Training and the Generalist's Vision in the History of Science

*By David Kaiser**

ABSTRACT

Commentators have often complained about specialization in the history of science. This essay discusses recent intellectual trends within our discipline in the light of significant changes in graduate training: both a relatively recent consensus as to the types of sources that are appropriate to analyze in a dissertation and the tremendous growth in the number of new dissertations completed each year in our field. It suggests that this kind of focus on pedagogical concerns provides useful analytic tools for historians investigating other fields in various times and places.

OVER THE PAST TWENTY YEARS various commentators have lamented the onset of specialization within the history of science. Fewer and fewer synthetic or generalist works get written; balkanization seems more and more the rule of the day.[1] Several explanations have been advanced. One important centrifugal force has been dissatisfaction with an older positivist philosophy of science, which once promised to define both what "science" is and how "the scientific method" works. All historians of science needed to do, it once seemed, was elaborate on the underlying picture with a few examples; the basic story line had already been set. Few historians or philosophers of science today subscribe to anything like that tidy program, and no single philosophical alternative has since earned pride of place. We no longer have any ready cue to tell us what "science" is, and hence we have lost one former source of coherence for our field.

Alongside the failure of positivism, according to most of these accounts, has been the rise within our field of social- and cultural-historical methods that have displaced a previous preoccupation with intellectual history. This shift has stemmed both from a widespread "anthropological turn" within history generally, with its attendant challenge to

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[1] Charles Rosenberg, "*Isis* at Seventy-Five," *Isis,* 1987, *78:*514–517; C. Hakfoort, "The Missing Syntheses in the Historiography of Science," *History of Science,* 1991, *29:*207–216; and James Secord, ed., "The Big Picture," special issue, *British Journal for the History of Science,* 1993, *26:*387–483.

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"grand narratives," and from the rise of constructivist programs in the history and sociology of science—all driving a wave of microhistorical case studies rather than synthetic or generalist overviews. As Russell Jacoby put it so dourly in the mid 1980s, such intellectual shifts (he would have called them "fads") throughout the humanities and social sciences led to the ultimate "inflation-proof" academic product: one could always spin out another postmodernist analysis of some other author, artist, or episode, *ad infinitum.*[2] Without the previous metric to define "historical significance," nearly any event or group could become the center of someone's analysis. And with no need to build grand narratives, there was no longer much need to coordinate individual intellectual contributions. Ironically, in this analysis, those humanists and social scientists who had most squarely rejected positivism reproduced the positivists' reigning metaphor for scholarly production: we're all just bricklayers, no single brick being much distinguished from the rest (we just disagree as to whether this collection of bricks is meant to add up to anything in particular).

Surely these shifts in intellectual fashions and methodological presuppositions have played a significant role in the decline of the "generalist's vision" within the history of science. But I suspect that there are other important forces at work as well—forces that are ill captured by focusing so narrowly on competing intellectual programs. My approach to the question of specialization in the history of science centers on the exigencies of training graduate students. What counts as "appropriate" or "acceptable" scholarly work—not as measured by a litmus test of positivism versus constructivism, but in the more quotidian mechanics of selecting research topics, gathering sources, filing dissertation proposals, outlining chapters, and all the rest—and how have these conventions responded to institutional and demographic changes during the past few decades? The question, in other words, is how educational infrastructure helps to shape the world of ideas. I have been fascinated by these kinds of questions in trying to understand changes in American physics during the Cold War, and I believe a similar approach may offer insights into recent trends in the history of science.

During the past few decades, as the history of science has become more and more entrenched as a professional, autonomous academic discipline, basic expectations and standards for graduate students' work have shifted. Today Ph.D. dissertations in our field are expected to be based largely on previously unpublished primary sources. "That's what professional historians of science do"; what could seem more natural or appropriate? The range of sources continues to widen—from scientists' correspondence and research notebooks to grant proposals, architectural plans, simulation codes, oral histories, material objects, and so on—but the basic expectation that new work will draw on *some* kind of unpublished materials now cuts clear across any "internalism"/"externalism" divide. Yet of course it has not always been this way. Alexandre Koyré, for example, produced justly renowned studies of Galilean and Newtonian mechanics based almost entirely on Galileo's and Newton's published works; it is difficult to imagine a dissertation committee at most of our departments approving such a plan were it proposed by a graduate student today. When today's graduate students are pushed to broaden their training (and read books like Koyré's), it is almost always in the context of preparing for "general exams"—the design

[2] Russell Jacoby, *The Last Intellectuals: American Culture in the Age of Academe,* rev. ed. (1987; New York: Basic, 2000), Ch. 6. More generally see, e.g., Peter Novick, *That Noble Dream: The* "*Objectivity Question*" *and the American Historical Profession* (New York: Cambridge Univ. Press, 1988), esp. Chs. 13–16; and Jan Golinski, *Making Natural Knowledge: Constructivism in the History of Science* (Cambridge: Cambridge Univ. Press, 1998).

of which seems more geared to providing students with sufficient background to teach a recognized field in a standard undergraduate curriculum than to honing a generalist narrative in their dissertations.[3]

There is certainly no need to rehash the myriad reasons why historians no longer rely solely on scientists' publications, but it is worth noting some consequences of this shift. Archives (and similar research sites, such as museums) have rightly become central to our field—and with this shift has come a strong tendency toward *localism.* Sitting comfortably in a well-stocked library, Koyré and his colleagues could compare the intellectual efforts of scholars from across the "learned world," analyzing decades or even centuries of developments. Today, by contrast, limitations of funds and time make it difficult for graduate students to pursue in-depth studies of more than a handful of geographically dispersed archival collections or to plumb such collections over time periods spanning more than a few decades. There is also an increase in what might be called "archival positivism": an assumption that if documents have been well preserved then the topic must warrant study. In our field, as in many social sciences, the availability of data can trump questions of their inherent interest or greater meaning. Of course archives are important; and of course the requirement that graduate students cut their teeth on archivally based dissertations contributes to specialized projects of limited temporal, geographical, and intellectual scope.

Even more important is the rapid growth in numbers of new practitioners. For at least forty years historians and sociologists have noted the correlations between an academic field's specialization and the size of its pool of practitioners. As Derek Price emphasized so beautifully in the 1960s, more scientists (and scholars generally) are alive and working today than the sum total that had ever previously toiled on this earth; such is the nature of exponential growth. As early as the 1830s, feeling overwhelmed by the ever-rising numbers of learned journals, scientists began turning to abstracting journals just to try to keep pace. Predictably, the number of new abstracting journals rose at the same exponential clip as the number of new research journals, yielding a fractal pattern of escalating literature.[4] With so many practitioners producing so many books and articles, no one could claim to follow all of the literature on her or his own, and thus specialties were born. Under the relentless pressure of continued growth, even these specialties subdivided over and over again—a kind of mitosis of the scholarly mind.

We in the history of science may have come to this state of rapid growth more recently than other disciplines, but there can be little doubt that we are in it now. As many as eight hundred participants now register for each year's annual meeting of the History of Science Society, and every year the society struggles to accommodate the fast-growing numbers of paper proposals and session submissions. No wonder the meetings have become so

[3] Alexandre Koyré, *Galileo Studies,* trans. John Mepham (1939; Atlantic Highlands, N.J.: Humanities, 1978); Koyré, *Newtonian Studies* (Cambridge, Mass.: Harvard Univ. Press, 1965); and Koyré, *From the Closed World to the Open Universe* (Baltimore: Johns Hopkins Univ. Press, 1957). See also Mario Biagioli, "The Scientific Revolution Is Undead," *Configurations,* 1988, *6:*141–147. The current expectation for early modernists seems to be that their dissertations will be based on primary sources that were unpublished in their day, even if they have since been published.

[4] Derek J. de Solla Price, *Science since Babylon* (New Haven, Conn.: Yale Univ. Press, 1961), Ch. 5 (on the exponential growth in numbers of abstracting journals see pp. 96–99); Price, *Little Science, Big Science* (New York: Columbia Univ. Press, 1963); Joseph Ben-David, *The Scientist's Role in Society: A Comparative Study,* 2nd ed. (1971; Chicago: Univ. Chicago Press, 1984); Diana Crane, *Invisible Colleges: Diffusion of Knowledge in Scientific Communities* (Chicago: Univ. Chicago Press, 1972); Nicholas Mullins, *Theories and Theory Groups in Contemporary American Sociology* (New York: Harper & Row, 1973); and Tony Becher, *Academic Tribes and Territories: Intellectual Enquiry and the Cultures of Disciplines* (Bristol: Open Univ. Press, 1989).

bloated: during the past three decades, the number of Ph.D. dissertations on the history of science completed in the United States and Canada has grown dramatically (see Figure 1).[5]

Three distinct phases mark these changes. First came an early stage of growth, between 1974 and 1988, during which the rate at which new Ph.D.'s entered our field grew by about 2 per year: 21 people earned Ph.D.'s in the history of science in 1974, while nearly 50 did so in 1988. Next came a period of much steeper growth: between 1988 and 1996, the number of new Ph.D. dissertations in our field rose by 14.5 per year, to a peak of 189 in 1996 alone. Since that peak, we have been in a phase of modest decline, falling by more than 8 Ph.D.'s per year, so that the number of new dissertations in our field in 2003 matched the number in 1993. Even with the recent decline, however, an average of nearly

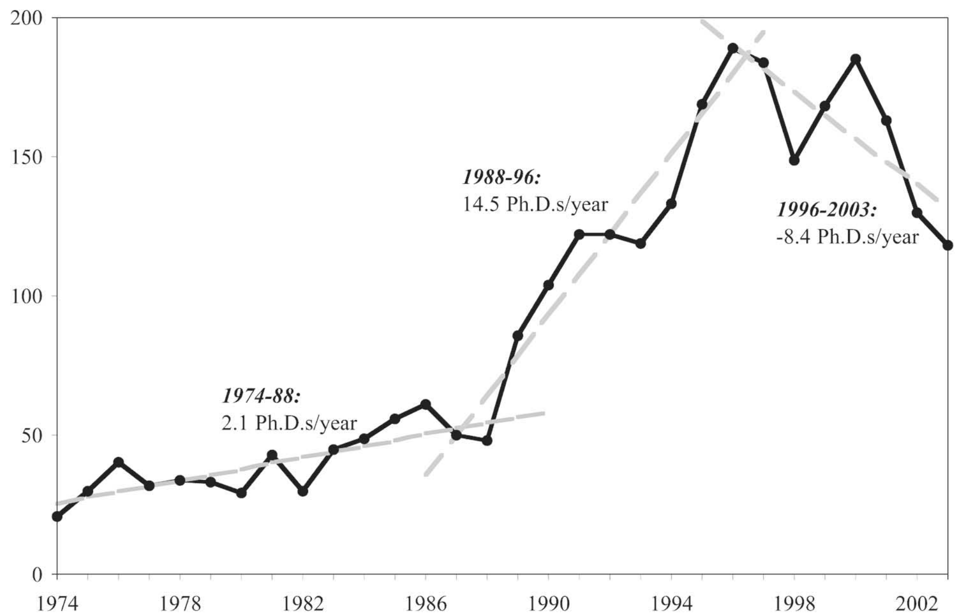

Figure 1. Number of Ph.D. dissertations on the history of science per year in the United States and Canada, 1974–2003. Dashed lines show average rates of change during each period.

[5] The data in Figure 1 come from entries in the Dissertation Abstracts database, maintained by ProQuest Information and Learning Company (formerly University Microfilms, Inc. [UMI]) and available at http://www.oclc.org. Included in Figure 1 are all Ph.D. dissertations accepted in the United States and Canada whose authors included "history of science" as one of the descriptors for the dissertation. Note that the database does not include separate subject descriptors for either "history of technology" or "history of medicine," and hence several of these dissertations are included in Figure 1, although these topics remain a minority of all dissertations included in the "history of science" category (see Table 3, below). In the Doctorate Records File maintained by the National Science Foundation, on the other hand, Ph.D. recipients were required to use only one subject descriptor instead of up to three to describe their dissertations. The number of Ph.D.'s per year counted in "history of science" in the United States within the NSF database remains basically the same as in the UMI set for the period 1974–1982 but ranges between 13 and 25 for the period 1983–1988, between 22 and 41 for 1988–1996, and between 36 and 50 for 1997–2002. The Doctorate Records File may be accessed at http://webcaspar.nsf.gov. I believe that the UMI dataset is more appropriate for present purposes, since it includes all Ph.D. recipients who self-identified as historians of science; these are people most likely to participate in the field's professional meetings, publish in its journals, apply for its jobs, and so on.

160 new dissertations relating to the history of science were produced *per year* during 1994–2003, compared with 82 per year during 1984–1993 and only 34 per year during 1974–1983. Each of the previous decades, in other words, has seen a jump by a factor of two or more in the average number of new Ph.D.'s entering the field in a given year. This has far outpaced the increase in the total number of Ph.D.'s awarded in all fields within the United States: between 1988 and 1996, the number of new dissertations in the history of science grew *eleven times* faster than the growth rate for all U.S. doctorates combined.[6] (Obviously the job market for our field has dramatically failed to keep pace, a subject that requires its own careful scrutiny.)

Consider some effects of this sudden surge in numbers on the question of specialization. Dissertations are expected to articulate and defend original theses based on primary sources; each graduate student and advisor must carve out *something* that the student can work on. Such rapid inflation in the number of new dissertations written and filed each year is bound to affect the range of "appropriate" or "acceptable" topics. What would it mean for a graduate student today (or during most of the past fifteen years) to say to herself, "I will write a dissertation covering the history of science during the early modern period," if *two dozen* dissertations on specific topics within that area had been written the previous year, with another two dozen the year before that, and so on? The problem is further exacerbated for topics from the nineteenth and twentieth centuries, which for the past thirty years have dominated each year's crop of new dissertations. Many other fields have faced this size-based dilemma in the past. Price called growth curves akin to those shown in Figure 1 "the disease of science" in the early 1960s. Physicists in the United States decried the centrifugal forces that threatened to rend their discipline apart in the face of an overwhelming and unprecedented population explosion after World War II—and the physicists themselves were several decades behind the curve set by medical doctors in this country.[7]

Intellectually, there are at least two responses that a discipline can make in the face of this kind of rapid growth: widen the scope of inquiry to include topics that had not been broached before; or divide up the known territory more and more finely into narrower and narrower subdivisions, so each practitioner knows more and more about less and less.[8] Both options carry potential consequences for the generalist's vision: while the former threatens to splinter a discipline into disjointed interest groups for whom it becomes increasingly difficult to find common ground, the latter threatens to create narrow-minded specialists unable to peer beyond the bounds of their delimited studies. In terms of the periods and places we study, historians of science have tended toward the latter option. Remarkably little shift has occurred in the broad time periods analyzed by all of those dissertations: the proportion of dissertations on ancient and medieval topics, on the early

[6] Data on total Ph.D.'s awarded in the United States come from the NSF's Doctorate Records File. Using only those Ph.D.'s marked "history of science" in the NSF database, rather than the UMI database, the rate of growth for our field still exceeds that for all Ph.D.'s by a factor of 5.6 between 1988 and 1999.

[7] Price, *Science since Babylon,* Ch. 5 (cit. n. 4). On the population explosion in U.S. physics and its effects see David Kaiser, "Cold War Requisitions, Scientific Manpower, and the Production of American Physicists after World War II," *Historical Studies in the Physical and Biological Sciences,* 2002, *33:*131–159; and Kaiser, "The Postwar Suburbanization of American Physics," *American Quarterly,* 2004, *56:*851–888. On specialization in American medical education see Paul Starr, *The Social Transformation of American Medicine* (New York: Basic, 1982), Ch. 3; and Kenneth M. Ludmerer, *Learning to Heal: The Development of American Medical Education* (New York: Basic, 1985).

[8] See John Ziman, *Knowing Everything about Nothing: Specialization and Change in Scientific Careers* (Cambridge: Cambridge Univ. Press, 1987).

modern period, and on the nineteenth and twentieth centuries was nearly the same in 2000 as it was in 1980, even as the total number of dissertations grew sixfold. (Within the modern category there has been a shift toward more recent topics; see Table 1.)[9]

One might have expected the "anthropological turn" to lead historians of science to consider a broader range of politico-cultural settings, to answer the persistent call to look seriously at non-Western examples. For the most part we have not done this, instead shifting the balance of our professional gaze from Western Europe to the United States, with nary a sidelong glance beyond these two regions (see Table 2).[10]

The distribution of topics within these times and places shows much greater variation (see Table 3). Twenty-five years ago, nearly one in two dissertations in the history of science concerned the modern physical sciences; by 2000 the proportion had fallen to one in ten. New areas of study, such as science in popular culture, have flourished. More important, recent dissertations have been distributed over topics much more evenly than those in previous years. Whereas we have continued to stick overwhelmingly to the modern period and to North America and Western Europe, we have extended our studies to a wider range of sciences.

Such an expansion of fields receiving intense study, together with the smoothing out of the numbers of people studying each area, carries no single valence. On the one hand, we

Table 1. Percentage of History of Science Dissertations Analyzing Various Historical Periods

Period	1980 (N = 29)	1990 (N = 104)	2000 (N = 185)
Ancient + Medieval	10.3%	6.7%	10.3%
Early Modern	17.2	11.5	15.7
Modern	72.4	77.9	71.9
19th century	44.8	26.9	25.4
20th century	27.6	51.0	46.5

NOTE.—Four dissertations each in 1990 and 2000, covering philosophical topics, did not have obvious temporal coverage and are not tabulated here.

Table 2. Percentage of History of Science Dissertations Analyzing Various Places

Place	1980	1990	2000
United Kingdom	27.6%	10.6%	7.0%
Western Europe	27.6	9.6	11.4
United States	13.8	54.8	45.4
Other	3.4	2.9	8.1

NOTE.—Only those dissertations covering the modern period are tabulated; percentages shown are based on the entire year's cohort.

[9] The data in Tables 1–3 are based on my analysis of the UMI set of "history of science" Ph.D. dissertations accepted by institutions in the United States and Canada. I have assigned dissertations to these various categories on the basis of their titles and abstracts.

[10] One third of the dissertations listed as "other" for the 2000 cohort in Table 2 focused on Canada; only 4.3 percent of that year's dissertations focused on Asia, the Middle East, or South America, and none focused on Africa. The Ph.D. cohort of 1990 was the most focused on topics set in the United States during the twentieth century: 46 percent of all of the 1990 history of science dissertations focused on this area, compared to 14 percent of the 1980 cohort and 35 percent of the 2000 cohort.

Table 3. Percentage of History of Science Dissertations Analyzing Various Topics

Topic	1980	1990	2000
Physical Sciences	44.8%	17.3%	10.3%
Biological Sciences	10.3	7.7	6.5
Social Sciences	6.9	10.6	5.4
Science Policy	6.9	1.9	7.0
Technology	3.4	9.6	8.1
Medicine	0	20.2	16.2
Popular Science	0	4.8	10.8
Psychology	0	4.8	5.9
Environment	0	1.0	1.6

NOTE.—Only those dissertations covering the modern period are tabulated; percentages shown are based on the entire year's cohort.

are no longer all studying the modern physical sciences (no doubt a healthy development); on the other, we have furthered a kind of balkanization. A room containing nine randomly selected young historians of science today is likely to contain specialists who work on as many distinct areas of research.

Does this widening of our (collective) research topics inexorably lead to our inability to talk with each other? I don't think so; demographics are not destiny. It does, however, indicate a need to reexamine some of our taken-for-granted assumptions about how best to train new members of our field. We would do well to instill a renewed emphasis on certain questions or themes that can speak across this patchwork of specialized topics and localized archives. Obviously these themes should not become doctrinal; nor is there much danger that that could happen: our field is already too large and varied for any single "program" to convert a majority. There are general questions, though, that might provide an interesting, if imperfect, *lingua franca.* These have to do with what I like to call "epistemic bite": How do our increasingly sophisticated analyses of context matter to the form and shape of scientific knowledge? What questions seemed coherent (let alone worth asking) to specific groups in various times and places? With what means did they find it appropriate to search for results, communicate findings, and adjudicate answers? What kinds of work went into spreading these findings among large and heterogeneous communities—and with what effects on the knowledge claims and on their claimants?

One approach I have used to gain traction with these questions is to promote pedagogy to a central analytic category. Since at least the middle of the nineteenth century, nearly all practicing scientists and engineers have gone through some kind of formal training; the past century and a half has seen the decline of the "gentlemanly amateur of science."[11] Naturally the forms of training have varied across time and place, as well as across the evolving disciplinary map. Yet the necessity of some form of training has emerged as one constant across these many settings. Scientific training thus presents a rich arena to explore along several axes—intellectual, political, cultural—across a broad range of sites.[12]

[11] Of course, pedagogical concerns have hardly been unique to the modern period. See, e.g., Owen Gingerich and Robert Westman, "The Wittich Connection: Conflict and Priority in Late Sixteenth-Century Cosmology," *Transactions of the American Philosophical Society,* 1988, *78:*1–144; Peter Dear, *Discipline and Experience: The Mathematical Way in the Scientific Revolution* (Chicago: Univ. Chicago Press, 1995); and Ken Alder, *Engineering the Revolution: Arms and Enlightenment in France, 1763–1815* (Princeton, N.J.: Princeton Univ. Press, 1997).

[12] For examples drawn from the modern physical sciences see esp. Kathryn Olesko, *Physics as a Calling:*

Historians and sociologists of education have long emphasized that pedagogy is anything but a passive or neutral activity. What counts as "appropriate" or "acceptable" pedagogy in a given setting is always conditioned by decisions as to what skills students should acquire (and why), as well as related concerns about labor supplies and the flow of human capital into and beyond instructional settings. Educational institutions serve as crucibles for reproducing cultural, political, and moral values as well as for replicating skills among new generations of practitioners. Schools guide students (with varying shades of subtlety and effectiveness) to become good citizens and to forge appropriate identities and roles in society. Of course, what counts as "good citizenship," just like what counts as "appropriate skills," always reflects active decisions (and often fraught controversy and bitter negotiations) made in specific contexts.[13] In short, historically contingent, culturally specific regimes of training tether the netherworld of scientific ideas and practices. As such, pedagogy offers historians of science a major theme with which to compare and combine otherwise-disparate case studies. Moreover, the central role of training extends far beyond the sciences, allowing for greater interactions with scholars in neighboring branches of history, sociology, and anthropology. While it is certainly no panacea, directing our attention to scientific training promises one way for us to learn from each other how the multifacted sciences—always partial and local—have stumbled along.

Discipline and Practice in the Königsberg Seminar for Physics (Ithaca, N.Y.: Cornell Univ. Press, 1991); John Rudolph, *Scientists in the Classroom: The Cold War Reconstruction of American Science Education* (New York: Palgrave, 2002); Andrew Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (Chicago: Univ. Chicago Press, 2003); Graeme Gooday, *The Morals of Measurement: Accuracy, Irony, and Trust in Late Victorian Electrical Practice* (New York: Cambridge Univ. Press, 2004); David Kaiser, *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics* (Chicago: Univ. Chicago Press, 2005); and Kaiser, ed., *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives* (Cambridge, Mass.: MIT Press, 2005). Similar themes are developed in Robert Kohler, *Lords of the Fly:* Drosophila *Genetics and the Experimental Life* (Chicago: Univ. Chicago Press, 1994).

[13] See, e.g., Joel Spring, *The Sorting Machine Revisited: National Educational Policy since 1945,* 2nd ed. (New York: Longman, 1989); Herbert Kliebard, *Schooled to Work: Vocationalism and the American Curriculum, 1876–1946* (New York: Teachers College Press, 1999); and Richard Arum and Irenee Beattie, eds., *The Structure of Schooling: Readings in the Sociology of Education* (Mountain View, Calif.: Mayfield, 2000). I explore these themes further in David Kaiser, "Introduction: Moving Pedagogy from the Periphery to the Center," in *Pedagogy and the Practice of Science,* ed. Kaiser, pp. 1–8; and in Cyrus Mody and Kaiser, "Scientific Training and the Creation of Scientific Knowledge: Historical, Sociological, and Anthropological Perspectives," in *Handbook of Science and Technology Studies,* rev. ed., ed. Ed Hackett *et al.* (Cambridge, Mass.: MIT Press, in preparation).