Research Schools and Their Histories

*By John W. Servos**

FEW ARTICLES have had greater impact on historians of modern science during the past twenty-five years than J. B. Morrell's "The Chemist Breeders: The Research Schools of Liebig and Thomas Thomson." Most of the papers in this volume of *Osiris* offer explicit homage to Morrell; all take his categories and questions as a point of departure. These papers constitute, in turn, a small fraction of a thriving literature devoted to the study of scientific research schools and their influence.[1] Morrell himself may not have created a "research school" in a narrow sense, since few of those scholars who share this interest have actually worked with him; but he helped launch a school in a broader sense—a tradition of thought and work directed toward the exploration of a subject that was inadequately treated by earlier scholars. In Morrell's case, this means the study of those laboratory-based research groups that have played important roles in the recent development of science.

This article will treat the provenance of the notion of research schools and the question of why this category has proved attractive to many historians of science. It will do so by attending to the ways in which historians and scientists have used the terms *school* and *research school.*

To outsiders, our clan can sometimes seem obsessed with words and their roots. Terms like *impetus* and *force* are the subjects of impressive scholarship, and rightly so. By tracing the development of such words, we gain some insight into the concepts and unarticulated assumptions of their users. Like other fields, ours has a history studded with examples of words and phrases that have, sometimes quickly and sometimes slowly, infiltrated our language and become organizing principles for investigation. Some have acquired special connotations because of their use within our field, and others have been borrowed from outside. A short list might include *paradigm, discourse, professionalization,* and *rhetoric.*

Historians generally have limited patience for studies of their own vocabulary.

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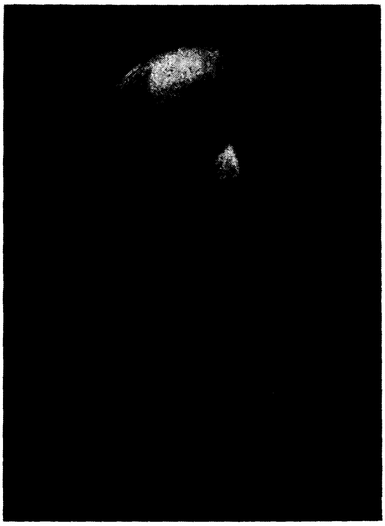
[1] See J. B. Morrell, "The Chemist Breeders: The Research Schools of Liebig and Thomson," *Ambix,* 1972, *19*:1–46. Gerald L. Geison reviews literature published before 1980 in his "Scientific Change, Emerging Specialties, and Research Schools," *History of Science,* 1981, *19*:20–40. Perusal of the most recent volume of *Isis Current Bibliography* reveals about a dozen titles that refer to scientific research schools, although the count must radically underestimate the size of the literature on the topic. Joseph S. Fruton, e.g., in *Contrasts in Scientific Style: Research Groups in the Chemical and Biochemical Sciences* (Philadelphia: American Philosophical Society, 1990), prefers the term *research groups* for laboratory-based circles of teachers and students (see his note, pp. 1–2). The notes and bibliography of Fruton's valuable book supply many references to recent writings on research schools.



Research schools, though useful "units of analysis" (see page 228), are not unambiguous ones. Below: *August Kékulé, although he "freed himself from the spirit of the school" (see page 4), went on to found his own. Courtesy of the Edgar Fahs Smith Collection.* Left: *Friedrich Kohlrausch also founded a school, but its distinctiveness was diluted once he published a textbook incorporating its practices (see page 28). Courtesy of the Chemical Heritage Foundation, Philadelphia.*

Better, most feel, to get on with the task at hand and leave such ruminations to the future. From time to time, however, it is good to pause and think about the origins and meaning of the words we use, and perhaps there is no better occasion for such reflection than the appearance of a volume, such as this one, that is organized around a fashionable phrase. By so doing we may not only learn something of the history of the phrase itself, but also understand better the origins of our historical consciousness of the institutions that the phrase describes, the nature of the project that engages students of those institutions, and the strengths and limitations of that project.

Although the expression *research school* seems so useful and right as to have had a venerable history, its frequent use in recent scholarship seems to have no parallel in earlier times, either among historians or scientists themselves. Of course, the unadorned term *school* has seen much service. Aside from its ordinary brick and mortar meaning, it has long been applied to groups, sometimes but not always teachers and disciples, that are united by the possession of common doctrine, method, or style (e.g., "the school of Aristotle" or "the Cartesian school"). While scientists sometimes use the word in such neutral ways, they also deploy it as a derogatory term. Such usage was especially common in the decades around the turn of the century, although not confined to that period. Groups deemed to have an unreasoned commitment to some pet theory or doctrine were labeled, usually by their critics, "schools." The *Jahresbericht* of Justus Liebig and Hermann Kopp differed from that of J. J. Berzelius, wrote T. E. Thorpe, because "it was to be done impartially, and with no special reference to any set of dogmas or particular school of chemical thought." The school of Wilhelm Ostwald, according to Henry E. Armstrong, regarded "all unbelievers as heretics worthy of the stake," commanded the "obedience of scientific youth," and could censor criticism since "[a]ll the major channels of communication and most of the minor are secured by the high priests of the cult."[2]

Membership in a school could impair objectivity; it could also close imaginations. Had August Kekulé "been shortsighted enough to accept the assistantship which Liebig offered him," Francis R. Japp wrote, he "might have gone on producing research work cut to a single pattern . . . and so on to the end of the chapter." But "Kekulé always emphasised the necessity for getting rid of preconceptions due to early training. 'Free yourselves from the spirit of the school,' he said; 'you will then be capable of doing something of your own.' " By choosing to work with other master chemists, Kekulé liberated himself from a potentially oppressive regime and found the freedom to express his creative genius. In his words, "Originally a pupil of Liebig, I had become a pupil of Dumas, Gerhardt, and Williamson: I no longer belonged to any school."[3]

Prolonged immersion in a "school" could inhibit budding genius; polemics between the leaders of "schools" could waste energy and effort; the hegemony of a single "school" could impede progress—the literature of nineteenth-century sci-

[2] T. E. Thorpe, "The Life Work of Hermann Kopp," in *Memorial Lectures Delivered before the Chemical Society, 1893–1900* (London: Gurney & Jackson, 1901), p. 780; and Henry E. Armstrong, quoted by R. G. A. Dolby, "Debates over the Theory of Solution: A Study of Dissent in Physical Chemistry in the English-Speaking World in the Late Nineteenth and Early Twentieth Centuries," *Historical Studies in the Physical Sciences* (*HSPS*), 1976, 7:297–404, on pp. 346, 387.

[3] Francis R. Japp, "Kekulé Memorial Lecture," in *Memorial Lectures*, pp. 99, 98.

ence is replete with cautionary tales about such dangers. At the same time, contemporary observers were not blind to certain advantages that schools offered. Whatever the implications for the creative development of their students or the conceptual advance of their disciplines, scientists who founded schools made capital contributions to industrial progress and national prestige. Thus Henry Armstrong praised "the Hofmann school" for contributing to the progress of industry by training technical chemists, and J. M. Crafts lauded Adolphe Wurtz and Charles Friedel for fashioning a "school . . . bound together by a common regard and by community of view," that "became an important factor in the nation's progress."[4]

Among those scientists and observers of science who used the term *school* in the nineteenth and early twentieth centuries, none was fonder of it than that Scottish polymath John Theodore Merz. His *History of European Thought in the Nineteenth Century,* still unsurpassed for scope, deploys the term promiscuously to describe the tightly focused schools of research of German laboratories, the intellectual affinities of individuals linked by association with a particular university or by citizenship in a single city or nation, and traditions of thought that span centuries, national boundaries, and linguistic groups. Members of his "schools" might look to common masters, but they might also simply share common interests, or ideas, or methods, or styles of thought. Thus we find references to Berzelius's "school of chemistry," to the "Berlin school of medicine," to "the modern English school" of biology "headed by Darwin," and to the "Scotch school of philosophy."

Like many of his contemporaries, Merz saw hazards in schools:

> At the time when the mathematical and physical sciences were leading the way in France, and gradually forcing their way into Germany, most of the universities in the latter country had one or more representatives of that new and apparently promising school which termed itself the "Philosophy of Nature." The trammels of this school had to be shaken off by those who . . . took up the cause of the exact or mathematical sciences.

But such passages are rare in Merz. He was far more impressed with the school's capacity to extend the influence of exceptional leaders by undertaking "to finish what the master has begun, to carry his ideas into far regions and outlying fields of research, or to draw their remoter consequences." Especially important here were the laboratory-based schools of German-speaking central Europe:

> Wherever the progress of learning and science requires a large amount of detailed study inspired by a few leading ideas, or subservient to some common design and plan, the German universities and higher schools supply a well-trained army of workers, standing under the intellectual generalship of a few great leading minds. Thus it is that no nation in modern times has so many *schools of thought* and learning as Germany, and none can boast of having started and carried through such a large number of gigantic enterprises, requiring the co-operation and collective application of a numerous and well-trained staff. The university system, in one word, not only teaches

[4] Henry E. Armstrong, "Notes on Hofmann's Scientific Work," *ibid.,* pp. 637–638, 640; and J. M. Crafts, "Friedel Memorial Lecture," *ibid.,* p. 993.

> knowledge, but above all it teaches *research.* This is its pride and the foundation of its fame.[5]

Scientists like Liebig and Friedrich Wöhler, Johannes Müller, and J. E. Purkyně might deploy different ideas and methods, but all were enrolled in a progressive movement and all were contributing to the advance of exact science. Britain, with its tradition of intense individualism, had much to learn from these continental examples, although Merz could not resist noting that individualism, too, had its virtues: "Minds like Newton and Faraday, full of new life, but modestly content with deepening and strengthening their secluded vigour, refrained from boastful publicity or ostentatious parade, working for all ages rather than for a special school or a passing generation." Even this celebrant of the "school of research" (a phrase Merz may have coined) salted his enthusiasm for the institution with some of the reservations of his contemporaries.[6]

Nineteenth-century scientists thus were ambivalent about the associations of students and teachers that we commonly call research schools. While recognizing their efficiency in transmitting technique, they found it hard to reconcile their methods with widely held notions about the norms and values of science and the workings of creative genius. Schools might train, but could they educate or liberate? That is, could they ever foster among students imagination and independence of mind?

This ambivalence, so common in the generations that created the first research schools, finds echo in the writings of historians, sociologists, and philosophers of science of the mid-twentieth century. Even those scholars who were instrumental in focusing attention on the community structure of science, most notably Derek J. de Solla Price, Diana Crane, and Thomas S. Kuhn, treated "schools" with reserve, not to say disdain.

Price was concerned, first and foremost, with the ways in which social arrangements and institutions adjust themselves to the problems posed by the expansion of the population of scientists and the growth of scientific knowledge. In an argument familiar to most historians of science, he contended that research scientists can keep up with the work of a community of other scientists limited to perhaps a hundred or so individuals and that invisible colleges form naturally as a consequence. These colleges are composed of individuals who exchange preprints and reprints, attend conferences and meetings together, and share research questions and techniques. Although Price borrowed the term *invisible college* from the seventeenth century, the groupings that best illustrated his idea were the informal networks of twentieth-century science that form and reform around such institutions as the Rochester Conference for fundamental particle studies. Such institutions, Price suggested in a closing flourish, have supplanted associations of "the great professor with his entourage of graduate students, the sort of thing for which Rutherford or Liebig are well known. The great difference here is that the apex of the triangle is not a single beloved individual but an invisible college; its locale is not a dusty attic of a teaching laboratory but a mobile

[5] John Theodore Merz, *A History of European Thought in the Nineteenth Century,* Vol. I, *Scientific Thought* (London: Blackwood, 1904; New York: Dover, 1965), pp. 204, 250, 167.

[6] *Ibid.,* pp. 205, 278 (quotations). See also, e.g. p. 167.

commuting circle of rather expensive institutions."[7] Schools and invisible colleges perform similar functions in Price's view, since both serve to subordinate young, inexperienced, or unimaginative scientists (who constitute the great majority) to leaders with more wisdom and talent. Invisible colleges, however, are more effective at the job, at least in a world of telephones and jet planes, since they give research groups the capacity to respond more quickly to changing methods and ideas.

Kuhn, influenced perhaps by scientists' usage, adopted "school" to describe groups that, although engaged in the study of the same parts of nature, cannot agree on fundamentals. In the first edition of *The Structure of Scientific Revolutions,* he relegated such schools to the prehistory of science. Maturity comes to a science when the anarchy of the schools is supplanted by the orderly puzzle solving of "communities" that share allegiance to certain fundamental methods and concepts. In his essays of the early 1970s Kuhn modified this position, suggesting that "there are schools in the sciences, communities, that is, which approach the same subject from incompatible viewpoints. But they are far rarer there than in other fields; they are always in competition; and their competition is usually quickly ended."[8] Whether Kuhn did irreparable harm to his original concept of "normal science" by such qualification is less important, from the present standpoint, than that he continued to treat schools as something of an embarrassment in mature sciences. Philosophers, artists, and sociologists may be analyzed in terms of their affiliations with schools, but scientists are best viewed as members of other kinds of communities, especially disciplines or specialties and, at a lower level, invisible colleges. If schools exist in a mature science, they are fleeting associations whose rivalries exist only within the larger and more enduring frameworks of common belief and association. While endorsing research into the social structure of science, Kuhn exhibited little enthusiasm for the school as a focal point of such inquiry.

Crane accorded a much more important role to research groups built around master and students than Price or Kuhn, all the while denying the appropriateness of the word *school* to describe them. "Solidarity groups," she suggested, coalesce around influential teachers who recruit and socialize new members, define the important problems for research in their specialties, and interact with members of other solidarity groups through "communication networks" or invisible colleges. As in schools, membership in solidarity groups implies some allegiance to a common "point of view." But these solidarity groups should not be confused with schools since "a school is characterized by the uncritical acceptance on the part of disciples of a leader's idea system. It rejects external influence and validation of its work. By creating a journal of its own, such a group can bypass the criticism of referees from other areas." Members of solidarity groups are capable of criticizing one another's ideas and interact with the members of other solidarity groups through invisible colleges. Schools, by contrast, are insular and intolerant of dissent. They, she added in a note, are like religious sects, which "break away from the church and build separate organizations,

[7] Derek de Solla Price, *Little Science, Big Science* (New York: Columbia Univ. Press, 1963), p. 90.

[8] Thomas S. Kuhn, *The Structure of Scientific Revolutions,* 2nd ed. (Chicago: Univ. Chicago Press, 1970), p. 177.

emphasizing aspects of doctrine or policy that they believe have been ignored or misinterpreted by the church. The religious sect is a relatively closed system that resists external influences rather than attempting to adapt to them. Members who deviate from orthodox views on any issue are quickly expelled." Like Kuhn, Crane reserves the term *school* for those associations which do not measure up as being truly scientific.[9]

The contrast between these earlier references to schools, many of which amount to sneers, and more recent studies of research schools, many of which are celebratory, is striking. Insofar as the shift can be followed on paper, it commenced in 1969, with the publication of Owen Hannaway's review of Maurice Crosland's *The Society of Arcueil.* In this shrewd notice Hannaway argued that the illustrious *savants* who congregated in the country home and laboratory of C. L. Berthollet are best viewed not as a circle of influential individuals nor as a society of scientific peers but rather as "a school" in which the relationship between Berthollet and his associates "was that of master and pupils." Echoes of an earlier ambivalence about schools were present. "The research problems the apprentices worked on," Hannaway wrote, "were grounded in the work of their seniors, and their conclusions were frequently influenced by their mentors' prejudices." Nevertheless, Hannaway was more concerned with the role of such schools in the "professionalization of science in the nineteenth century" than with any limitations they might place on individual expression. Here, Hannaway suggested, the principal significance of the Society of Arcueil may have been as a model and inspiration for the young Liebig—a model, that is, of "a research school."[10]

The expression had not been much used between the time of Merz and Hannaway's review, but references to research schools proliferated quickly in the early 1970s. Especially noteworthy were Jerome R. Ravetz's provocative and wide-ranging *Scientific Knowledge and Its Social Problems,* Morrell's study of Liebig and Thomson, and Robert Fox's article on the rise and fall of Laplacian physics.[11] All made the research school a central category in their analyses, all cited Hannaway's review as the source of the expression, none felt the compulsion to apologize for its use. The old word *school* could almost be said to have been rehabilitated by modifying it with the word *research.* It is worth inquiring into the reasons for the sudden popularity of this expression and asking whether its growing use was merely an accident of fashion or denotes a more fundamental change in the way historians viewed science.

Of these questions, the first is perhaps the easiest to answer, and it may best be approached by considering the factors that recommended the most influential of these essays, that of Morrell, to historians of science. When published in 1972, Morrell's paper found a receptive audience among readers concerned to make the history of science more sensitive to social context, more comparative, and more fully historical. It is customary to invoke the name of Thomas Kuhn when dis-

[9] Diana Crane, *Invisible Colleges: Diffusion of Knowledge in Scientific Communities* (Chicago: Univ. Chicago Press, 1972), pp. 34–35, 87 (quotations).

[10] Owen Hannaway, review of Maurice Crosland, *The Society of Arcueil: A View of French Science at the Time of Napoleon I,* in *Isis,* 1969, *60*:578–581, on p. 581.

[11] Jerome R. Ravetz, *Scientific Knowledge and Its Social Problems* (New York: Oxford Univ. Press, 1971); and Robert Fox, "The Rise and Fall of Laplacian Physics," *HSPS,* 1974, *4*:89–136.

cussing these trends, and not without cause.[12] By undermining claims about the importance of some inflexible "scientific method" and attributing the special success of science to peculiar features of its social organization (albeit not to schools), Kuhn's *Structure of Scientific Revolutions* offered historians of science a powerful justification for studying the institutions in which science was done. His habit of seeking patterns through the consideration of scientific ideas separated by time and place, a method not unknown to historians of science but more natural to colleagues in philosophy, emboldened others to think more comparatively about the past. And while his essay was less an example of historical scholarship than a reflection on its uses, *Structure* offered historians of science a persuasive rationale for putting aside whiggish concerns about questions of priority, precursors, and the validity of scientific ideas of the past—concerns that had long sidetracked the inquiries of historians, amateur and professional alike. His essay preached a humility about the present and respect for the past that is essential to sensitive historical inquiry.

With or without Kuhn, however, historians were gravitating toward study of the social institutions of science. This movement reflected political and social forces far larger than the history of science: the resurgence of traditions of scholarship inspired by Marx, the remarkable growth of social history and sociology during the 1950s and 1960s, and changes in the recruitment and education of historians of science that attenuated their links with the sciences and strengthened their links with history. Perhaps most important, however, were the efforts to open up the science of the nineteenth and twentieth centuries to historical study. The size, complexity, and technicality of modern science posed formidable challenges to historical research and even more daunting obstacles to historical teaching. It seemed possible to discover thematic unities in the science of earlier eras without egregious simplification and to convey those themes to students with modest backgrounds in science and mathematics. Modern science resisted such treatment. To be sure, historians could offer students epilogues of sorts to the Scientific Revolution by picking their targets carefully. A few topics in modern science could even be treated in some depth. The history of evolutionary biology, so long as it does not press too far into genetics, embryology, or debates over systematics, offers one such example. But the prospects of writing a history of modern science serviceable in undergraduate classrooms or even graduate courses appeared dim—at least if such synthetic efforts took as their model the books that served historians of ancient and medieval science or the Scientific Revolution so well. It seemed even less plausible to think that such syntheses, if produced, could ever appeal to "general" historians or others outside the profession.

As Kathryn Olesko points out so skillfully in her contribution to this volume and elsewhere, the demands of the classroom shape traditions of research, and this is as true in the history of science as it is in the sciences themselves. The real genius of Morrell's essay was, I would suggest, that it constituted an example of how modern science could be handled in ways that are both historically sensitive and eminently teachable. And in doing so, it suggested avenues by which much

[12] See, e.g., Roy Porter, "The History of Science and the History of Society," in *Companion to the History of Modern Science,* ed. R. C. Olby, G. N. Cantor, J. R. R. Christie, and M. J. S. Hodge (London: Routledge, 1990), p. 38.

additional research into and teaching about modern science might be organized into similar form.

Morrell's organizing principle, of course, was the research school—an institution that flourished in the universities and research institutes of the nineteenth and twentieth centuries, an institution that depends on patrons, regular infusions of students, a ready supply of problems solvable in limited time by predictable methods, reliable means of reaching readers, and leaders capable of directing efforts along profitable lines. Nonspecialists could take from Morrell's concise account a sense of how Liebig's science differed from its antecedents and recognize in his description of the research school an institutional form still extant. Specialists recognized in his article categories that promised utility in the analysis of all kinds of laboratory science, assertions that could easily be turned into questions when deployed in other contexts, and appealing associations with other bodies of literature.

Indeed, although much of his article consisted of an exposition of the careers of Liebig and Thomson, Morrell missed few opportunities to link his inquiry with emerging themes in the history of science. His description of Liebig as a successful entrepreneur and his laboratory as a "knowledge factory" reverberated among readers familiar with recent studies of business leadership. His suggestion that "Big Science began at Giessen in the early 1840s" suggested continuities between Liebig's laboratory and the industrial-scale science of the twentieth century. His decision to contrast Liebig with a British contemporary brought his inquiry into relation with the question of national styles in science and more particularly with the issue of why Germany seized leadership in most branches of laboratory science in the nineteenth century. His reference to the role of research schools in "the expansion of specialization" raised questions about the emergence of new disciplines. His attention to the role of the technique of combustion analysis at Giessen and his interest in the routine behavior of his subjects seem prescient in view of recent efforts to take laboratory practice seriously. His emphasis on the role of charismatic leadership and personal discipleship in the transmission of craft knowledge linked his work to Michael Polanyi's writings on the tacit component of knowledge. Small wonder that many historians of science have made research schools a focal point of investigation during the past twenty years. Their study has offered many sorts of students of modern science a common port of entry and departure.

It has also been a safe harbor for those historians, which is to say most historians of science, who felt uneasy with extreme forms of idealism or social determinism and with dogmatically internalist or externalist approaches to historical analysis. The research schools of Liebig and Thomson, as depicted by Morrell, were shaped by "intellectual, institutional, technical, psychological and financial circumstances." Their success or failure could not be reduced to the presence or absence of some one essential ingredient; their study demanded that attention be paid to student populations and account books, to research apparatus and journals, to group dynamics and individual psychology, to the research programs of individuals and the intellectual traditions of their communities, to university politics and the goals of patrons. Without polemic or jargon, and without making an exhaustive survey of these circumstances, Morrell presented readers with a wonderful sketch of how cognitive, social, and material factors interacted to generate different levels of success in the efforts of his historical actors. While opening many doors, he closed none.

Morrell's influence on later writers testifies to the enduring power of eclectic forms of history to excite imaginations. Many historians of science have come to think of research schools as elemental to modern science. They are the vehicles by which knowledge, especially tacit knowledge, is transmitted. As such they may play a role in the development and preservation of national and regional styles in science. They are natural units within universities and certain other institutions for scientific research. They are the collectives that exploit and articulate the ideas of their individual members. They compete with one another for patronage, for space in journals, for students, for prestige, and for influence in disciplines. They are the wombs within which new concepts and methods develop and, sometimes, new specialties. Larger aggregates, such as the science faculties of universities and disciplines, may be resolved into these elements; individual scientists in many fields find full opportunity for expression only within them.

How does this view of the research school differ from that which earlier writers endorsed? The differences appear to be threefold. First, and perhaps most important, recent work suggests that conflict is far more important to the normal processes of science than earlier observers were willing to allow. Our scientific specialties frequently embrace schools that can hold incompatible views of what constitute proper questions, methods, and answers. As Steven Turner's essay in this volume shows so elegantly, members of such schools can even speak different languages. And rival schools may coexist, not peacefully, for decades and even generations. The work of the past two decades, while drawing some inspiration from Kuhn, has undermined part of his thesis. Conflict is not so easily segregated from consensus in the history of science as his model suggests, a point that David Kushner makes explicitly in his contribution on George Darwin and geophysics.

Nor can recent work sit in perfect comfort alongside that of Price and Crane. Like Kuhn, they stressed the cooperativeness of scientists and minimized conflict. Scientists may bring different viewpoints to the invisible colleges in which they participate, but they subordinate their differences in the interest of achieving the rewards of membership in larger communities. The invisible colleges described by Price and Crane surely exist, yet recent studies of research schools suggest that they are not so much devices for regimenting atomistic scientists (Price) nor networks for connecting generally cooperative solidarity groups (Crane) as arenas in which members of rival schools compete.[13] Dominance of such networks may confer prestige and resources on one or another school, but such dominance often eludes any single group.

A second shift in the assumptions governing the way historians view research schools (and, indeed, schools more generally) is that during the past twenty years they have tended to apply different normative standards to their subjects than did earlier writers. Instead of viewing manifestations of interestedness among scientists as infractions of the methods or moral code of science, recent writers tend to

[13] Such a picture emerges from historical studies of research schools but is best captured by Bruno Latour and Steve Woolgar in *Laboratory Life: The Social Construction of Scientific Facts* (Beverly Hills, Calif.: Sage, 1979).

accept such behavior as normal and inevitable. Value judgments, when ventured at all, are now directed toward evaluating the fruitfulness of different lines of inquiry. These evaluations sometimes reflect the influence of Imre Lakatos and his effort to appraise research programs as "progressive" or "degenerative" on the basis of their capacity to generate novel predictions and theories.[14] The conceptual categories of the philosopher of science are augmented and complicated a good deal, however, by historians. Not only must successful research schools promote ideas that are reasonably coherent and fecund; they must also be effective in recruiting students, mobilizing material resources, and propagating their message. As in Darwinian natural selection or economic competition, the factors making for success are complex and context-specific. Indeed, the very same factors that may lead to prosperity in one setting can be liabilities in others.

Third, unlike earlier observers, writers of the past twenty years have been little troubled by any dichotomy between originality and "schooling." They typically see no necessary conflict between training and education, between the acquisition of proficiency in some body of ideas and techniques and the vigorous expression of creative energy. The capacity to be inventive cannot be taught like multiplication tables, but under the proper regime it can be cultivated. Indeed, many recent studies have stressed the importance of rigorous apprenticeships in the development of exceptional creative talent.[15] These may entail the transmission of a kind of craft knowledge of the fine points of laboratory technique or of the subtleties of mathematical analysis. But perhaps even more important may be the broader lessons that proven research scientists may impart—lessons about where to look for questions, how to phrase them, and how to apportion the scarcest of resources, time. As Joel Hagen points out in his contribution to this volume, not every research director, or would-be research director, proves an effective model for students. But the frequency with which eminent scientists emerge from the laboratories of other eminent scientists belies any suggestion that "schooling" dulls imagination. Tightly focused research schools may turn out hacks, but they also turn out highly original scientists and do so with regularity. By stressing the role of apprenticeship in the creative work of science, recent studies of research schools have contributed to that broader demystification of genius which has been an important theme in the modern history of science.

Consideration of the changing fortunes of the word *school* reveals to us how much the history of science has changed in the last twenty years. Perhaps it is appropriate to conclude by asking if recent work on the history of research schools can withstand the same sort of scrutiny that we apply to our historical subjects. Can the study of research schools, that is, still move us forward in our study of the

[14] Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in *Criticism and the Growth of Knowledge,* ed. Lakatos and Alan Musgrave (Cambridge: Cambridge Univ. Press, 1970), pp. 91–196.

[15] See, e.g., Fruton, *Contrasts in Scientific Style* (cit. n. 1); Robert G. Frank, Jr., "American Physiologists in German Laboratories, 1865–1914," in *Physiology in the American Context, 1850–1940,* ed. Gerald L. Geison (Bethesda, Md.: American Physiological Society, 1987), pp. 11–46; and Jack Morrell's essay in this volume. Historians were not the first to explore this subject; see Robert K. Merton's 1968 essay, "The Matthew Effect in Science," in *The Sociology of Science: Theoretical and Empirical Investigations* (Chicago: Univ. Chicago Press, 1973), pp. 452–453.

history of modern science? The essays collected in this volume suggest that there is a future, although they also reveal some of the difficulties and limitations of organizing historical research around research schools. Such studies do not have boundaries as clear and unambiguous as do the lives of scientists or the histories of institutions that are coincident with physical or legal structures. Several contributors have difficulty fitting their materials to the category; most find it necessary to stretch or qualify it. But such discomfort is hardly unusual. Historians have long encountered similar difficulties with such categories as *class* and *elite.* Demarcation problems may be discouraging, but they need not be fatal. Indeed, as several of the essays here collected demonstrate, efforts to compare the tightly focused and laboratory-based research schools of a Liebig or a Michael Foster with looser confederations of scientists can result in useful and important insights. Whether Kushner's George Darwin or Turner's Hermann Helmholtz led research schools is moot; but surely we see them more clearly for being able to compare them and their circles with communities that come closer to meeting Geison's now-canonical definition of the research school as "small groups of mature scientists pursuing a reasonably coherent programme of research side-by-side with advanced students in the same institutional context and engaging in direct, continuous social and intellectual interaction."[16]

In a more general sense, work on research schools must lead historians to demarcation problems, since, as Geison pointed out a decade ago, the study of the schools cannot be entirely severed from investigation of the individuals that compose them and the larger networks through which they interact.[17] On the one side their study merges with biography; on the other with the histories of disciplines, universities, traditions of thought, and even national styles of science. Far from being a liability, however, this situation is advantageous, since it gives the historian license to move freely from consideration of the largely private realm of creative effort to the more public arena of justification and persuasion.

More troubling than any problem of demarcation are three issues associated with the basic assumptions of recent work on research schools. The first is the growing tendency to discount the role of consensus in scientific conduct. That conflict and competition exist and are normal parts of science can hardly be denied, but are they as pervasive as recent literature suggests? Historians thrive on conflict. Not only does it give us the opportunity to study individuals, ideas, and institutions under stress, when their strengths and weaknesses become most apparent, but it also affords us much-prized elements of drama. Research schools, with their entrepreneurial leaders and ambitious disciples and their webs of personal loyalties and enmities offer conflict-minded historians ample material. But to what extent does our instinct for the good story dictate our choice of topics and color our picture of scientists' behavior? We tend to see in the past that which we seek. Were we to look harder for harmony and cooperation among research schools, would we find it? Need a recognition of the importance of "schools" in science imply adversarial relations among those schools as a correlate?

A second question grows out of the criteria that historians use to evaluate research schools. We no longer accuse those who backed the wrong horse of crimes

[16] Geison, "Scientific Change" (cit. n. 1), p. 23.
[17] *Ibid.,* p. 35.

against science, but we continue to judge some scientists and their schools successes and others failures. It is right and proper, and perhaps inevitable, to do so. Even if we are reluctant to offer an opinion as to whether so-and-so's work was "any good," we must ascertain and explain the judgments made of that work by our subject's contemporaries. But as critics of Lakatos's writings have pointed out, evaluating the fruitfulness of research programs is a tricky business, and judgments of the success or failure of research schools often entail such evaluations.[18] The normative standards we apply to our subjects may be different from those applied by our predecessors, but they may be no more secure.

The third question arises from the recent inversion of the old relationship between originality and schooling. We have abandoned myths of heroic genius by linking the fulfillment of creative potential to apprenticeship in research schools. But have we demystified genius or simply substituted one form of mystification for another? Olesko rightly criticizes those who would wrap an impenetrable cloak around the acquisition of research skills. When we say that research schools are efficient at breeding creative scientists because they are effective in transmitting tacit knowledge of craft skills from masters to disciples, are we not obscuring precisely what needs to be illuminated?

Schools teach. Or so the theory goes. But what? Any parent will know that it can be difficult, sometimes depressingly so, to discern what Johnny learns by attending school. And anyone who has sat at both ends of the classroom will know that what teachers seek to transmit is not always what students receive, a point made recently by Lisa Rosner in her fine book on medical education at Glasgow.[19] What do research schools teach?

My own suggestion, for what it is worth, is that tacit knowledge of technique constitutes but a small part of what masters transmit to their disciples in such institutions. Far more important may be the guidance that they offer on the problem-structure of their disciplines and the enthusiasm and inspiration to persevere that some inspire through informal exchanges and example.[20] In the end we may find that such hard-to-specify knowledge (if such it should be called) can indeed be conveyed by routes other than personal contact. A textbook, for example, that is suffused with the persona of its author may be able to stand in for personal contact; one thinks of James D. Watson's *Molecular Biology of the Gene* or Richard Feynman's *Lectures on Physics.* But few scientists have such powerful personalities as Watson or Feynman or such facility at written expression. Far more numerous are those whose power to enliven their subjects extends little further than the laboratory bench or classroom, but who, within those precincts, show a remarkable ability to identify, groom, and inspire talent.[21] Be this as it may, Olesko's challenge is ignored only at our peril. Until the question of what is

[18] See, e.g., Thomas S. Kuhn's comments in "Reflections on My Critics," in *Criticism and the Growth of Knowledge,* ed. Lakatos and Musgrave (cit. n. 14), pp. 239–241, 256–259.

[19] Lisa Rosner, *Medical Education in the Age of Improvement* (Edinburgh: Edinburgh Univ. Press, 1991), p. 2.

[20] See the sources cited in note 15 above.

[21] E.g., Franz Hofmeister, Michael Foster, and Arthur A. Noyes: see Fruton, *Contrasts in Scientific Style* (cit. n. 1); Gerald L. Geison, *Michael Foster and the Cambridge School of Physiology: The Scientific Enterprise in Late Victorian Society* (Princeton, N. J.: Princeton Univ. Press, 1978); and John W. Servos, *Physical Chemistry from Ostwald to Pauling: The Making of a Science in America* (Princeton, N. J.: Princeton Univ. Press, 1990).

taught in research schools is more fully addressed, claims about their significance to modern science rest on incomplete foundations.

Despite these formidable difficulties, the study of research schools seems likely to remain a vitally important part of our effort to explore the history of modern science. These institutions are simply too important to the cognitive and social history of the enterprise and offer too many advantages as integrative vehicles to fall into historical neglect. And although recent changes in the social organization of certain sciences have led to new kinds of interactions among scientists, research schools have not disappeared.[22] "As the competition for funding and fame becomes ever fiercer among molecular biology laboratories," a recent issue of *Science* tells us, "conversation among colleagues often turns to questions of style: What manner of lab is best for producing good science and staying competitive? . . . Must large labs sacrifice creativity for efficiency? How many people and research projects can one investigator manage? Under what conditions might a lab director lose track of the papers to which he is signing his name—risking becoming party to fraud, misconduct . . . or just plain embarrassment?"[23]

The study of laboratory-based research schools, it would seem, offers rewards not only to those whose primary concern is the past, but also to those whose eyes are on the present and future.

[22] See Peter Galison, *How Experiments End* (Chicago: Univ. Chicago Press, 1987), pp. 275–276.
[23] Marcia Barinaga, "Laboratories of the Famous and Well-Funded," *Science,* 1991, *252*:1776.