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THE HISTORIES OF SCIENCE:
DIVERSE WORLDS FOR DIVERSE AUDIENCES

The invitation to deliver a keynote address at the XVII International Congress of History of Science has been for me a source of both pride and pleasure, the more so because it returns me to Berkeley where my work as program-builder in history of science began. But the assignment has been also a source of perplexity, the main residue of which is the distance between my title, supplied several months ago, and the remarks I shall actually be making today.

As many of you know, my career has moved back and forth between history of science and philosophy of science, fields which, in the English-speaking world, have seldom much communicated. A decade ago my pendulum swung back to philosophy, since which time my colleagues, students, teaching, and reading have been primarily in that field rather than history. As a result, preparing for this occasion has required me to behave like an awakening Rip Van Winkle. I have had to begin, that is, by looking about, seeing what has happened while I slept, and puzzling about how to fit the current scene into the developmental patterns I knew in my earlier life.

• For the required explorations I have leaned hard on the programs of three International Congresses: the VIth at Amsterdam in 1950, the first which I attended; the Xth at Ithaca in 1962, the first held in the United States; and, finally, this Congress in Berkeley, the XVIIth. Taken together, they display several developmental highpoints, of which I shall here concentrate primarily on three. Let me begin by describing them, after which I shall briefly discuss two problems they reveal.

The most obvious change displayed by a comparison of the three Congresses is, of course, very rapid growth. Fewer than 70 papers were abstracted for delivery in Amsterdam, about 220 for Ithaca, and about 725 for delivery here.¹ Between each of these Congresses the number of papers slightly

¹These figures and those that follow are products of a very rough and ready count: quantitatively they are not to be relied on. But the qualitative phenomena they display are, with one exception to which I shall return, too marked to be erased by statistical errors. For help in counting, as well as for much thoughtful discussion, I am greatly indebted to John Heilbron.

more than tripled, a fact which illustrates the speed of development—an increase by a factor of ten in thirty-five years—but which also suggests a puzzle about its nature. There is no way these figures can be fitted to an exponential curve. The interval between Ithaca and Berkeley is twice that between Amsterdam and Ithaca. If growth were normal and the number of contributions tripled in the first interval, then it should have increased ninefold in the second. Either the initial growth was somehow artificially stimulated or the profession reached the top of Derek Price's S-curve exceptionally fast. I am quite sure both are the case, but the first is of greater present interest, and I shall be returning to it. Think of it as forced growth.

What accompanied this growth is a remarkably rapid shift in the temporal center of gravity of the field. At Amsterdam about 70% of all papers dealt with topics before 1750: i.e., with antiquity, the middle ages, and the Scientific Revolution. Only 10% were on topics falling between 1750 and 1880, and there were none at all on the years that followed. (The papers not accounted for, 20% of the total, were not periodizable.) By the Ithaca Congress twelve years later there had been a decided change. Topics before 1750 had fallen from 70% to 40%. The period 1750—1880 now contained over 30% of all papers, and a few papers, about 7%, dealt with topics falling in the last 100 years. Thus, very nearly as many papers were devoted to the period after 1750 as to the years before, a vast change from Amsterdam. And at this Congress the transformation appears complete. Almost half the papers to be presented here deal with topics since 1880. A full two-thirds are devoted to the years since 1750. The earlier years, to which 70% of the Amsterdam papers were devoted, are here the subject of only a quarter of the contributions.

A third change revealed by the comparison of Congresses is at least equally striking, especially because it developed at a different rate. To isolate it, restrict attention to papers on substantive topics in history of science, omitting, that is, papers on technology and medicine as well as those on such topics as historiography, research tools, and philosophy of science. Most of the papers at any Congress remain, and they break down in the following way. At Amsterdam, over 90% of these papers approached science through the history of ideas and techniques. Only 7% dealt at all with questions of institutional or social history of science. At Ithaca the situation was a bit different, but not very much. There, 15% of the papers were in the social or institutional historical mode. The remaining 85% were traditional history of ideas. Given the small numbers at Amsterdam and the difficulties of classification, that difference may not be significant. But at this Congress the significance of the shift cannot be questioned. Fully half the substantive papers abstracted for this Congress are institutional or social-historical in their orientation. Parity has clearly been reached.²

²I have the impression that the shift was far more pronounced for contributions from the English-speaking world than for those from the European continent.

Looking over the titles and abstracts of the papers for these Congresses suggests much else that might appropriately be discussed, and it adds up to a record of substantial progress: increased attention to fields of study and to geographical areas that were badly underrepresented in the past; splendid new research tools, like biographical dictionaries, manuscript archives, and computerized indices to make them accessible; and so on. Given that there are more of us, that we stand on the shoulders of giants, and that the use of these tools is compulsory for professionals, we have immensely increased both the amount and the sophistication of what is known in the field. And finally, though we still write mostly for one another, we have acquired a serious audience among philosophers, sociologists, and historians—an audience that, except within continental philosophy, scarcely existed before.

All of this you know as well as I, however, and I shall not elaborate it further. Instead, I want to stay with the three main trends just sketched: sudden growth, a marked shift in the field's temporal center of gravity, and a great change in the balance between intellectual and social historical studies. These changes are not so much growth as transformation (I shall not say a revolution), and I want to ask how they came about. Speaking to that question I shall have to be both personal and parochial. Personal, because the question has not been much studied, and I must rely on my own experience. Parochial, because that experience is in the United States and decisively affected by the nature of U.S. institutions, especially educational institutions. It is central to the story I want to tell that in this country most historians of science are attached to colleges and universities, institutions whose central role is teaching. This situation is not unique to the United States, but it is not universal either. One thing that people at this Congress might well discuss is how much difference it makes. Pending such discussion, I shall simply assume that the forces I am going to single out can be found at work in other countries, too; and that they have there often produced comparable results, though through different institutional channels.

To begin with, let me remind you of the position of history of science courses in this country before the Second World War. There were not many of them, but most of those that existed were taught by scientists, within their own departments, and for the benefit of science students. Exposure to the history of one's own science would, these scientists believed, help to locate the future professional in his or her field. It would, that is, contribute to the formation of the student's identity as a scientist.

That situation did not survive the war, and for two very different reasons. First, the pace of scientific research quickened rapidly after the war, and the result was a corresponding intensification of competitive professional pressures. Increasingly, scientists felt that neither they nor their students had time for history. It had become a luxury they could not afford. Having been set aside for the duration of the war, it seldom reappeared in science departments thereafter.

Just as history was disappearing from science departments, however, a

second effect of the war began to be felt. People everywhere emerged from it with an acute awareness (sometimes fear) of the power of science and of its potential social importance. The scientific enterprise had changed the world in totally unforeseen ways, and it would doubtless continue to do so. How, people asked themselves, was its power for good or evil to be managed and controlled? The felt difficulty was that only scientists appeared to understand science. And it was widely agreed, often among scientists themselves, that the social consequences of their enterprise were too vast to be left exclusively to them.

As a result, there was a widespread sense, at least in this country, of the urgent need to provide both the electorate and the officials they elected with an understanding of science sufficient to permit intelligent decisions about its use. One must find ways, it was repeatedly said, to bridge the gap between what C.P. Snow would later describe as "The Two Cultures". To many people history of science appeared a promising way to bridge that gap. James Conant of Harvard provides a prominent example. He described how history might be effectively used in a little book called *On Understanding Science*. And it was an exposure to his method in action that first attracted me and some others in the new generation to the field. There were a number of other similar routes as well.

Results of this new motive for pursuing history of science were scarcely apparent at Amsterdam. An early paper by Derek Price on the science of science was perhaps the only visible exception. I and a few students like me may have been invisible ones. But by the middle 1950's the effects of the movement were unmistakable, for it began rapidly to open up new jobs. My arrival at Berkeley in 1956 was among the early ones. During the decade that followed, perhaps a hundred more jobs might have been filled in the U.S. if only there had been people to fill them. The future seemed unlimited and, for a time, it was. War-bred consciousness of the power of science was, I am thus suggesting, the primary cause of what I previously called the "forced growth" of history of science. It accounts for the explosive expansion of the field in the dozen years separating the Congresses in Amsterdam and Ithaca.³

Turning next to the qualitative changes in the field, one must begin by asking where the people who filled the new positions were placed. Very rarely, as I have indicated, did they go to science departments. They were not much wanted there, and the new movement was, in any case, directed to non-scientists rather than scientists. In fact, given the new conception of their role, historians of science had no natural home, and they were therefore placed in whatever part of the college or university could be persuaded to take them. More of them went to history departments than to any other single place, but not usually because historians themselves felt a need. Departments of history

³The rate of growth varied, of course, from country to country. Initially it centered in the United States, followed, I believe, by England and Canada. There are at least a few major countries in which it is still scarcely manifest at all.

had not traditionally provided a home for historians of other disciplines: art, music, or literature, for example. Why should they do so for science? But historians did sometimes assent when told by a university administration that they could have an extra position if only they would use it to appoint an historian of science. History departments were not, however, the only home found for us. Some of us went to philosophy departments. I myself was originally jointly appointed in philosophy and in history. A favored few went to special departments or programs created for them. And there were other *ad hoc* arrangements besides.

About the results of this manner of institutionalization, three things need to be said. First, if its object was to bring an understanding of science to non-scientists, it failed utterly. I doubt that even well-educated members of the general public understand science significantly better today than their parents did a generation ago, and I do not foresee future change. The two-culture problem seems to me rooted in the nature of developed science; it will not be resolved by bridging a gap; we require other ways to manage the problems that it creates. Besides, even if gap-bridging could be done, history of science on longer seems an effective way to go about it.

A second fact about the newly institutionalized movement is that for some time it did not vastly change the nature of history of science, just its size and, to a lesser extent, its scope. My generation did, I think, begin to move the center of gravity of the field forward in time, to bring our subject matter closer to the science experienced by our students. That showed clearly at Ithaca, as I have already indicated. In addition, something I did not point out earlier, we began to expand the range of sciences well-represented in the field. There was, for example, far more biological science at Ithaca than at Amsterdam, and the trend has continued. But, with a few noteworthy exceptions, our courses continued to be offered pretty much in the traditional history-of-ideas mode, the mode initially directed primarily to science students. The biggest qualitative change in history of science—the vastly increased role of social and institutional history—was the work not so much of the first generation of postwar historians of science as of their students, especially, but not exclusively, those trained or placed in history departments. Those students had not quite come of age at Ithaca: they have here!

Whatever my generation lacked, it was not models. We were well aware of the classic work—often Marxist in inspiration—of men like Hessen, Zilsel, and Merton. Much of it we admired, and we insisted that our students pay attention to it. In addition, we knew and used the standard institutional histories—Martha Ornstein's for example—and wished that more and better ones were available. But with few notable exceptions, mostly programmatic, we did not undertake that sort of work ourselves. To me it then seemed not quite history of science but rather an essential resource for practitioners of that field, a resource which might as appropriately be provided by general historians or by historically inclined sociologists as by me or my students.



Many factors have combined to change such attitudes, making what had once seemed tools now central to the substance of our field. Here I must settle for a very partial and impressionistic overview. First and foremost, of course, were intellectual considerations. Even those who, like myself, chose to concentrate their research on the development of scientific ideas could not conceivably view scientific development as the product exclusively of interactions between reason, on the one hand, observation and experiment, on the other. Historical forces of other sorts had shaped scientific development too; little was known about them; it seemed time to rectify the imbalance.

But considerations of that sort, though surely the primary motives for the new evaluation of social and institutional history of science, had been accessible for a long time before actual change occurred. Other factors are required to explain why the shift took place when and as it did. And the most central of these were, in my view, pragmatic consequences of the new objectives and institutional arrangements that were supporting the rapid expansion of the profession.

Both professional role and professional identity were deeply involved. The new history of science was, as I have said, to be directed primarily to non-scientists, to students who knew little science and who displayed a decided reluctance to learn much more. The new role of the historian of science thus required subject matter that could be taught to such students, a requirement incompatible with responsible treatment of most scientific ideas developed more recently than the Scientific Revolution. For those placed in history departments, professional identity was important as well. To the extent that historians of science and the future professionals they trained dealt primarily with scientific ideas, they found themselves painfully isolated from their departmental fellows. Turning to social and institutional problems sometimes seemed the only way to feel like an historian.

Whatever the mix of causes responsible for it, the shift to such problems is apparent in the balance of the papers on the program of this Congress. I welcome it, as I do the other changes I have so far discussed. But I cannot conclude this account of reasons for professional self-congratulation without confessing that, as I look back to my early years in the field, there is something that I miss, something I could not conceivably want back in the form it then had but which nevertheless badly needs to be replaced. I shall close by talking very briefly about it, for the task of replacement seems to me the outstanding challenge now facing the profession.

The older history of science, addressed primarily to scientists, saw itself as philosophy teaching by example. It displayed the irresistible march of humanity towards objective truth, the inevitable triumph of reason and method over ignorance and superstition. In those years one knew how science worked and what scientific progress was. Clearly no such view is remotely tenable today and for two very different sorts of reasons. The history of scientific ideas has shown that new science emerges, not from error or superstition but from

old science, the concepts fundamental to scientific work changing as the transition occurs. Simultaneously, the social/institutional history of science has shown that more than the interplay of observation and reason is important to an understanding of the shape and direction of scientific advance. Members of both historiographic groups often now converge in speaking of the world explored by scientists as a construction. Increasingly, one hears talk of the *construction* of scientific objects or of scientific facts.

As far as it goes, that way of speaking seems to me just right, but it does not go very far. What it leaves open are such questions as: what are the materials out of which these constructions are made? What are the principles of sound construction? What is the relation between older constructions and their newer replacements, the relation that makes the latter seem so much more powerful than the constructions they replace? It is not, of course, the responsibility of historians to answer such questions. That is more nearly the philosopher's job. But history—not every historian, but the historical profession—has a responsibility for helping with the problem, partly because historians played a primary role in the destruction of the traditional viewpoint, and partly because their works are going to be read as supporting one or another answer whether or not the authors of those works intend that they should.

People who care about such philosophical issues currently tend to read historians of science as conveying one of the two following views. Those who emphasize history of ideas typically read it as the story of the increasingly close approach of successive scientific constructions to the real world, a reading which I take to be simply incoherent. Those who emphasize institutional and social history, on the other hand, tend to view it as displaying the dominant role of interests in determining the conclusions reached by scientists, usually socio-economic interests but often interests of a broader sort as well. The role of reason and experiment in scientific development is for them minor at best, a conclusion I take to be as unsatisfactory as the first. We simply no longer have any useful notions of how science works or of what scientific progress is.

That is the gap that currently needs to be filled, and I think historians of science—some of them—will need to help in filling it. But, if they are to do so, then the traditional intellectual historical approach to science and the newer social institutional approach are going to have to interact more often and more fully in the future than they have in the past. To date, the cliché about ships passing in the night suits them well. There have been exceptions but in both directions: on the one hand, active communication between ships; on the other active exchange of gunfire.

The reasons for these difficulties in communication are clear. The two approaches to history of science are very different in their methods, in their sources, and in the research temperaments to which they appeal. In any case, the newer approach doubtless needed to establish a base of solid work before serious efforts to build bridges could begin. But that base is now established, as this Congress testifies. Fruitful and continuing interactions should by now be

possible, and I see a number of recent signs that they have in fact begun. Prospects are at long last encouraging. Even so, it has to be said that the movement to bring ideas and social history together is still in its infancy. Almost everything remains to be done. Let me therefore close these remarks with the hope that whoever next attempts to survey the development of history of science between International Congresses will be able to report relevant changes as dramatic as those I have recorded here.