



Of What Use is the History of Science?

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April 13 '08

Pais Prize Lecture: **Of What Use is the History of Science?**

Gerald Holton

I am deeply grateful for the honor extended to me here, and rather overwhelmed by it, especially as the Prize is in memory of Bram Pais, who was an extraordinary physicist, and also who has no peer as a historian of physics. His countless essays and thirteen books helped to define the field, and set the highest bar. I knew him quite well. We also served together on the committee that helped turn out the first volumes of the Correspondence of Albert Einstein. I am glad to say that in our extensive correspondence he chose to be kind about my own work. He was, in a way, the modern conscience about the history of science; and it is in this spirit that I wish to make two points, both concerning the fact that history matters, especially in this somber time, for so many, in physics, and beyond.

If you heard Leo Kadanoff's speech yesterday (shortly to be on his website), you know that this society and its members have a big task now to stem the decline in research funding, in status, in education, in general scientific literacy of the public—not only for ourselves but also for our country.

What I have to say here may, I do believe, give added conviction and authority to those who want to be effective in this difficult task.

My first point concerns the sense of self, the intellectual identity, of each of us individually; and the second, related, point will be about our opportunity, perhaps even duty, to our students.

As to the first: physicists and other scientists tend to be quite understandably oriented above all to the future of their field rather than to the

past. Such are the characteristic identities of pioneers at a frontier, rather than as scholars focusing on the past.

Let me illustrate this view, together with a rare conversion experience. In 1972, the Enrico Fermi summer school of physics was held in Varenna, on Lake Como. The topic was “The History of 20th-century Physics.” The faculty for this school consisted of a small group of physicists and historians of science. To our delight, P. A. M. Dirac agreed to be part of the faculty. We, the faculty, all met before the school started, to synchronize our work. Dirac listened intently, and finally spoke up in his quiet way, saying “I don’t understand why there should be a history of physics. Either a thing happened, or it did not.” This produced panic among the rest of us.

Near the end of the summer school’s term, Dirac gave a set of lectures to our students, saying at the start: “I have learned a great deal here at Varenna... I have learned to appreciate the point of view of the historian of science.... [By contrast,] the research physicist wants rather to *forget* the way by which he attained this discovery.... He feels perhaps a bit ashamed, disgusted with himself, that he took so long....

“However, with the understanding of what the historians of science are concerned with, I have tried to think over the past....[and] how these things led me to the style of work, which I followed later in life.” And then Dirac gave a splendid set of three historical lectures, “Recollections of an Exciting Era,” which were published later.

To be sure, few scientists have experienced a conversion like Dirac’s. Only rarely is a researcher interested in reading one of the publications on the history of science, or for that matter reading a physics paper or volume published many decades in the past—as was done by I. I. Rabi. He wrote once that one day he happened to be reading, for sheer pleasure, Maxwell’s Treatise

of 1873. That gave him a clue for quickly measuring the magnetic susceptibility of a crystal, a central question in his research project at the time. It was for him not the only time that history helped to make a present puzzle become a future solution.

For Rabi, and for relatively few physical scientists today, such as Steve Weinberg and Freeman Dyson, a sense of the historical development leading up to their current physics preoccupation has been important for a more comprehensive sense of self. And, I maintain, it should be so for far more scientists. For in truth, for each of us, the science research project of today is the temporary culmination of a very long, hard-fought struggle by a largely invisible community of our ancestors. Each of us may be standing on the shoulders of giants; more often we stand on the graves of our predecessors. To know nothing about them is, to me, as limiting in one's self-regard as not knowing one's actual parents.

I was lucky to realize this simple fact as a PhD student under P. W. Bridgman. He was not only that hard-driving experimental physicist, who got his Nobel in 1946, and who also eloquently wrote on what was called the operationalist approach to the methodology of science. But the first thing a new student of his would do was to read his great text, The Physics of High Pressure. And there, his first chapter is titled "Historical Introduction"--29 pages on the great sequence of prior high-pressure experimenters, some 75 of them, from Hans Christian Oersted in 1823 on. This was one example of acknowledging the serious debt any advance pays to its genetic forebears.

To illustrate further, let me refer to work I did with two research associates some time ago, and published under the title "How a Scientific

Discovery is Made.”¹ As you know, in 1986 and ’87, there appeared out of the blue several papers on high-temperature superconductivity, by the Swiss physicist Alex Müller, formerly a student in Wolfgang Pauli’s classes, and by Müller’s former student, Johannes Georg Bednorz. Starting in 1983, at the IBM lab in Switzerland, they worked rather secretly, in order that if they failed, they could, as Müller told me, give their work a “burial in very restricted family circumstances, so as not to jeopardize Bednorz’s career.” Yet they caused a real sensation when they announced their findings. They had broken through a long-standing barrier, reaching superconductivity, first at about 30 Kelvin, with the completely unconventional use of a ceramic compound which had a perovskite structure. Others quickly converged on this field, and pushed the transition temperature to about 160 degrees Kelvin.

I got interested in just how Müller and Bednorz made their discovery. Specifically, what had been the historic treasury of intellectual and material resources that were available to them and used by them? Happily, both men cooperated with us in giving us interviews and exchanging letters. I especially wanted to know how they fitted into the grand, age-old network of available knowledge on the way to the new knowledge. How did their work fit into the big jigsaw puzzle whose pieces were prepared by previous advances?

So we traced, in their own key publications, the explicit and implicit serious citations. Then we looked at the explicit and implicit citations in the publications of those immediate ancestors; and in fact we went further back in this way for a total of about four generations.

Analyzing the original five papers that comprised the announcement of their breakthrough revealed the number of silent resources which they had put to use: for example, tools for observations of standard techniques which are no longer referred

¹ See *American Scientist*, v. 84, no. 4 (July-August 1997), pp. 364-375.

to explicitly, such as x-ray diffraction (von Laue) or the criteria for identifying superconductivity, namely zero electrical resistance (by Kamerlingh Onnes in 1911), and the Meissner effect, implicitly referring to a 1933 publication. Similarly, the platinum thermometers which Müller's team used, imply references to an 1887 publication by one Hugh L. Callendar of the Cavendish Lab, which ushered in the platinum resistance thermometer as a practical means of measuring temperature.

The origins of the apparatus Müller's team used to liquefy helium stems of course from the principles of cooling, laid out first by the British physicist, William Thomson and James Joule in the 1850s, and by the French chemists, Nicolas Clément and Charles-Bernard Desormes, in 1819. And so forth. Unwittingly but documentably, the stage for Müller and Bednorz's discovery in the 1980s was set by scientists, many long dead, if not forgotten.

And there was one special ancestor in Müller's work: Johannes Kepler. Müller told me that he had an unusual fascination with perovskites, which have a very high symmetry, and which he had used with great success in many other research projects. This fascination had originally stemmed from his having been a student in Wolfgang Pauli's class, when Pauli was sharing his ideas on an essay he was writing on Johannes Kepler and Kepler's archetypes, especially those five Platonic, highly symmetrical bodies. So it turns out that Kepler helped lead to high-temperature superconductivity.

From these and many other examples we can generalize that any significant advance relies, not vaguely but documentably, on a large, international, identifiable set of earlier contributions, all serving the emergence of new science or technological achievement.

This fact is also a support for that old assumption that there *is* some underlying unity in science and technology, not a unity found by one grand

synthesis, but a different unity, an operational one, in which interlinking parts of science and technology help one another.

The lesson here is that Dirac was right in his advice in his Varenna lectures. Indeed, every advance reported in an APS meeting or publication is a new fruit on an old family tree, one with many branches, near and far. Moreover, these long-gestated fruits of science have nourished not only current physicists, but were, and continue to be, crucial help for other sciences, for applications--and for the forces on behalf of enlightenment, of reason and sanity, and potentially for the upgrading of the human condition.

In this recognition lies a large part of the moral authority of one's profession. And when not enough scientists assert it, others rush in, to define it in their destructive way, as they have done again and again. I dare to confess frankly that a good part of the reasons for my doing some of the things this award asserted about my activities has been largely motivated by the view that our physical sciences, when seen through the double lenses of the achieved present and the painful development over centuries, are at least as important a part of humanity's culture and long-term health as any other enterprise.

Of course, at this point I hear some skeptical voices. For many scientists, the adrenaline of the day-to-day excitement in the lab is quite enough to feel utterly secure within themselves. Others make do very well with a combination of good work at the bench or desk, plus important public service, like many of our role models, or those who battle the tone-deaf administrators and the scientific deniers of our time.

Taking on these roles is of course needed too, and is fulfilling for those who do, and crucial for the rest of us. But there is at least one role that seems to me to require from the scientist a living sensitivity and witness of serving as a link of a grand chain of being. This role is that of educator.

And so I come to my second point: how best to attend to the opportunity, perhaps duty, that we may have to our students.

If you accept the suggestion that many working scientists deserve a larger, more secure sense of identity, being confident beneficiaries of the past and contributors to the present culture and civilization, it follows that they have also an opportunity to help their young colleagues and students, for them at least to glimpse their own role in this great venture. This can be done easily when one is teaching physics, where we convey to students many of the great breakthroughs, from Galileo to Feynman and to today's. What I am about to suggest applies to any of these, but let me concentrate for a moment on the opportunity to teach Relativity Theory in this mode, as one example.

Students usually look forward to being introduced to this topic, and there are by now hundreds of ways to present to them the main concepts and equations, and their uses. That must be done. But many instructors have found there is in addition even more excitement and result, by making a little room to give students also a glimpse of why and how this theory came about, and became a key part of physical science.

Even in Einstein's own writings, it is easy to find what he regarded the immediate antecedents of his theory. I would recommend turning to one of Einstein's early love letters to his future wife, Mileva Maric. Writing in August 1899, he says he has been reading Hertz on Maxwell's theory, and he presents to Mileva his conclusion: "The introduction of the word 'Ether' in the electric theory has led to the conception of a medium of whose motion one can talk, without, I believe, connecting with that assertion a physical sense."

So, in 1899, six years before his 1905 paper, he already had the audacity to dismiss the ether. Later, Einstein added that the Fizeau experiment

of 1851, stellar aberration, and Faraday's induction experiment were the critical antecedents to his own work. And in his autobiography, written in 1946, he added that his early self-education included reading Kirchhoff and Helmholtz, especially on Maxwell's theory. Indeed, he referred to his own approach to physics as the Maxwellian Program.

Now that we have begun to make the student aware of some of the steps, so to speak, of a ladder down which relativity came into being in Einstein's mind, we can stop at this important point to explain what in Einstein's view is that Maxwellian program. It is of course an exemplification and realization of the oldest motivating force in physics, namely, the attempt at a grand synthesis, at a unification of disparate elements, a tradition I have liked to call the Ionian Enchantment, going far back in time.

In a way, some of the most recent works being presented at this APS meeting are children of that great family dynasty, the movement toward unification within a branch of science, going back to, among others, the Vienna Circle for the Unity of Science, then further back to the syntheses worked on by Maxwell, by Faraday, by Oersted, by Kant and the Nature Philosopher-- all the way back to Newton, who in his Preface to the *Principia* said he hoped that by mechanical principles one could "derive the rest of the phenomena of nature," and ultimately back to Thales of Miletus in ancient Greece; and then forward to what Einstein initially called the generalized Relativity Theory, and on to today's ideas of a theory of the synthesis of all forces. Giving some idea of this grand arc is showing science as a living being, with huge energy, struggles, despair, visions, vexations, and victories.

In short, when students are dealing with the work of any of those who helped our current science to be born, they should see that physics, through the centuries-long application of rationality, intuition, and skill, has achieved a high degree of organic coherence, rather than being just one thing after another, like those separate chapters in so many textbooks. So, should not at least some of us, when teaching, for example, about Einstein's work as reflected in his equations, let it be heard also that Einstein himself noted [in 1918, *Prinzipien der Forschung*] that "the supreme task of a physicist," as of any intellectual, is to form "a coherent and lucid world picture"? And, for that matter, should it not be heard also that Einstein urged a fierce defense of science as well as upgrading the conditions of mankind? Would that not add greatly to the sense of self of future scientists, a sense which may be diminished if they see their main purpose only to do yet another narrow set of assigned tasks? And, just possibly, may this larger self-confidence, as sons and daughters of an extraordinary family, would that not allow them, in this area of unreason and neglect, to act when necessary, on behalf of our profession—and beyond?

Dear friends and colleagues, having shared a call of conscience in Bram Pais' spirit, I thank you again for this singular honor, and for your attention.

April 13, 2008

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