Social Studies of Science: Society Crosses Disciplinary Lines

Heavy government involvement in science during World War II and after spurred interest in the politics and economics of science. That interest, organized into science policy studies programs, found a place in academia where the subfields of history, philosophy, and sociology of science were also enjoying a modest postwar growth. By and large, the practitioners of these specialties have maintained their differing perspectives and gone their separate ways. In recent years, however, as the boundary conditions have changed, conversation among them has picked up and so, even, has cooperation in research.

An organization formed very much with an eye to fostering such ecumenicism is the Society for Social Studies of Science (4S), which held its third annual meeting in Cambridge on the weekend of 14 to 16 October. The consensus at the meeting seemed to be that as a scholarly society, 4S is able to fly, but is not yet fully fledged. And the state of the society is seen as reflecting the evolving and still somewhat uncertain relations between the specialties.

The main impetus toward forming 4S came from the ranks of the sociologists of science, and the general impression is that Robert Merton of Columbia, the doyen of the discipline, was instrumental in getting the group going. Merton's role now appears to be rather like that of an honorary colonel of a regiment in the British army.

4S now has about 500 members, roughly 35 to 40 percent of them sociologists with historians of science forming the other large bloc of members—amounting to perhaps 20 percent. The backgrounds and interests of the balance of the members is pretty thoroughly mixed. The current president of the society is Warren Hagstrom of the sociology department of the University of Wisconsin, and a majority of the governing council are sociologists, but from the outset, care has been taken to see that other parishes have strong representatives on the council.

At the Cambridge meeting, a major topic of discussion was one of concern to scientists in general and of common interest to the specialties which make up the 4S group—control of scientific research. The site of the meeting lent something to the discussion since the City of Cambridge's advisory council on recombinant DNA research represents the best known example of intervention by local government into research policy-making.

The matter was introduced at the meeting by an invited paper titled "Limits of inquiry: Straws in the wind" by Gerald Holton, a Harvard physicist and one of the practicing scientists who has made lateral entry into the history of science field. The main purpose of the Holton paper was to suggest that increasing restraints on scientific research in recent years make the subject an eminently worthy one for research by those interested in social studies of science.

The End of an Era Suggested

In his paper, Holton observed that the present situation can be seen as something of a historical watershed. He noted that "it is now maintained by many that scientists and scholars have long had a bargain with society by which they have produced ideas and devices with few constraints, but that now this bargain is in danger of breaking down, or in need of revision. Understanding the extent and reasons for such changes should be of interest in its own right, and may also improve our ability to deal with practical problems some of us are now facing."

One of the commentators on Holton's paper, Dorothy Nelkin of Cornell, stressed that the key question now is "who is to establish limits on research?" Scientific activity has always been subjected to external controls of one sort of another—money, security, public health considerations, for example. What is different now and is upsetting to scientists is the effort to involve the public, said Nelkin.

For those who are concerned with science policy, and feel there is a fragility in the relationship between science and the public, Nelkin sees the main focus of concern shifting from the question of support of research to that of control.

One source of tension among 4S members is the issue of the extent to which their work should have direct policy applications along with policy implications. Those in science policy studies are, by definition, policy-oriented. The field was pioneered by working scientists with high-level government advisory experience, such as Harvey Brooks of Harvard and Franklin Long and Raymond Bowers of Cornell, or by those with direct experience in government administration, such as Don K. Price of Harvard. Political scientists are the dominant group in science policy studies sectors these days and they favor "outputs" relevant to decision-making.

Philosophers of science continue to be influenced by main currents of contemporary philosophy in their study of science, seeking to identify, for example, legitimate scientific questions. Philosophers of science were not much in evidence at the 4S meeting.

History of science is probably the liveliest of the disciplines represented and the most at odds with itself. The field was dominated for years by scholars holding "internalist" views, that is, by those who felt that the proper study of the historian of science was of the relationships of scientific ideas in isolation from social and political influences. This view has been effectively challenged by individual scholars who have transcended or transgressed the prevailing rules. One of these scholars is Derek de Solla Price of Yale who is identified with analyses based on the exponential growth of science and has been involved in science policy studies nationally and internationally. Price was also a prime mover in the establishment of 4S.

Both historians and sociologists of science have been heavily engaged in efforts to come to grips with Thomas Kuhn's work on scientific change. His book, *The Structure of Scientific Revolutions*, continues to make intellectual waves not only in his own field of history of science, but in other disciplines as well (*Science*, 8 July). Kuhn's insistence that scientific ideas might have to be set in the sociology of the scientific community to be properly understood has had a side effect of forcing reappraisal of the methodological differences among disciplines.

Historians of science have traditionally resisted quantitative techniques. Increasingly, historians see merit in statistical methods, such as content analysis, that are used by sociologists, and this has brought the two groups closer together.

Sociology of science found a firm foothold in the universities somewhat later than the other disciplines, and is undergoing its growth period—in numbers of graduate students and of publications—somewhat later. It also seems to have avoided the crunch in the academic marketplace which has hit the other disciplines.

Scholars who belong to the 4S group and come to its meeting would be expected to be friendly to an interdisciplinary approach to social studies of science. A few of the papers at the meeting, however, seemed to reflect more parochial preferences. And as one 4S member lamented, "Some of the younger people in the field are hung up on methodologies, they're losing perspective, forgetting that science is a social and cultural activity."

Nevertheless, the meeting gave evidence of a growing willingness to overlook differing disciplinary ideologies and assumptions. There seems to be a convergence which, in the language of the trade, is both cognitive and methodological.

Twenty-five years ago, Robert Merton in a foreward to Bernard Barber's book, *Science and the Social Order*, made a prediction that is inevitably quoted at events like the 4S meeting. Merton wrote in effect that the social studies of science would develop only when science itself came to be widely regarded as a social problem. The 4S group sees that prophecy being fulfilled, and the job of 4S to help the process along.—JOHN WALSH

Scandal in the Heavens: Renowned Astronomer Accused of Fraud

Charges of faking data to support his theories have been made against a famous astronomer whose magnum opus is known familiarly as "The Greatest."

The astronomer cannot personally answer the charges, having died not quite 2000 years ago, but at least one historian of science is prepared to do battle on his behalf.

The astronomer is Claudius Ptolemy, whose synthesis of Greek astronomical ideas was taken as the last word on the subject from the time of its composition, around A.D. 150, until the age of Copernicus some 1400 years later. Ptolemy's name became synonymous with the geocentric theory, according to which the earth rests at the center of the universe with the sun, planets, and celestial spheres rotating around it.

Ptolemy's accuser is Robert R. Newton, a member of the Applied Physics Laboratory at Johns Hopkins University. Newton considers that Ptolemy systematically invented or doctored earlier astronomers' data in order to support his own theories. "Ptolemy," he concludes, "is not the greatest astronomer of antiquity, but he is something still more unusual: He is the most successful fraud in the history of science."

Newton's charges are grave and his evidence erudite and imposing. Having checked through all the sums in the *Almagest*, he has documented his case for prosecution in various articles and a recently published book entitled *The Crime of Claudius Ptolemy*.*

But the accused has found a champion in Owen Gingerich, an astronomer and historian of science at Harvard. Gingerich agrees that Ptolemy's book, *Almagest*, contains "some remarkably fishy numbers," but he does not consider fraud to be the explanation. "When Newton and Einstein are generally considered frauds, I shall have to include Ptolemy also. Meanwhile I prefer to think of him as the greatest astronomer of antiquity," Gingerich concludes in a recent paper.

Newton's path intersected Ptolemy's when his work on satellite dynamics led him into the question of secular changes in the motions of earth and moon and from there to data collected by ancient astronomers. He noticed that Ptolemy's data disagreed both with those of other ancient astronomers and with the backward extrapolation of modern data.

Claudius Ptolemy

Systematic errors in Ptolemy's observations have been noticed before but have generally been attributed to innocent causes. For example it has been shown by J. P. Britton that if for some reason Ptolemy's watch, as it were, had been permanently half an hour slow, so that he made his observations at 12:30 p.m. when he thought it was noon, that would account for certain regular discrepancies in his data.

Discrepancies in Ptolemy's Data

But Newton sought and found another kind of explanation for Ptolemy's errors. Many of the *Almagest*'s data can be derived exactly by working out what the answer should be from Ptolemy's theory. A striking example is that Ptolemy says he observed an autumn equinox at 1400 hours on 25 September A.D. 132. This is strange because back calculation from modern tables shows that an observer at Alexandria in Egypt, Ptolemy's base of operations, should have seen the equinox at 9.9 hours on 24 September, more than a day earlier.

The discrepancy is doubly strange because Ptolemy comments that this particular observation was "one that he measured with the greatest care." Newton says he was puzzled by this emphasis, which reminded him of the behavior of students who work out the right result of a laboratory exercise from theory and insistently claim the answer as their own observation.

In this case, Ptolemy used his equinox observation to show how accurately an earlier astronomer, Hipparchus, had measured the length of the year. Hip-

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