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Physics in Perspective

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Shifting Trends in Modern Physics, Nobel Recognition, and the Histories That We Write

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Since the late-nineteenth century, scientists have been labeled with disciplinary fields and scientific achievements have been identified largely with heroic individuals. Reward systems such as the highly visible Nobel Prizes have reinforced such a view of science. This paper examines long-term trends in Nobel Physics awards since 1901 and asks whether the awards have registered the increasing specialization, collaboration, and transdisciplinary research that mark the course of modern physics. A second major question is the extent to which, in turn, histories of physics since the 1960s have reflected trends in physics or in Nobel recognition. Historians of physics appear to have favored accounts of particle physics and relativity theory over other areas of physics, with biography remaining a strong tradition in the history of physics, even while institutional and social history has become significant. Concluding remarks address hierarchies of prestige in science, the accessible and emotional appeal of heroic and revolutionary accounts of science, and the perennial appeal of fundamental questions, like reductionism and emergence.

Key words: Biography; collaboration; condensed matter physics; history of physics; Nobel Prizes; particle physics.

Every autumn, there is anticipation of the announcement of the year's Nobel Prizes, among them the Nobel Prize in Physics. In early October, physicists, chemists, science journalists, research administrators, and historians of physics wonder about the outcome. But what does the prize tell us? It tells us what some elite physicists consider to be the most important achievement, or two achievements, of the last few years or decades and what combination of one to three people receive Nobel credit for that work. We might ask, however: Do the prizes, over time, convey the range of research fields in physics or the different proportions of researchers in each field? Do the awards respect disciplinary boundaries or challenge them? How do factors other than the quality and significance of the work influence the Nobel decision? And how meaningful is the status as winner that comes to individuals, theories, and institutions through the Nobel Prizes?

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These are questions that we also might very well ask about the history of physics. In an important sense, historians of science do much the same kind of work as the Nobel process, and historians often observe something similar to the fifty-year cutoff rule that conceals the inner workings of nominations and Nobel Physics Committee deliberations from public view. There frequently is a notable lag time between the date that scientific work is first done and the time of either a Nobel award or of a historical narrative and interpretation. For many a younger historian, if it occurred in her lifetime, it is not history.

With this in mind, the aim of this inquiry is to analyze how accurately histories of modern physics have captured the various and shifting domains of physics, the changes in theories and practices, and the relative credit given different theorists and experimentalists. How, too, have histories of physics changed over the last few decades? The focus principally is on the physics of the twentieth century, which coincides with the beginning of the Nobel awards, and on histories of physics written since the 1960s, when the history of science became an expanding academic discipline practiced both by physicists writing history and by professional historians.

The first part of the analysis is a discussion of two aspects of twentieth-century physics that often are remarked upon: its fragmentation into specialties and its corresponding increase in collaborative authorship and teamwork, often transdisciplinary in character. I then turn to trends in the ways in which the Nobel Physics Prizes have made awards to individual physicists in different fields in physics for sets of theories and experiments. Finally, I consider how histories of physics, like the Nobel Prizes themselves, have sometimes distorted the history of physics as a whole—particularly who counts and what counts—but also how historians of physics have adapted to changes in physics and adopted historical approaches that vastly enrich the history of physics beyond a Nobel tale of heroic discovery and invention.

Specialization and Collaboration

In a recent article in *Physics Today*, the historian of physics David Kaiser writes of a keynote lecture that Karl Darrow, the atomic physicist and American Physical Society executive secretary from 1941–1967, gave at an American Institute of Physics meeting in Chicago in October 1951. Asked to speak on "The Whole of Physics," Darrow said that he found the task impossible, and that the last man who might have done this had been Hermann von Helmholtz at the end of the nineteenth century.[1]

As Kaiser writes, twentieth-century growth of physics, along with its fragmentation and specialization, is reflected by entries from the 1940s through the 1970s in *Physics Abstracts*. The number of published abstracts increased from about 4000 just after the Second World War to 7,500 in 1948 and then to 84,000 in 1971, doubling every six years from 1945 to 1971.[2] Around 1970, the number of physics PhDs in the United States hit its peak before a decline that briefly almost fully reversed for a while in the early 1990s.[3] Corresponding to the doubling every six years from 1945 to 1971 of abstracts published in *Physics Abstracts*, the number of subject areas within physics increased. In 1930, there had been eight main categories: general physics; meteorology, geophysics and astrophysics; light; radioactivity; heat; sound; electricity and magnetism (with four subdivisions); and chemical physics and electrochemistry. In 1955 nuclear physics appeared as a category, divided into six subfields; and by 1965 nuclear physics had thirty-five subcategories. Solid state physics, which became a named division within the APS in 1947, had thirty-eight subfields in 1965.[4]

Physics journals changed in response to these developments. Beginning in 1963, the *Physical Review* was printed in two sections, one for solid state, molecular, and atomic physics, and the second for nuclear and high energy physics. By 1967, the journal was divided into five parts, a pattern of sub-specialization followed by other physics journals such as *Physics Letters* and *Zeitschrift für Physik*.[5] By adding the study of the properties of liquids to solids in the 1960s, the category "condensed matter" became an umbrella category that included the solid state and was used in journals, physics societies, and research groups.[6] Approximately one-third of American physicists came to identify themselves as condensed matter physicists by the late 1990s, with problem domains and methods that often overlap with "materials science" and, more recently, with the "nanoscience" of materials ranging from a few atoms to one-hundred times larger in size.[7] Like their colleagues in nuclear and high energy physics, condensed matter physicists frequently make use of fundamental quantum mechanics, but unlike their quantum physics colleagues, who focus on elementary particles and fields, condensed matter physicists concern themselves with many-atom systems.

Associated with this increasing specialization in physics was the growth of collaborative research, a trend that also accelerated after the Second World War. Physics had seemed a heroic science during its first centuries, epitomized by giant figures such as Isaac Newton or Hermann von Helmholtz or Albert Einstein who could "do it all" and who "knew everything" while making revolutionary discoveries within their scientific communities. The heroic figure still was front and center just after the war when J. Robert Oppenheimer, the theoretical physicist and scientific director of the Manhattan Project's Los Alamos laboratory, became an iconic figure who could be identified publicly, as Kaiser notes, simply by a photograph of his porkpie hat, which appeared on the cover of the first issue of *Physics Today* in May 1948.[8]

In contrast, decades later, in 2012, when two experimental teams at CERN's large hadron collider simultaneously announced the experimental confirmation of the Higgs boson, formal recognition and authorship was collective. The teams, each composed of thousands of members affiliated with hundreds of different institutions, simultaneously announced their "observation" of the Higgs particle in the same issue of *Physics Letters B*, one team under the authorship of the "ATLAS

Collaboration," led by Fabiola Gianotti, and the other under the name of the "CMS Collaboration," led at that time by Guido Tonelli. The names of ATLAS team members, including Gianotti, were listed alphabetically, and CMS team members were listed alphabetically within institutional affiliations, with Tonelli listed alphabetically under the University of Pisa.[9]

Not all research teams are so large, of course, but single-authored research became increasingly uncommon during the course of the twentieth century. A few years before announcement of the Higgs boson confirmation, historian of science Mott Greene noted in an article on scientific biography that the most frequent number of co-authors on research papers in *Nature* and in *Science* in the early 2000s was eight.[10] As a historian, Greene worried that the rationale for the biographical organization of the history of science was coming to an end, a worry that, in fact, was not new. It surfaced around 1960 when sociologist Derek Price noticed that 80% of papers in chemical science were multi-authored in contrast to only 20% around 1900. Price reported that the average number of co-authors in chemistry had increased from roughly 2.7 in 1946 to 9.5 in 1963. He melodramatically predicted: "if the trend holds … by 1980 … we shall move steadily towards an infinity of authors per paper."[11] Price used the term "Big Science" to characterize what was happening, as did Alvin Weinberg, the director of the Oak Ridge National Laboratory, in what became a famous article by Weinberg in *Science* in 1961.[12]

As noted by historian of physics Peter Galison, the Brookhaven Laboratory particle physicist Alan M. Thorndike captured this strongly collaborative character of modern scientific research in 1967: "The experimenter, then, is not one person, but a composite. He might be three, more likely five or eight, possibly … more. He may be spread around geographically, although more often than not, all of him will be at one or two institutions.… He may be ephemeral, with a shifting and open-ended membership … he is a social phenomenon.… One thing, however, he certainly is not. He is not the traditional image of a cloistered scientist working in isolation at his laboratory bench."[13] Thorndike himself was one of thirty-three authors on the announcement of the first observation of the omega-minus particle, considered a crucial confirmation of the strangeness, eight-fold way, and quark theories for which Murray Gell-Mann received the Nobel Prize in 1969.[14]

Modern collaborative research has many advantages. Some things can only be discovered collaboratively and there is evidence that collaborative work generally assures higher quality of results and wider recognition, for example in prizes and awards.[15] The expanding scale of collaboration in the twentieth century reflects many factors, foremost among them increasing specialization within research groups or teams in which new kinds of instruments and computers required expertise or computing skills that the laboratory director or project manager might not personally possess. Such collaboration often has resulted from the identification of transdisciplinary research problems that require interdisciplinary, or what Lindley Darden and Carl Craver term *interfield*, strategies for discovery.[16]

One example from my own historical work is the Oxford University X-ray crystallographer Dorothy Crowfoot Hodgkin, whose structural determinations for penicillin, Vitamin B_{12}, and insulin from the 1940s through 1960s included collaborators at Imperial Chemical Industries, Princeton University, and the University of California, Los Angeles, in addition to her Oxford research group.[17] Collaborative research also flourished after the 1940s because multiply resourced funds from government, industry, and the military became available on a scale unimaginable before the Cold War, at the same time that travel and communication became increasingly easier, not only physically, but also digitally.[18]

Trends in Nobel Prizes in Physics

How do the Nobel Prizes, as a highly visible recognition and account of scientific work, mirror these developments in specialization, collaboration, and transdisciplinary research that occurred during the course of the twentieth century? An easy and quick way to identify leading fields of research and breakthrough achievements in physics has long been the Nobel Prize. It is perhaps the foremost of international awards and certainly the one that garners the most public attention, for good or ill. The Royal Swedish Academy of Sciences has awarded the prize since 1901 on recommendation from its Nobel Physics Committee, which is formally composed of five voting elected academy members and a secretary.[19] The committee solicits nominations made by themselves and other Academy members, former laureates, and eminent professors in physics in Nordic countries and elsewhere.[20] The originally Western geographical base for the Prize gradually expanded, with the Physics Committee first soliciting nominations from Japanese physicists in 1910, for example, and from Indian scientists in 1929.[21]

The original intent of the Nobel Physics Prize was to recognize the most recent important discovery or invention in the domain of physics, although the notion of "recent" became extremely flexible in the hands of the academy.[22] From its beginning, the Physics Prizes helped establish the notion of progress, even revolution, in physics, with early awards given for relatively recent experiments and theories on radiations, radioactivity, atoms, and electrons. Negotiations between the academy's physics and chemistry committees account for some of the decisions about whether a prize, for example in radioactivity, would be given in chemistry or physics, but, as historian of physics Helge Kragh notes, the relationship between physics and chemistry was not symmetrical. Thus, over time, a large number of chemistry prizes were awarded to scientists whose work usually is considered physics, but not the other way around.[23]

Initially, however, the chemistry committee largely ignored physical chemistry and chemical physics on the grounds that this work was physics, despite early chemistry awards to physical chemists J. H. Van't Hoff and Svante Arrhenius.[24] The award of the 1936 Chemistry Prize to Peter Debye for studies of molecular structure by investigations on dipole moments and the diffraction of X-rays and electrons in gases went, like some other prizes, to someone who clearly considered himself a physicist.[25] Among the pioneers in applying quantum mechanical theory to molecules was Linus Pauling, who early in his career resisted the appellation of "chemical physicist" in favor of calling himself a theoretical chemist. Pauling's 1954 Chemistry Nobel Prize opened future prizes to applications of electron theory and quantum mechanics by scientists who often were truly transdisciplinary in their approaches.[26] The molecular physicist and physical chemist Bretislav Friedrich recently estimated that approximately one-third of Chemistry Nobel awards have honored physical/theoretical chemists or physicists.[27]

From the 1930s on, the physics prizes most often recognized work in radiation and spectroscopy, quantum mechanics and electrodynamics, and elementary particles (including the electron, proton, neutron, mesons, and leptons). Initially, astronomy and astrophysics were unofficially excluded from the physics prize, a trend reversed in 1967 by the award to Hans Bethe for his 1938 theory of stellar energy production, which had to do with nuclear physics.[28] Astrophysics and astronomy received five awards from 1974 to 2011, including the 1983 award to Subrahmanyan Chandrasekhar and William Fowler for investigations of the formation of chemical elements in the universe. Arguably the first Nobel award in condensed matter physics was the 1913 Prize to Heike Kamerlingh Onnes for low temperature studies and the production of liquid helium. Prizes for researches on superfluids, superconductors, crystals, and metals followed.[29] Since Nobel Prize winners subsequently become Nobel Prize nominators, an expanding number of condensed matter Nobelists presaged increased numbers of awards in this field.

The sociologists Yves Gingras and Matthew Wallace have analyzed trends in the physics awards for the years 1901–2000, using an eight-category scheme adapted from the classification developed by the American Institute of Physics. In a more recent analysis, Hamish Johnston, a physicsworld.com editor, published by the Institute of Physics in London, used seven categories to classify prizes.[30] Gingras and Wallace, on the one hand, and Johnston, on the other, both find that nuclear and particle physics account for at least one third of the Nobel Prizes in Physics by subdiscipline, dominating the awards in the 1950s and 1960s. In Johnston's scheme, nuclear and particle physics, in combination with quantum physics and atomic, molecular and optical physics, account for 57% of both prizes and laureates. Although more than one-third of American physicists now identify themselves as condensed matter physicists, their share of Nobel Prizes has been proportionately smaller as detailed in an infographic published by Johnston, which shows the percentage of awards and laureates in condensed matter physics at 22% through the year 2014. The Nobel Prize official website itself lists twenty-eight physicists as Nobel laureates in condensed matter physics for the period 1901–2014, but divides physics into forty fields and lists some laureates in more than one field.[31]

Thus, in various definitions and arrangements of physics fields, the Nobel Prize appears historically to have favored other fields over condensed matter physics, although this has been changing. It should be noted that chemistry awards also have recognized the condensed matter field with at least four awards, including the 1998 Prize to physicist Walter Kohn for his work on the electronic properties of metals and his development of density functional theory, which makes it possible to calculate quantum mechanical electronic structure by electronic density rather than by the many-body wave function.[32]

Shared prizes became more prevalent around 1950, with the last two unshared prizes awarded in 1991 to Pierre Gilles de Gennes for his studies of order and disorder phenomena in liquid crystals and polymers and in 1992 to Georges Charpak for his invention and development of a new kind of particle detector.[33] As mentioned above, a Nobel rule limits an award to no more than two achievements and three living scientists. Although the 1903 Prize was divided among three scientists—Pierre and Marie Curie and Henri Becquerel—a three-person award did not occur again until 1956 for William Shockley, John Bardeen, and Walter Brattain, recognizing their investigations at Bell Labs on semiconductors and their discovery of the transistor effect. Most physics awards before the 1950s went to a single person, reflecting the pre-1950s trends in publication, authorship, and attribution of leadership discussed earlier.[34] Until 2018, only two women had received the Nobel Physics Prize, in both cases as a shared prize, Marie Curie in 1903 and Maria Goeppert-Mayer in 1963, each of them married to a male physicist at the time. As Helge Kragh notes, since they each received a quarter of a prize, the grand total of women's Nobel Physics Prizes was one-half, a situation marginally changed by the October 2018 announcement of one-quarter of the Physics Prize to Donna Strickland for her development with Gérard Mourou of a method for amplifying high-intensity laser pulses. Like astrophysicist Jocelyn Bell Burnell, Strickland was a graduate student working with her adviser at the time of her initial discovery, but Bell Burnell was less fortunate by not sharing Nobel recognition with her advisor Antony Hewish for her discovery of pulsars.[35] Five women have received the Nobel Prize in Chemistry, the most recent also in 2018, as have twelve women in physiology or medicine.[36]

The role in recent physics of very large, internally specialized research teams has begun to bring into question the award of each Nobel Prize to no more than three individuals, most recently in the cases of the 2013 prize to theorists François Englert and Peter Higgs for the Higgs boson and the 2017 Prize to Rainer Weiss, Barry C. Barish, and Kip Thorne of the LIGO/VIRGO collaboration for the observation of gravitational waves.[37] Although the new, privately funded Fundamental Breakthrough Prizes have broken with this tradition, dividing a $3 million award among more than 1,000 contributors in the LIGO collaboration, it seems unlikely that scientists or the public will easily relinquish the notion of the standard bearer or gifted leader.[38] It is possible, nonetheless, that the Swedish Academy could change the rules and award a science prize to a group, as has been the case since 1904 with the Peace Prize, awarded by a committee chosen by the Norwegian Parliament.

Indeed, there was an attempt at collective authorship and responsibility at the very beginning of scientific institutions when the Paris Academy of Sciences was founded in 1666. The intention was to submerge individual egos to the general good and to give credit for individual members' contributions to the academy as a whole, as Francis Bacon originally had prescribed in the 1620s for the new science. The Paris Academy abandoned this formula in 1699, however, and began assigning credit to individual scientific contributors, as was always true of the Royal Society in London since its establishment in 1660, and has remained scientific tradition.[39]

Histories of Physics in Transition

Let us turn now to historians of physics and histories of physics. What has changed or remained the same in the history of science and science studies since the mid-twentieth century? Do histories of physics replicate in broad outline the history of Nobel Prizes? Historian of physics Richard Staley has argued that a focus on scientific revolutions and on physical theories has remained very strong over many decades in the history of science. He also notes the increased attention since the 1960s to social and political contexts of science, the experimental and material dimensions of science including the practices of research groups, and the institutions of big science.[40] There is less heroic history, and there are more heroines in recent histories, but the notions of intellectual genius and charismatic leadership nonetheless remain commanding tropes in histories of physics.

In the 1960s, when the history of science and science studies were becoming professional academic fields, the mainstream topics in histories of physics and in histories of science more generally were a repertoire of seventeenth- to early-twentieth-century physical science: classical atomic and molecular theory, quantum theory, and relativity, along with the Copernican and Newtonian "revolutions," as well as eighteenth and nineteenth century theories of electricity, heat, and light, and the chemical revolution. Thomas Kuhn's wildly popular book of 1962, *Structure of Scientific Revolutions*, focused on this very range of subjects and made them examples of scientific paradigms and paradigm change at the heart of engagement within the history and philosophy of science. These subjects remain a substantial part of histories of physical science today.[41]

Kuhn's *Structure*, like most other histories of science in the 1950s and 1960s, was mainly intellectual history, or the history of scientific ideas and theories. What was different and new was Kuhn's argument that scientists normally proceed in their everyday work by solving puzzles and not by doubting the overall current scientific framework or by trying to establish entirely new theories. Kuhn suggested that historians and philosophers need to be able better to explain how a scientific community arrives at any new theoretical framework, given what he characterized as the rigid set of theoretical presuppositions, experimental procedures, and organizational structures within which scientists learn their trade and routinely practice their work.[42]

Kuhn shifted many historians' interest away from a heroic history of ideas and individuals toward the history of groups and communities, that is, from histories of eureka moments of discovery to histories of communication, organization, and social context. The move toward social history and sociology of science was not Kuhn's doing alone, however. For one thing, the trend in the general historical profession of the 1960s led away from intellectual history toward social history.[43] Importantly for science historians, some influential scientists, such as Oak Ridge National Laboratory director Alvin Weinberg, themselves were speaking out in the 1960s about the organization of science and how it had changed in the new era of "Big Science" with consequences for how science is done.[44]

Among scientists I have studied who addressed these matters were the nuclear and cosmic-ray physicist Patrick Blackett, the X-ray crystallographer J. D. Bernal, and the physical chemist and chemical physicist Michael Polanyi.[45] Another scientist who directly affected the way in which historians and sociologists thought about science was the condensed matter physicist John Ziman, who set out self-consciously to re-orient historians and philosophers of science toward social studies of science and toward Ziman's own epistemological view that science is reliable and verifiable knowledge, reached by consensus, rather than transcendent truth revealed to genius.[46] With this perspective, some science historians began to ask how reliable science is constructed rather than how scientific truths are discovered, and many historians of science turned toward the study of scientific communities and institutions.

Take for example the journal *Historical Studies in the Physical Sciences* (*HSPS*), first established in 1969 under the editorship of historian Russell McCormmach. The journal's emphasis was mainly nineteenth- and twentieth-century physical science. Published articles were notable for their frequent focus on social history, alongside theoretical and experimental history.[47] From 1980 until 2008, under the editorship of Kuhn's former student John Heilbron, the journal also opened its pages to histories of the increasing variety of subfields of physics, with about 70% of its articles devoted to the twentieth century. Stellar energy, astrophysics, cosmic rays, and geophysics joined quantum physics and atomic theory in the journal, in the form of histories of theory and experiment, but also as accounts of an array of institutions and research groups that included Bell Labs, the Brookhaven Laboratory, the Geophysical Laboratory, CERN, the Lawrence Berkeley Laboratory, the Atomic Energy Commission and National Research Council, and military research.[48]

Among new specializations in physics, solid state physics debuted in *Historical Studies in the Physical Sciences* in 1981 in historian Lillian Hoddeson's article on the point-contact transistor, followed by other historians' articles on imperfect crystals, superconductivity, and the electron theory of metals.[49] In other words, *HSPS* was capturing faithfully the mainstreams of modern physics, as they had been developing since the Second World War, including the fields of solid state and condensed matter physics, practiced, like nuclear physics and particle physics, in increasingly complex networks of institutions and collaborations in university, industrial, military, and governmental settings.

Do these examples from one leading journal suggest that historians generally were writing about different fields of physics, especially condensed matter physics, in proportion to these fields' practice and strength among physicists? It appears that this is not the case. In *Physics in Perspective*, *Studies in History and Philosophy of Modern Physics,* and the *European Physical Journal H for Historical Perspectives on Contemporary Physics*, as well as in books and essays directed at both professional and popular audiences, it is nuclear physics, particle physics, quantum physics, relativity, and their founding heroic pioneers that by and large have remained favored genres in history of physics. As Richard Staley wrote in 2013, "we know much less about the work of the vast bulk of the physics discipline than we do about major theoretical developments and high profile elites in particle physics."[50]

Why is this the case? Three related reasons come to mind. First, narratives of revolutionary science generally make considerably more gripping stories than the industrially linked and often non-converging fields of solid state and condensed matter physics.[51] Michael Eckert, a historian of solid state science, notes that the popular, if controversial, Kuhnian model of scientific revolution and scientific growth does not work for solid state physics: "there were no switches of incommensurable paradigms," Eckert suggests: "or [there were] too many of them on a smaller scale."[52] Noting that experimental achievement has been more central than theoretical work to the Nobel awards since the 1980s, Gingras and Wallace suggest that the strong recent recognition of experimental work reflects the absence of major paradigm shifts in physics since the late 1960s.[53] Helge Kragh writes that what most people would single out as the most profound and wide-ranging conceptual innovations of the twentieth century are relativity and quantum mechanics, and that solid state and condensed matter physics are examples of the kind of progress that results from extending knowledge already opened up for research by more precise measurement or by developing new instruments.[54]

Second, the large segment of physics that falls outside the fields of elementary particle physics, gravitational physics, and astrophysics appears to register lower down the rungs of a persistent hierarchical ladder defining intellectual status. Egon Orowan, one of Michael Polanyi's colleagues, lamented in the 1960s, for example, that his and Polanyi's earlier researches on the strength of materials—the kind of research that Wolfgang Pauli famously called "dirt physics"—had been a "prosaic and even humiliating proposition in the age of De Broglie, Heisenberg and Schrödinger."[55] Similarly, historian of physics Joseph D. Martin finds that the broad range of publicity and newspaper coverage for Nobel Prize awards associates high energy physics with fundamental intellectual insights into the universe, its origins and meaning, in contrast to descriptions of solid state and condensed matter research as useful and applied science, connected to chemistry, metallurgy and engineering. Martin calls this phenomenon "prestige asymmetry."[56]

Third, in relation to prestige asymmetry, the greatest prestige—no matter how much we know about a research achievement's organization, instrumentation, economic funding, political background, and other factors—still goes to the one or few individuals who are in charge, who have responsibility and who seem rightly to deserve credit.[57] The history of physics still seems to need heroes and heroines, and many of the easiest of these to identify are the founders of quantum theory and relativity theory. Albert Einstein still by far dominates histories of modern physics and will continue to do so as more and more of his papers and correspondence appear in *The Collected Papers of Albert Einstein*, now roughly halfway through its projected thirty volumes published in print and online.[58] Prominent, too, remain Isaac Newton, Galileo, and the late Stephen Hawking, whose burial service at Westminster Abbey in March 2018 was attended by more than one thousand people. Marie Curie, Robert Oppenheimer, Richard Feynman, and Werner Heisenberg are among other iconic figures in the history of physics and scientific biography.[59]

In the last decades, many historians have refocused their stories from single individuals to small groups, such as scientific families, scientific couples, scientists working together as a laboratory research group, or scientists in the same research field.[60] Laura Otis has written what she calls a "labography" that examines professional and personal relationships between a scientist father figure (the distinguished nineteenth-century German physiologist Johannes Müller) and seven of his students and protégés, with detailed descriptions of where they lived and worked, how they competed with each other, and how the former students struggled to win their own reputations and independence from their *Doktorvater*.[61] This might well be a model for some histories of twentieth-century science, although the small-scale labography remains largely biography, a genre that has great emotional appeal for the reader, while functioning as an accessible entrée into the complexity of a scientist's intellectual preoccupations and social networks. A larger-scale labography risks becoming a biographical dictionary that does not have a story line that is easy to read or to write.

Wolf Beiglböck, editor of *European Physical Journal H*, has called upon historians of physics to collaborate with physicists in order to better write about the technical physics and its networks.[62] Some historians already have adopted this approach, among them Lillian Hoddeson. On the other hand, Hoddeson, like many historians, has not abandoned the idea of *True Genius*, the title of her 2002 biography, with Vicki Daitch, of John Bardeen.[63] Nor does collaboration come easily to the majority of historians, who, like most humanists, have a very different tradition of writing and authorship from contemporary scientists. Indeed, most historians' conception of authorship (in contrast to editorship) still is largely rooted in a literary sensibility of private inspiration and creativity, in contrast to modern scientists' strategic necessity for social collaboration and consensus.[64] That said, many scientists still excel in single-handedly writing review essays about their own research or their field.[65]

Conclusion

Histories of physics have mirrored in many ways both stability and change in physics itself over the course of the last half-century, including physicists' own kinds of choices for attributions of the Nobel Prize. In general, historians of physics, in contrast to historians of technology, have favored conceptual, biographical, institutional, and sociological attention to high-profile fields of fundamental physics—particle physics, high-energy physics, astrophysics, and relativity—rather than solid state and condensed matter physics, but this is changing.

Historians Robert Crease and Catherine Westfall have called attention recently to what they call "The New Big Science" that has emerged in the last decades, as US national laboratories have constructed accelerator-based radiation and neutron-source machines for studying structural and behavioral properties of matter, including biological molecules, at the electronic, atomic, and molecular levels, while shifting an increasing proportion of high-energy and nuclear physics research to facilities abroad.[66] Historians of science and historians of technology increasingly have been paying attention to condensed matter, materials, and nanosciences and technologies, often in national, university, or industrial laboratories, and often under the guise of "technoscience," reflecting trends in recent physics.[67]

The next few years of Nobel Prizes likely will continue to bring attention to only a few physicists, perpetuating the culture of scientific heroes. However, historians of physics and popular science writers, while often focusing on heroes and heroines, also will be writing histories of institutions, laboratories, and research groups, including histories of large collaborations in research settings very different from classical or early-twentieth-century physics. Historians of physics also often are placing women scientists, who have been largely shunned by the Nobel committee, back into the picture of physics as a whole, one of the important trends in histories of physics in the last decades, as noted earlier. For its part, the Swedish Academy of Sciences in 2018 explicitly adopted a policy that calls on nominators to consider diversity in gender, geography, and topic for the 2019 Physics Prize.[68]

Finally, it seems important to consider some observations about reductionism, paradigms, and scientific revolutions in histories of physics. Philip Anderson has argued since the 1970s that complexity, involving the conception of "emergence," is "the fundamental philosophical insight" of late-twentieth-century science. Physicists investigating many-body systems have been discovering behaviors that cannot be predicted or understood on the basis only of the fundamental laws of elementary particles and wave mechanics. Anderson writes that properties emerge from the solutions of equations that are not readily apparent from the equations themselves, so that condensed matter physics is not just applied elementary particle physics.[69] In a review in the summer of 2018 in *Physics Today*, physicist Michael Stone called emergent behavior "the central paradigm of modern condensed-matter physics," a view reflected, for example, in recent Gordon Research

Conferences that focus on the understanding of emergent phases and long-range entanglement in correlated electron systems, a matter of concern in both condensed matter and atomic, molecular, and optical (AMO) physics.[70]

Among historians, Silvan S. Schweber, himself a theoretical particle physicist, was one of the first historians of physics to remark on the concern of many particle physicists that the reductionist approach "that has been the hallmark of theoretical physics in the 20th century is being superseded by the investigation of emergent phenomena" and that "physics, it could be said is becoming like chemistry."[71] Schweber, as a theoretical particle physicist, worried about the future of his field and its quest for reductionism and unified understanding of the physical world since, in his view, high energy physics and condensed matter physics were becoming decoupled from one another despite ongoing interchange of ideas.[72]

As historians more and more engage with developments in late-twentieth-century physics, it may be useful to consider the question of whether one of Kuhn's paradigm shifts has occurred or, rather, whether two different but not incommensurable ways of seeing the world have come to characterize physics. If so, then physics has arrived at an epistemology not so different than the epistemology of chemical and biological science in which the notion of emergent properties has long coexisted with the search for basic elementary bodies and fundamental laws.[73] For historians, there is a continuing challenge to describe and interpret the processes of specialization, collaboration, and problem choice that have characterized modern physics and led physicists increasingly to cross disciplinary boundaries into both the chemical and biological sciences where it is sometimes necessary to relinquish the reductionist paradigm.

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[2] David Kaiser, "Booms, Busts, and the World of Ideas: Enrollment Pressures and the Challenge of Specialization," *Osiris* **27** (2012), 276–302, on 290, 391, figure 6.

[3] Kaiser, "Booms, Busts" (ref. 2), 299, figure 7.

[4] Kaiser, "Booms, Busts" (ref. 2) 294–95. For the current classification system used by the APS and in INSPEC, Section A, Physics (the successor to *Physics Abstracts*), see https://publishing.aip.org/publishing/pacs/pacs-2010-regular-edition and https://www.theiet.org/resources/inspec/about/coverage/.

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[12] Alvin M. Weinberg, "Impact of Large-Scale Science," *Science* **134**, no. 3473 (1961), 161–64. Multi-authored collaborative research accounted for about 20% of physics and chemistry abstracts in 1910 and 1920, and co-authored publications rose to 60% by 1960, approaching 98% in the *Journal of the American Chemical Society* in 2000. The mean number of authors for articles published during the 1990s in *Physical Review Letters* was 5.5, with an increasing number of papers counting dozens or more authors. See Donald deB. Beaver and R. Rosen, "Studies in Scientific Collaboration," pt. 3, "Professionalization and the Natural History of Modern Scientific Co-Authorship," *Scientometrics* **1**, no. 3 (1979), 231–45, 241, figure 5; K. Brad Wray, "Scientific Authorship in the Age of Collaborative Research," *Studies in History and Philosophy of Science Part A* **37** (2006), 505–14, on 507.

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[16] Lindley Darden and Carl Craver, "Strategies in the Interfield Discovery of the Mechanism of Protein Synthesis," *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* **33**, no. 1 (2002), 1–28.

[17] See Mary Jo Nye, "Mine, Thine, and Ours: Collaboration and Co-Authorship in the Material Culture of the Mid-Twentieth Century Chemical Laboratory," *Ambix: The Journal of the Society for the History of Alchemy and Chemistry* **61**, no. 3 (2014), 211–35; "The Republic vs. The Collective: Two Histories of Collaboration and Competition in Modern Science," *NTM (Zeitschrift für Geschichte der Wissenschaften, Technik und Medizin)* **24**, no. 2 (2016), 169–94.

[18] Edward J. Hackett, "Essential Tensions: Identity, Control, and Risk in Research," *Social Studies of Science* **35**, no. 5 (2005), 787–826; Carsten Reinhardt, *Shifting and Rearranging: Physical Methods and the Transformation of Modern Chemistry* (Sagamore Beach, MA: Science History Publications, 2006).

[19] Elisabeth Crawford, "Introduction," in *Science, Society and Technology in the Time of Alfred Nobel,* ed. Carl Gustaf Bernard et al. (Oxford: Pergamon Press, 1982), 307–20, on 312. The Nobel physics committee members for the 2018 award included a (female) particle physicist as chair, a molecular physicist, a theoretical physicist, a subatomic physicist, a nanostructure physicist, and a computational biologist/biological physicist. See: "The Nobel Committee for Physics," NobelPrize.org, accessed January 17, 2019, https://www.nobelprize.org/prizes/uncategorized/the-nobel-committee-for-physics/.

[20] Crawford, "Introduction" (ref. 19), 308. Between 1901 and 1929, the annual average of nominators was thirty three and nominators made an average of thirty-eight nominations per year, proposing nineteen candidates on average each year, with nominators often favoring their own nationality. See Günter Küppers, Norbert Ulitzka and Peter Weingart, "The Awarding of the Nobel Prize: Decisions about Significance in Science," in *Science, Society and Technology in the Time of Alfred Nobel* (Oxford: Pergamon, 1982), 332–51, on 335.

[21] James R. Bartholomew, "One Hundred Years of the Nobel Science Prizes," *Isis* **96**, no. 4 (2005), 625–32, on 627. The nomination base expanded after its initial decades from two- or three-hundred nomination request letters annually before World War II to over 2,000 request letters in more recent times.

[22] Helge Kragh, *Quantum Generations: A History of Physics in the Twentieth Century* (Princeton: Princeton University Press, 1999), 429–30.

[23] Kragh, *Quantum Generations* (ref. 22), 431, 432, table 28.2.

[24] For example, the committees negotiated the 1904 Chemistry award to William Ramsay for inert gases and the 1904 Physics award to J. William Strutt, Lord Rayleigh, for the discovery of argon while analyzing gas densities. Crawford, "Introduction" (ref. 19), 316. Robert M. Friedman, *The Politics of Excellence: Behind the Nobel Prize in Science* (New York: W. H. Freeman, 2001), 182.

[25] Debye had earlier been nominated in physics, but he was blocked by one committee member. Friedman, *Politics of Excellence* (ref. 24), 207–9.

[26] Mary Jo Nye, "Physical and Biological Modes of Thought in the Chemistry of Linus Pauling," *Studies in History and Philosophy of Science Part B: Studies in the History and Philosophy of Modern Physics* **31**, no. 4 (2000), 475–92.

[27] Bretislav Friedrich, "How Did the Tree of Knowledge Get Its Blossom?: The Rise of Physical and Theoretical Chemistry, with an Eye on Berlin and Leipzig," *Angewandte Chemie International Edition* **55**, no. 18 (2016), 5378–92, on 5385.

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[29] Kohn, "An Essay" (ref. 7), S77. On the new fields, Joseph D. Martin, "What's in a Name Change?: Solid State Physics, Condensed Matter Physics, and Materials Science," *Physics in Perspective* **17**, no. 1 (2015), 3–32.

[30] Yves Gingras and Matthew L. Wallace, "Why It Has Become More Difficult to Predict Nobel Prize Winners: A Bibliometric Analysis of Nominees and Winners of the Chemistry and Physics Prizes (1901–2007)," *Scientiometrics* **82**, no. 2 (2010), 401–12; Hamish Johnston, "What Type of Physics Should You Do If You Want to Bag a Nobel Prize?," *Physics World*, October 2, 2014, http://blog.physicsworld.com/2014/10/02/what-type-of-physics-should-you-do-if-you-want-to-bag-a-nobel-prize/.

[31] "The Nobel Prize in Physics Fields," NobelPrize.org, accessed January 3, 2019, https://www.nobelprize.org/nobel_prizes/physics/fields.html.

[32] Marvin L. Cohen, "Fifty Years of Condensed Matter Physics," *Physical Review Letters* **101**, no. 25 (2009), 250001.

[33] De Gennes's prize is considered condensed matter physics. See Michael Rubinstein, "Polymer Physics—The Ugly Duckling Story: Will Polymer Physics Ever Become a Part of 'Proper' Physics?," *Journal of Polymer Science Part B: Polymer Physics* **48**, no. 24 (2010), 2548–51. Charpak pioneered the multiwire proportional counter, which is not based in photography.

[34] Note, however, that during 1966 to 1969, there were single-person awards to Alfred Kastler, Hans Bethe, Luis Alvarez, and Murray Gell-Mann.

[35] It was announced in September 2018 that Jocelyn Bell Burnell was the recipient of a $3 million privately funded Special Breakthrough Prize in Fundamental Physics. The $3 million Breakthrough Prizes have been awarded since 2012 in the separate areas of Fundamental Physics, Mathematics, and Life Sciences by a founding group that includes Sergey Brin, Mark Zuckerberg, Priscilla Chan, Yuri Milner, and Jack Ma. See https://breakthroughprize.org/. On Jocelyn Bell Burnell, see https://breakthroughprize.org/News/45.

[36] Kragh, *Quantum Generations* (ref. 22), 433. The death in December 2016 of astrophysicist Vera Rubin reminded some scientists of the possibly gendered question of why the discovery of dark matter had not received Nobel recognition, in which Rubin likely would have been included. Lisa Randall, "Why Vera Rubin Deserved a Nobel," *New York Times*, January 4, 2017, https://www.nytimes.com/2017/01/04/opinion/why-vera-rubin-deserved-a-nobel.html; Dennis Overbye, "Vera Rubin, 88, Dies; Opened Doors in Astronomy, and for Women," *New York Times*, December 27, 2016, https://www.nytimes.com/2016/12/27/science/vera-rubin-astronomist-who-made-the-case-for-dark-matter-dies-at-88.html. Chemistry Nobel awards went to Marie Curie (1911) and later to her daughter Irène Joliot-Curie (1935) for researches on radioactivity, to Dorothy Crowfoot Hodgkin (1964) and Ada Yonath (2009) for X-ray crystallographic studies of molecular structure, and to Frances Arnold (2018) for her evolutionary-mutation method of producing new enzymes. Gerty T. Cori was the first woman to receive the Nobel Prize in Physiology or Medicine, sharing one-half of the 1947 award with her husband Carl F. Cori and the other half with Bernardo Alberto Houssay.

[37] Dennis Overbye notes that at least six theorists had the idea of the Higgs boson including the still-living Tom Kibble, Carl Hagen, and Gerald Guralnik. In this same article in 2016, Overbye suggested that the LIGO collaboration should receive a group prize. Dennis Overbye, "Bob Dylan Won. But in Science, the Times They Aren't A-Changing," *New York Times,* October 31, 2016, https://www.nytimes.com/2016/11/01/science/bob-dylan-won-but-in-science-the-times-they-arent-a-changin.html.

[38] See ref. 35 and the May 2016 announcement at https://breakthroughprize.org/News/32.

[39] David L. Hull, *Science as Process: An Evolutionary Account of the Social and Conceptual Development of Science* (Chicago: University of Chicago Press, 1988), 312.

[40] Richard Staley, “Trajectories in the History and Historiography of Physics in the 20th Century,” *History of Science* **52**, no. 2 (2013), 151–77, on 151.

[41] Among the many sources on Kuhn, see Robert J. Richards and Lorraine Daston, eds., *Kuhn’s Structure of Scientific Revolutions at Fifty* (Chicago: University of Chicago Press, 2016); Jürgen Renn et al., eds. *Towards a History of the History of Science: 50 Years since ‘Structure’* (Berlin: Edition Open Access, 2016), 287–93, http://www.edition-open-access.de/proceedings/8/index.html.

[42] Thomas Kuhn, *The Structure of Scientific Revolutions,* 50th anniversary ed. (Chicago: University of Chicago Press, 2012). As Staley has pointed out, Kuhn’s 1978 monograph *Black-Body Theory and the Quantum Discontinuity* (Chicago: University of Chicago Press, 1978) presented a more finally nuanced “intellectual sociology of the physics community.” Staley, “Trajectories” (ref. 40), 161.

[43] Peter Novick, *That Noble Dream: The ‘Objectivity Question’ and the American Historical Profession* (Cambridge: Cambridge University Press, 1988).

[44] Weinberg, “Impact of Large-Scale Science” (ref. 12).

[45] Mary Jo Nye, *Blackett: Physics, War, and Politics in the 20th Century* (Cambridge, MA: Harvard University Press, 2004); “Re-Reading Bernal: History of Science at the Crossroads in 20th-Century Britain,” in *Aurora Torealis: Studies in the History of Science and Ideas in Honor of Tore Frängsmyr*, ed. Marco Beretta, Karl Grandin and Svante Lindqvist, 237–60 (Sagamore Beach, MA: Science History Publications, 2008); *Michael Polanyi and His Generation: Origins of the Social Construction of Science* (Chicago: University of Chicago Press, 2011).

[46] John Ziman, *Reliable Knowledge: An Exploration of the Grounds for Belief in Science* (Cambridge: Cambridge University Press, 1978). On physicists as antirealists, see Carl G. Adler, “Realism and/or Physics,” *American Journal of Physics* **57**, no. 10 (1989), 878–82.

[47] The journal *Historical Studies in the Physical Sciences* (*HSPS*) became *Historical Studies in the Physical and Biological Sciences* (still abbreviated *HSPS*) in 1986 and then *Historical Studies in the Natural Sciences* (*HSNS*) in 2008.

[48] Recent book-length studies along these lines include: Robert P. Crease, *Making Physics: A Biography of Brookhaven National Laboratory, 1946–1972* (Chicago: University of Chicago Press, 1999); Patrick McCray, *Astronomical Ambition and the Promise of Technology* (Cambridge, MA: Harvard University Press, 2004); Peter J. Westwick, *The National Labs: Science in an American System, 1947–1974* (Cambridge, MA: Harvard University Press, 2003); and Erik M. Conway, *Atmospheric Science at NASA: A History* (Baltimore: Johns Hopkins University Press, 2008).

[49] Lillian Hoddeson, “The Discovery of the Point-Contact Transistor,” *Historical Studies in the Physical Sciences*, **12**, no. 1 (1981), 41–76; Krzysztof Szymborski, “The Physics of Imperfect History—A Social History,” *Historical Studies in the Physical Sciences* **14**, no. 2 (1984), 317–56: Per Dahl, “Kamerlingh Onnes and the Discovery of Superconductivity: The Leyden Years, 1911–1914,” *Historical Studies in the Physical Sciences* **15**, no. 1 (1985), 1–38; Michael Eckert, “Propaganda in Science: Sommerfeld and the Spread of the Electron Theory of Metals,” *Historical Studies in the Physical and Biological Sciences* **17**, no. 2 (1987), 191–234.

[50] Staley, “Trajectories” (ref. 40), 164.

[51] See Michael Eckert, “Plasmas and Solid-State Science,” in *The Cambridge History of Science*, vol. 5, *The Modern Physical and Mathematical Sciences*, ed. Mary Jo Nye, 413–28 (Cambridge: Cambridge University Press, 2003). Also, Bernadette Bensaude-Vincent, “The Construction of a Discipline: Materials Science in the United States,” *Historical Studies in the Physical and Biological Sciences* **31**, no. 2 (2001), 223–48.

[52] Michael Eckert, “Plasmas and Solid State Science” (ref. 51), 427–28. On the history and epistemology of nanoscience, see Anne Marcovich and Terry Shinn, *Toward a New Dimension: Exploring the Nanoscale* (Oxford: Oxford University Press, 2014).

[53] Yves Gingras and Matthew L. Wallace, "Why It Has Become More Difficult to Predict Nobel Prize Winners: A Bibliometric Analysis of Nominees and Winners of the Chemistry and Physics Prizes (1901°2007)," arXiv, August 19, 2008, https://arxiv.org/abs/0808.2517, 10. Others suggest that the increase in the initial ages at which Nobelists' prize-winning research initially was accomplished results from a shift toward experimental work that requires long experience and skill in drawing upon already established work. See Benjamin F. Jones and Bruce A. Weinberg, "Age Dynamics in Scientific Creativity," *Proceedings of the National Academy of Sciences* **108**, no. 47 (2011), 18910–14; also, Zoë Corbyn, "Experience Counts for Nobel Laureates," *Nature News*, November 7, 2011, http://www.nature.com/news/2011/111107/full/news.2011.632.html.

[54] Kragh, *Quantum Generations* (ref. 22), 443.

[55] On Pauli's remark, see Michael Eckert and Helmut Schubert, *Crystals, Electrons, Transistors: From Scholar's Study to Industrial Research*, trans. Thomas Hughes (New York: American Institute of Physics, 1990), 184–85, and Egon Orowan, "Dislocations in Plasticity," in *The Sorby Centennial Symposium on the History of Metallurgy*, ed. C. S. Smith (New York: Gordon and Breach, 1965), 359–76.

[56] Joseph D. Martin, "Prestige Asymmetry in American Physics: Aspirations, Applications, and the Purloined Letter Effect," *Science in Context* **30**, no. 4 (2017), 475–506, on 488. Martin's book *Solid State Insurrection: How the Science of Substance Made American Physics Matter* (Pittsburgh: University of Pittsburgh Press, 2018) is a new history of the field.

[57] Galison, "The Collective Author" (ref. 13), 329.

[58] The Einstein Papers Project: *The Collected Papers of Albert Einstein,* published in print by Princeton University Press. Volume 15 appeared in April 2018, and the total number of volumes is expected to be around thirty. See http://www.einstein.caltech.edu/what/index.html.

[59] My July 2018 analysis of Amazon.com lists for "biographies and memoirs" resulted in 430 entries for Einstein, 315 for Newton, 297 for Galileo, 258 for Stephen Hawking, 126 for Marie Curie (many aimed at young readers), 59 for Robert Oppenheimer, 51 for Richard Feynman, 30 for Niels Bohr, 21 for Werner Heisenberg, and 6 for Emilio Segrè. Given the changing nature of the Amazon.com website, this count surely cannot be replicated. Recent notable biographies or memoirs of physicists include: Gino Segrè and Bettina Hoerlin, *The Pope of Physics: Enrico Fermi* (New York: Henry Holt, 2016); David Schwartz, *The Last Man Who Knew Everything: Life and Times of Enrico Fermi* (New York: Basic, 2017); and Paul Halpern, *The Quantum Labyrinth: How Richard Feynman and John Wheeler Revolutionized Time and Reality* (New York: Basic, 2017). Autobiographical accounts include Freeman Dyson, *Makers of Patterns: An Autobiography Through Letters* (New York: Liveright, 2018), and Luis W. Alvarez's *Alvarez: Adventures of a Physicist* (1987; Lexington: Plunkett Lake Press, 2017). All of these biographical or autobiographical subjects are associated with particle physics or relativity.

[60] For example, Silvan S. Schweber, *QED and the Men Who Made It: Freeman Dyson, Richard Feynman, Julian Schwinger and Sin-Itiro Tomonaga* (Princeton: Princeton University Press, 1994), and Istvan Hargittai, *Martians of Science: Five Physicists Who Changed the Twentieth Century* (Oxford: Oxford University Press, 2008), on Theodore von Karman, Leo Szilard, Eugene P. Wigner, John von Neumann, and Edward Teller. Scientific families that have been the subjects of biographies include the Cassinis, the Bernoullis, the Becquerels, the Curies, the Darwins, the Huxleys, the Herschels, and the Braggs. Pnina G. Abir-Am, Dorinda Outram, and Helena Pycior pioneered collections of essays on scientific couples.

[61] Laura Otis, *Müller's Lab* (Oxford: Oxford University Press, 2007).

[62] Wolf Beiglböck, "Editorial," *European Physical Journal H* **35**, no. 1 (2010), 1–2. For a critique of a sociological history of physics, charging that the author did not have a clear understanding of his subject of the LIGO Scientific Collaboration detection of gravitational waves, see Bruce Allen,

"Review of Harry Collins, *Gravity's Kiss: The Detection of Gravitational Waves* (MIT Press, 2017)," *Physics Today* **70**, no. 12 (2017), 53. Allen is a physicist associated with the LIGO collaboration.

[63] Michael Riordan and Lillian Hoddeson, *Crystal Fire: The Invention of the Transistor and the Birth of the Information Age* (New York: Norton, 1998); Lillian Hoddeson, Adrienne W. Kolb, and Catherine Westfall, *Fermilab: Physics, the Frontier, and Megascience* (Chicago: University of Chicago Press, 2008); Michael Riordan and Lillian Hoddeson, *Tunnel Visions: The Rise and Fall of the Superconducting Super Collider* (Chicago: University of Chicago Press, 2015); Vicki Daitch and Lillian Hoddeson, *True Genius: The Life and Science of John Bardeen: The Only Winner of Two Nobel Prizes in Physics* (Washington, DC: Joseph Henry Press, 2002).

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[65] Harriet Zuckerman, "Patterns of Name Ordering Among Authors of Scientific Papers: A Study of Social Symbolism and Its Ambiguity," *American Journal of Sociology* **74**, no. 3 (1968), 276–91, on 289.

[66] Crease and Westfall write that materials science and nanoscience researchers rely on a more decentralized organizational structure of using large machines in concert with smaller ones in order to piece together mosaics of properties, in contrast to the more centralized research of high energy and nuclear physicists that adds pieces to a single, coherent puzzle. Robert P. Crease and Catherine Westfall, "The New Big Science," *Physics Today* **69**, no. 5 (2016), 30–36, esp. 30, 34. Also, W. Patrick McCray, "Will Small Be Beautiful? Making Policies for Our Nanotech Future," *History and Technology* **21**, no. 2 (2005), 177–203.

[67] On the erosion of the technology versus science distinction following the Second World War and the possibility of distinguishing recent science from technoscience, see Alan J. Rocke, "Theory versus Practice in German Chemistry: Erlenmeyer beyond the Flask," *Isis* **109**, no. 2 (2018), 254–75, on 269–71. Also, Bernadette Bensaude-Vincent, "Matters of Interest: The Objects of Research in Science and Technoscience," *Journal of the General Philosophy of Science* **42**, no. 2 (2011), 365–83.

[68] Elizabeth Gibney, "What the Nobels Are—and Aren't—Doing to Encourage Diversity," *Nature News*, September 28, 2018, https://www.nature.com/articles/d41586-018-06879-z.

[69] Philip W. Anderson, "More Is Different," *Science* **177**, no. 4047 (1972), 393–96; Anderson, *More Is Different: Notes* (ref. 6), 90.

[70] Michael Stone, "Modeling Quantum Behavior in Condensed Matter," *Physics Today* **71**, no. 6 (2018), 57—a review of Ramamurti Shankar, *Quantum Field Theory and Condensed Matter: An Introduction* (Cambridge: Cambridge University Press, 2017). On the research conferences, see https://www.grc.org/correlated-electron-systems-grs-conference/2018/. International Gordon Research Conferences have been meeting since 1931 in order to address what is considered frontier research.

[71] Silvan S. Schweber, "Physics, Community and the Crisis in Physical Theory," *Physics Today* **46**, no. 11 (1993), 34–40, on 34. See the discussion in Elena Castellani, "Reductionism, Emergence, and Effective Field Theories," *Studies in History and Philosophy of Science Part B: Studies in the History and Philosophy of Modern Physics* **33**, no. 2 (2002), 251–67.

[72] Schweber, "Physics, Community and the Crisis" (ref. 71), 38–39.

[73] For example, see Michael Polanyi, "The Value of the Inexact," *Philosophy of Science* **3**, no. 2 (1936), 233–34; *Personal Knowledge: Toward a Post-Critical Philosophy* (Chicago: University of Chicago Press, 1958), 394.

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