historico-philosophical question also turns out to be of considerable contemporary interest when it is used, as the philosopher Karl Popper proposed, as a criterion to demarcate science from pseudoscience. If a theory does not make testable predictions – if it cannot be verified by an actual experiment or observation – then it does not deserve to be called scientific. The criterion has been used by both sides in the creation-evolution debate; it has been used to undermine

12 See Randy Bell *et al.*, "The Nature of Science and Science Education: A Bibliography," *Science & Education* 10 (2001): 187–204.

the credibility of disciplines like psychoanalysis; and it has been enshrined in an Opinion of the U. S. Supreme Court (the *Daubert* case).

In my opinion Popper's "falsifiability criterion" is itself false: sciences widely acknowledged as legitimate (evolutionary biology, historical geology and much of astronomy) do not generally satisfy it: they deal with phenomena in large domains of space and time, which cannot easily be brought into the laboratory for controlled experiments. As a historian I have to conclude (from my own research and that of others) that even in fields where predictions can be tested, the results of those tests do not usually determine whether the theory is accepted; other factors such as the explanatory power and esthetic beauty of the theory may be equally or more important. Social and psychological factors may play a significant role.[13]

Nevertheless, the frequent public statements glorifying the hypothetico-deductive method as the key to the success of modern science have led some younger or inexperienced scientists to believe that confirmation of a novel prediction based on a bold hypothesis is the quickest way to establish their reputation. This belief can lead them astray. As Joachim Dagg has suggested, "misunderstanding the predictive power of science as a sort of guarantee to be right may be the primary motive for forgery". Even without any dishonorable intent, a scientist may unconsciously focus on empirical data that support the hypothesis and ignore or minimize data that refute it – behavior that psychologists call "confirmation bias". This phenomenon may explain some of the frauds involving apparently respectable scientists.[14]

Thus we have two propositions about the Nature of Science: (1) scientists do not in general accept a theory primarily because it has led to successful novel predictions; (2) the belief that they do so because of publicity about the "scientific method," is one reason why scientists may (unintentionally?) falsely report that a theory has been confirmed by experiment. Neither proposition has been conclusively established, but they are attractive targets for future *historical* research, even though they derive from a *philosophical* claim about science. If the propositions turn out to be valid and if their validity is made known to science educators and to the

13 See S. G. Brush, "Why was Relativity Accepted?" (cited in note 4); "Dynamics of Theory Change: The Role of Predictions," *PSA 1994* 2 (1995): 133–145; "How Theories Became Knowledge: Morgan's Chromosome Theory of Heredity in America and Britain," *Journal of the History of Biology* 35 (2002): 471–535, and other papers cited therein.

14 Joachim L. Dagg, "Forgery: Prediction's Vile Twin," *Science* 302 (2003); 783–784. On "confirmation bias" see Ryan D. Tweney, Michael E. Doherty & Clifford R. Mynatt, *On Scientific Thinking* (New York: Columbia University Press, 1981); M. J. Mahoney, *Scientist as Subject*: *The Psychological Imperative* (New York:: Ballinger, 1976); P. C. Wason, "On the Failure to Eliminate Hypotheses in a Conceptual Task," *Quarterly Journal of Experimental Psychology* 12 (1960): 1290–1340. The problem seems to have been recognized as early as 1933; see the remarks by Ernest Rutherford about the discovery of the positron following its prediction by Dirac, at the Solvay Congress held in that year, quoted by D. V. Skobeltzyn in *Early History of Cosmic Ray Studies*, edited by Y. Sekido & H. Elliot, p. 50 (Dordrecht: Reidel, 1985). Cf. A. P. French, "The Strange Case of Emil Rupp," *Physics in Perspective* 1 (1999): 3–21.

public, the future of science itself might be affected.[15] In this case, the historian of science would be not just a passive observer of science but would play a more active role. However, that happens only if the historian is willing to go beyond mere description of *what* happened (in this case, a theory was accepted) and try to analyze *why* it happened.[16]

IS MATHEMATICS THE KEY TO THE UNIVERSE?

And what about the mathematics of sport, the other interdisciplinary subject mentioned by the *Washington Post* columnist? As it happens, that subject is relevant to a curious connection between recent and modern cosmology, a connection that also shows why historians of science should pay more attention to mathematics.

In his *Timaeus*, Plato identified the regular solids (cube, icosahedron, octahedron, tetrahedron) with the four elements (earth, water, air, fire); the fifth solid, the dodecahedron, was identified with the cosmos. Commentators on *Timaeus* have suggested that Plato had in mind a popular game involving a ball made by sewing together 12 pentagonal pieces of leather (like a modern soccer ball). That would be a dodecahedron if the pieces were rigid and flat but because of the elasticity of the leather, the pieces bulge out to form a sphere when air is pumped into the ball. This would be a simple, practical way to make a model of the celestial sphere but

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15 "One big problem with science fairs is that everybody tries to force-fit students into the mold of what they call'the scientific method' [hypothesis-testing]" – Randy Bell, University of Virginia, quoted by Valerie Strauss, "Science-Fair Hypothesis Fraying", *Washington Post*, 20 February 2001, p. A9. The notion that the validity of a philosophical concept like the confirmatory value of novel predictions could enter public discourse is not quite as far-fetched as it sounds. Consider the following exchange published in *Parade* magazine, a Sunday newspaper supplement that reaches millions of readers: "I recently read that the ancient Babylonians could accurately predict solar and lunar eclipses. But how was that possible if it was not yet known that the Earth actually traveled around the Sun, rather than the other way around. – Scott Morris, Highland, Ind. [Reply:] "They didn't need to know *why* the eclipse was occurring… [They] assembled meticulous observational tables for so long that, even though they thought the Sun revolved around the Earth, they still had great success in predicting eclipses. *This is an excellent example of how prediction–widely accepted by scientists as the truest test of the accuracy of a theory–is utterly inadequate…*". Marilyn Vos Savant, "Ask Marilyn," *Parade*, 10 August 1997, pp. 4–5 (italics in original).

16 A well-known example is the influence of Kuhn's theory of scientific revolutions, published in 1962, on the rhetoric of geophysicists involved in the "Revolution in the Earth Sciences" that revived continental drift theory under the name of Plate Tectonics. By appealing to [Kuhn's view of] the history of science, they helped to establish [their view of] the history of the Earth. It is consistent with one interpretation of quantum mechanics, according to which any observation of a phenomenon has an effect on the phenomenon itself.

Two recent examples of attempts to go beyond descriptive narrative and analyze how science works are: David L. Hull, "Studying the Study of Science Scientifically," *Perspectives on Science* 6 (1998) 209–231; Frank J. Sulloway, *Born to Rebel: Birth Order, Family Dynamics, and Creative Lives* (New York: Pantheon, 1996).

the choice of the dodecahedron rather than some other solid involves theoretical considerations in 3-dimensional geometry.[17]

All historians of science are familiar with Kepler's use of the five regular solids in his first model of the solar system and with the role of Platonic/Pythagorean thinking in the works of Galileo and other physical scientists. But now, we have a more specific question, which only an historian who takes mathematics seriously can discuss: why did Jean-Pierre Luminet and his colleagues select the dodecahedron (more precisely, the sphericalized "Poincaré dodecahedral space", which looks like a soccer ball) to represent the universe, in a paper that one of the world's most prestigious scientific journals not only accepted for publication but featured as its cover story? *Nature* 425 (2003): 593–595. How is their reasoning related to that of Plato? Having rejected the "whig interpretation of the history of science," we cannot ignore this question just because other cosmologists are skeptical about the validity of the Luminet model, and it may be forgotten in a few months.

The case of the dodecahedral universe is only an extreme example of a more common phenomenon that deserves more attention from historians of science: what Eugene Wigner called, in the title of a famous paper "The unreasonable effectiveness of mathematics in the natural sciences".[18] Some of the most revolutionary ideas in modern physical science were introduced first as purely mathematical hypotheses that contradicted well-established views about the nature of the world, yet could not be ignored because they led to superior explanations and predictions of empirical facts.

Astronomers had to use the Copernican system in their calculations because the planets seemed to move *as if* they were going around the Sun, not the Earth, even though in the late sixteenth century most of them could not accept the absurd idea that the Earth itself moves around the Sun as well as around its own axis. They could "exploit Copernicus' mathematical system… while denying or remaining silent about the motion of the Earth" with the result that "the final victory of the *De Revolutionibus* was achieved by infiltration".[19] Eventually, since the mathematical hypothesis

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17 "The dodecahedron was familiar to anyone familiar with the construction of balls out of twelve pentagonal pieces of leather… Of the five solids inscribed in one and the same sphere the dodecahedron has the maximum volume and comes nearest to coinciding with the sphere, as well as looking most like it in shape. So the *Phaedo* (110 b6) compares the spherical Earth with… balls made by sewing twelve [pentagonal] pieces of leather together." Another possibility is an "allusion to the mapping out of the apparently spherical heavens into twelve pentagonal regions for the purpose of charting the constellations." A. E. Taylor, *A Commentary on Plato's* Timaeus (Oxford: Clarendon Press, 1928), pp. 359, 377. See also F. M. Cornford, *Plato's Cosmology: The Timaeus of Plato translated with a running commentary* (London: Routledge & Kegan Paul, 1937), p. 219.

18 E. P. Wigner, *Communications on Pure and Applied Mathematics* 13 (1960): 601–614.

19 Thomas S. Kuhn, *The Copernican Revolution* (Cambridge, MA: Harvard University Press, 1957), pp. 185–187. Kuhn argues that the "as if" attitude of these sixteenth-century astronomers was similar to that of their predecessors: "Ptolemy himself had never pretended that all of the circles used in the *Almagest* to compute planetary positions were physically real; they were useful mathematical devices and they did not have to be any more than that" (p. 187). Kuhn's interpretation has been

of the Earth's motion was incompatible with the physics of Aristotle, Galileo invented a new physics that would be consistent with the mathematical hypothesis.

Galileo, however, refrained from proposing a comprehensive philosophy to replace Aristotle's; that task was left to Descartes. Descartes developed a mechanistic picture of the universe in which space was completely filled with matter; pieces of matter could push each other around by contact action but could not exert any forces over a finite distance. Indeed, the idea of "action at a distance" was condemned by the Cartesians as an unscientific remnant of the "occult qualities" and magical mysticism popular in earlier centuries. Isaac Newton agreed in principle with this view but found that he could explain planetary motion and other phenomena quite effectively by postulating a universal force of gravity. Falling apples, the Moon, planets, comets and the oceans behaved *as if* they were subject to a force acting through empty space, even though everyone, including Newton, knew that there could not in reality be such a force. Of course, Newton did not consistently reject all non-contact forces, but he did express his distaste for the idea that the Sun could simply reach out over millions of miles to pull the Earth, without the help of an intervening substance. According to Alexandre Koyrè, "Newton… never admitted attraction as a "physical" force. Time and again he said that it was only a "mathematical force".[20] However, like the Earth's motion, Newton's theory of gravity was so successful that it had to be accepted, even though the major continental physicists like Huygens and Leibniz did so with the stipulation that gravity itself does not exist as an inherent property of matter. It was only when the next generation had translated Newton's theory into the language of Leibnizian calculus, and found that it was also far superior to Descartes' vortex theory in dealing with problems such as the return of Halley's comet, the orbit of the Moon, and the shape of the Earth, that the Continentals shrugged off their antipathy to long-range forces; mathematics had again infiltrated physics and forced it to change its fundamental principles.[21]

A similar story could be told about atomic randomness, the photon, quantum entanglement, antiparticles and general relativity – based, like the above examples, on facts well known to historians of science. But in each case where historians find that a concept was first introduced as a mathematical hypothesis that could not represent physical reality, then was later accepted as real because of its empirical success, there is a tendency to treat it as an anomaly, rather than an instance of what might be a general rule. What is lacking is an adequate recognition and analysis of the *creative role of*

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somewhat modified by subsequent historical research but his basic premise, as applied to the reception of Copernicus by German astronomers, is supported by the detailed studies of Robert S. Westman; see for example "The Melanchthon Circle, Rheticus, and the Wittenberg Interpretation of the Copernican Theory," *Isis* 66 (1975): 165–193.

20 *Newtonian Studies* (Cambridge, MA: Harvard University Poress, 1965; reprint, Chicago: University of Chicago Press, 1968), p. 7.

21 E. J. Aiton, "The Vortex Theory in Competition with Newtonian Celestial Mechanics," in *The General History of Astronomy*, Volume 2, *Planetary Astronomy from the Renaissance to the Rise of Astrophysics*, Part B, *The Eighteenth and Nineteenth Centuries*, edited by R. Taton & C. Wilson, 3–21 (New York: Cambridge University Press, 1995); Koffi Maggio, "The Reception of Newton's Gravitational Theory by Huygens, Varignon, and Maupertuis: How Normal Science may be Revolutionary," *Perspectives on Science* 11 (2003): 135–169.

mathematics in the development of physical science. Instead, the current fashion is to emphasize the role of laboratory experiments. Granted, this role has been neglected in the past, when historians wrote mainly about theories and concepts and a lot more research needs to be done to correct that imbalance. My point is not that experiments need less attention from historians but that when we do discuss theories we should consider mathematical concepts as more than just convenient fictions.[22] In particular, we should recognize the possibility that at least in some cases, the mathematics is not merely a tool to express a new physical idea and deduce its empirical consequences; rather, mathematics may run ahead of physics, forcing physicists to use and eventually to accept a new concept they initially rejected.

In short, historians of science have not really "taken on board" (to use a current cliché) the remarkable statement of Albert Einstein:

> Nature is the realization of the simplest conceivable mathematical ideas. I am convinced that we can discover, by means of purely mathematical constructions, those concepts and those lawful connections between them, which furnish the key to the understanding of natural phenomena. Experience may suggest the appropriate mathematical concepts, but they most certainly cannot be deduced from it. Experience remains, of course, the sole criterion of physical utility of a mathematical construction. But the creative principle resides in mathematics. In a certain sense, therefore, I hold it true that pure thought can grasp reality, as the ancients dreamed.[23]

Institute for Physical Science & Technology and Department of History
University of Maryland
College Park, MD 20742, US

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22 There is also a Contextualist aspect here, which is often forgotten (by me as well as by other historians): how did the scientist learn the mathematics that was to prove so useful? We now have some good accounts of the way physicists prepared for the Tripos examinations at Cambridge University, including what kinds of mathematics they would have encountered there. James Clerk Maxwell certainly profited from this kind of education, yet he apparently got the idea for his remarkable derivation of the velocity-distribution law before he went up to Cambridge by reading John Herschel's lengthy review of Quetelet's books on social statistics. It was the success of this law, which postulated that molecules in a gas behave *as if* they moved randomly, that infiltrated the idea of atomic randomness into physics at a time when physicists generally assumed that atomic motion was governed by deterministic Newtonian mechanics.

23 Albert Einstein, *On the Method of Theoretical Physics: The Herbert Spencer Lectures delivered at Oxford, June 10, 1933* (Oxford: Clarendon Press, 1933). Part of the context for this statement is Einstein's own experience during the previous two decades: he followed a mathematical path to deduce the equations of general relativity, then found that when applied to the universe they entailed an unacceptable consequence: a static collection of massive bodies would be unstable because there was nothing to prevent gravitational forces from making them collapse into a small space. So for *physical* reasons he added the arbitrary "cosmological constant," in effect a long-range repulsive force to prevent this collapse. The subsequent discovery that the universe is expanding made this correction unnecessary, so Einstein retracted the correction. Introducing the cosmological constant was what he later called his "biggest blunder" [according to George Gamow, *My World Line* (New York: Viking, 1970), p. 44] – i.e., to let the physics override the mathematics. Einstein's views on the role of mathematics in science are discussed by Christa Jungnickel and Russell McCormmach, *Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein*, vol. 2 (Chicago: University of Chicago Press, 1986), pp. 334–347.