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SCIENCE & TECHNOLOGY STUDIES

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About the Authors

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Instrumentation

Susan E. Cozzens

Among the major topics in academic research policy over the last ten years in the United States has been instrumentation: are we investing enough in it? First Smith and Karlesky in their pathbreaking overview, *The State of Academic Science*, listed out of date instrumentation as among the problems of U.S. universities under steady state funding.¹ A series of specialized studies followed.² The general tone of their conclusions was summed up by the Association of American Universities:

The quality of research instrumentation in major university laboratories has seriously eroded. Not all, but many researchers in the nation's best-funded universities are struggling to work effectively with obsolete tools.³

Attention has also been directed to the issue in other nations.⁴

During this period of ferment and discussion, U.S. policy analysts who were asked to prepare reports or briefings on instrumentation faced a dilemma: in the early 1980s, there was neither systematic descriptive data on the stock of research instrumentation used in U.S. research universities, nor a body of knowledge about how instrumentation functioned in the research process. In the intervening years, the first deficiency has been to a large extent overcome. Data is now gathered annually on equipment expenditures at universities and colleges.⁵ A national baseline survey of the stock of instruments in many fields of science has been completed, and results from the first wave of followup are about to be announced.⁶

So now we have data: we know how much instrumentation is housed in universities and what kinds of instruments are being used. But we are still missing understanding: what is the role of instrumentation in the process of research? Where do new instruments come from? How does their appearance change the work worlds of scientists? How do instruments contribute to the interaction between research process and industrial technology? In short, what are the implications, not just for research but for innovation more generally, of different kinds of investment in instrumentation?

In this case, the pressing need for policy information happens to coincide with an upswelling of interest in instrumentation within science and technology studies. For example, Derek Price's last paper, published posthu-

mously, was a progress report on his thinking on this topic, with a clear policy recommendation: if you want to stimulate industrial innovation, make sure that your basic researchers have the resources to tinker with their instruments.⁷ The attention to laboratory practice and the process of persuasion has also directed some attention in this direction. Who can forget Bruno Latour's slide show of inscription devices to the 4S meeting in Philadelphia in 1982, the abstract for which read:

The slide is not a simple illustration of a theme that could be developed by way of words, it is an essential feature of the demonstration itself. It *shows* scientific activity as ways of showing, displaying and writing.⁸

And finally among historians, there has also been a growing attention to instruments, as the technologies of research and as factors in the cognitive and organizational development of fields of science.⁹

There is still, however, only a handful of researchers devoting special attention to the problems of instrumentation, and a long list of topics they need to address. First, where do new instruments come from? From the economist Eric von Hippel, we learn that the instrument industry gets them from bench scientists.¹⁰ Under what circumstances are scientists most likely to tinker and improve their means of research? Under what circumstances do they adopt and adapt, as radio astronomy did?¹¹ Does an individual, a laboratory, or even a whole field commonly take on the role of instrument-innovator? (It is said that organic chemistry has performed this function for chemistry as a whole in the last few decades.) If so, why? What rewards are there to reap?

Second, under what circumstances and in what pattern does the use of a particular instrument diffuse, with what consequences for social and cognitive development? Some changes in instrumentation are incremental; others are earth-shattering. Some researchers can afford any new instrument they want; others will never afford more than a few. How are the innovations and the resources distributed among the institutions where researchers work and among the participants in research areas? What role do instruments play in redistributing them?

Finally—and this is the most difficult part of the agenda, in my view—in what ways do instruments serve

as an interface between research process and industrial innovation? Is instrument invention a significant factor in industrial innovation, or is it negligible? Does the overall level of university/industry contact in a field affect the rate of instrument innovation? Vice versa? Or are the relevant factors at the individual level? Alternatively, is the process random, not accessible to intervention or encouragement?

With this issue, *Science & Technology Studies* opens its pages to a series of papers on instrumentation. The series begins with Merriley Borell's paper on the impact of the kymograph on physiological research in the late nineteenth century, a paradigm case for consideration of revolutionary instrument innovation. Other articles are scheduled to appear in future issues, and the series can be extended if more work is contributed. We welcome your submissions—of research reports, short or long, or of commentary—in order to stimulate the development of an agenda for research on instrumentation, to encourage exchange among researchers from different fields, and to foster consideration of the policy implications of this emerging research area.

Notes

1. Bruce L. R. Smith and Joseph J. Karlesky, *The State of Academic Science* (New York: Change Magazine Press, 1977).
2. Examples are Frank J. Atelsek and Irene L. Gomberg, "Shared Use of Scientific Equipment at Colleges and Universities, Fall 1978," Higher Education Panel Report Number 44, American Council on Education, November 1979; Irene L. Gomberg and Frank J. Atelsek, "Expenditures for Scientific Research Equipment at Ph.D.-Granting Institutions, FY 1978," Higher Education Panel Report Number 47, American Council on Education, March 1980; "The Scientific Instrumentation Needs of

Research Universities," report to the National Science Foundation by the Association of American Universities, June 1980; *Engineering Education Problems: The Laboratory Equipment Factor* (Washington, DC: National Society of Professional Engineers, September 1982); "Report of the Panel on Large Scale Computing in Science and Engineering," National Science Foundation, December 26, 1982; "Instrumentation Needs of Academic Departments of Chemistry: a survey study," American Chemical Society, April 1984; *Financing and Managing University Research Equipment* (Washington, DC: Association of American Universities, 1985).

3. Association of American Universities, "Scientific Instrumentation Needs," executive summary.

4. See for example, "Scientific Instruments: Lessons Derived from Some Country Surveys," Organization for Economic Cooperation and Development, Paris, 5 June 1985, DSTI/SPR/85.27.

5. Available from the Division of Science Resources Studies, National Science Foundation, Washington, DC 20550 USA.

6. The report on the baseline survey is "Academic Research Equipment in Selected Science/Engineering Fields, 1982-83," National Science Foundation, August 1985. New data available from the Division of Science Resources Studies (see note 5).

7. Derek deS. Price, "The science/technology relationship, the craft of experimental science, and policy for the improvement of high technology innovation," *Research Policy* 13 (1984): 3-20.

8. Bruno Latour, "On Inscription Devices," *4S Newsletter* vol. 7, number 3 (1982): 29.

9. For example, see the papers presented in a session on "The Technology of Science," cosponsored by the Society for the History of Technology and History of Science Society at their annual meetings in Pittsburgh, 24 October 1986. Papers were by Peter Galison ("The Material Culture of Modern Physics"), Timothy Lenoir ("Instruments, Nerves, and Muscles in the Late Nineteenth Century"), Norton Wise ("The Role of the Telegraph in British Electromagnetic Theory"), and Michael Mahoney ("Reading a Machine").

10. Eric von Hippel, "The Dominant Role of Users in the Scientific Instrument Innovation Process," *Research Policy* 5 (1976): 212-239.

11. David O. Edge and Michael Mulkay, *Astronomy Transformed* (New York: John Wiley & Sons, 1976).

Instrumentation and the Rise of Modern Physiology

Merriley Borell
Harvard University

The rise of experimental physiology has received much attention in the last thirty years from both historians and sociologists of science. Close analysis of the rapid institutionalization and professionalization of this field, as it occurred especially in Germany, has provided an important case study of the social, economic and political factors that enhance the development of autonomous scientific fields.¹ The spread of experimental physiology and its supporting institutions from Continental Europe to Great Britain and North America has also been discussed recently in much detail by historians.² However, despite intensive scholarly activity, few have been able to link persuasively the social and intellectual features of this rising discipline.³

This paper suggests that laboratory instrumentation can provide important insights into the changing intellectual and social structure of science in the late nineteenth and early twentieth centuries. Instruments have afforded not only new conceptual foci for investigators but also the material culture which has shaped experimental design and laboratory practice as professionalization and institutionalization have occurred. Recording instruments, particularly, have shaped the way scientific data are presented, discussed and accepted. The role of this instrumentation in the intellectual and social changes associated with the rise of experimental physiology and other autonomous sciences has yet to be explored systematically. This paper suggests that close study of research instruments introduced by physiologists in the 1840s and 1850s can provide valuable data with which to discuss social and intellectual linkages in all their richness and depth.

Specifically, the incorporation of the recording drum or "kymograph" into physiology in the late 1840s led immediately to the study and analysis of a wide range of physi-

ological events that had previously been inaccessible to researchers.⁴ Emulating the experimental approach of physical scientists, physiologists after 1850 increasingly sought to measure as well as describe physiological processes. This approach was readily assimilated and popularized through the use of recording apparatus, relatively simple instrumentation that made rapid and thus invisible physiological processes visible. Following the path set by the "1847 group" in Germany, experimental physiologists endeavored to formulate general laws and to predict physiological phenomena.⁵ In this manner, they achieved a vast extension of the range of events capable of being studied within physiology. Registration instruments effectively opened new physiological events and processes to analysis, particularly those which occurred very rapidly or very slowly. Such instruments both extended the senses of observers allowing them to analyze previously undetected phenomena, and at the same time ostensibly removed the observer from intervening in the measurement of physiological events.⁶ As a consequence, researchers could and did claim a new level of objectivity for their science—an important goal in the life sciences at this historic time.

The first physiological events to be recorded were related to blood pressure. Self-registration of changing blood pressure by translation of motion from a mercury manometer to a recording stylus resulted in the autographing of transient events that previously could not be seen or measured. The obvious utility of such analysis led to the rapid incorporation of recording apparatus into research protocols. Investigators modified the recording drum and its accessory apparatus to measure a wide variety of events. They and their successors also adopted this new method of physiological analysis, self-registration, to meet the practical needs of the clinic. The measurement of each heretofore little understood physiological event was explicitly linked to the expectation that such data would prove to be of diagnostic value in detecting and monitoring disease.⁷

Physiology subsequently gained new social authority both in science and in medicine through the application of recording instrumentation. Physiologists could claim objective, quantitative analysis of a given event. They also argued that the data generated by registration instruments could lead to the diagnosis, monitoring and more effective treatment of disease. While such claims

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My appreciation of these issues has been especially enhanced by the comments, criticisms and suggestions of Adele Clarke, Deborah Coon, H. Hughes Evans, Gail Hornstein, Peggy Kidwell, Robert R. Pool, Jr. and John Harley Warner. This research was supported by a grant from the National Science Foundation (RII-8503650) through its Visiting Professorships for Women program.

were certainly premature in the 1850s and 1860s, the new methodology of physiologists seemed at that time to be applicable both at the laboratory bench and at the bedside. The historian can follow the adaptation of these instruments from their invasive laboratory forms to their non-invasive clinical counterparts and watch in the process the transformation of experimental physiology from a specialized research activity conducted in small ill-equipped rooms to an important teaching and research activity supported by a vast array of apparatus and instruments in medical schools. The claims of both objectivity and utility were fully exploited by advocates of the experimental approach although both of these claims were challenged throughout this period.

By the turn of the twentieth century, kymographic recording apparatus had become the central tool of physiological analysis and the symbol of a new style of pedagogy especially in American medical schools.⁸ This style, which can be observed in texts and handbooks produced between 1890 and 1910, was marked by an ever-increasing emphasis on hands-on laboratory experience for introductory students. Students were explicitly trained in an analytical method, as well as in a subject. Registration apparatus thus transformed physiology both intellectually and socially, helping physiologists to secure their place within medical academia. It is the purpose of this paper to explore this transformation, suggesting points of interaction and intersection between cognitive and institutional factors that might in the future be fruitfully studied by sociologists and historians interested in the growth of science.

Introduction of New Instruments

Physiologists traditionally mark the birth of modern physiology with the work of Carl Ludwig (1816-1895), especially his introduction in the winter of 1846-47 of the "kymographion" or revolving drum recorder into physiology.⁹ Ludwig's technical innovations in the measurement of blood pressure were to have far-reaching consequences both for the content of physiological data and how physiologists viewed that data. Important conceptual and cognitive changes appeared as a result of the widespread use of registration apparatus in the period 1850-1870, that is, in the period historians usually associate with the rise and institutionalization of Continental physiology.

Ludwig initially wanted to examine the relationship between blood pressure and respiratory movements.¹⁰ This was difficult to do because blood pressure, as read from a manometer attached by catheter to a blood vessel of an experimental animal, was not stable. The level of mercury in the manometer oscillated and could not be read easily; it could only be approximated.¹¹ By placing a float on top of the mercury and attaching a stylus to the float, Ludwig could use a pen to register on the surface of a revolving drum the changing level of the mercury. The height of the excursion of the stylus bearing the pen

indicated maximum pressure. The curves produced in this manner could be measured and compared with other curves, even those made by other investigators, minimizing individual differences. They could also be preserved indefinitely and correlated with other records produced under different experimental conditions. In other words, the experimental set-up could readily be manipulated to study changes in blood pressure under different physiological conditions. Ludwig was specifically interested in determining the effects of respiratory rates and pressures on arterial blood pressure.

Ludwig sought to obtain precise readings from his graphs, yet he recognized the scientific value of the curves themselves, especially when more than one event was recorded. Tübingen physiologist Karl Vierordt (1818-1884) exploited this pictorial presentation further. In order to provide physicians with a visual record of clinical signs that up until then could only be read by touch or palpation, he adapted the kymograph to monitor the external pulse rather than the blood pressure.¹² His instrument, which he called the "sphygmograph" or "pulse writer" was bulky, cumbersome, and poorly adapted for clinical needs. However, this difficulty was overcome by modifications made by a Parisian medical student who took up the problem in the late 1850s; Étienne Jules Marey (1830-1904) adapted Vierordt's device to make it more convenient, reliable and clinically useful.¹³ Marey subsequently embarked upon a scientific career devoted to the extension of related research techniques.¹⁴ He referred to these collectively as the "graphic method." In the 1860s and 1870s, he designed numerous registration instruments that produced graphic records of other physiological and pathological processes. These included: the cardiograph for registering movements of the heart (Marey created both internal and external probes for this purpose), the polygraph (to record various physiological processes), the pneumograph (for respiratory movements), the myograph (for neuromuscular events), the thermograph (to measure temperature changes), the electrometer (to monitor electrical changes), and the plethysmograph (to record volume change in limbs or organs). During his career of nearly fifty years, he popularized the use of mechanical, electrical, and photographic recording instruments in physiology, medicine, and biology.¹⁵

Marey's utilization of registration apparatus coincided with the extension of recording techniques into other physiological problem areas. In 1850, Ludwig's colleague and friend Hermann von Helmholtz (1821-1894) adapted graphic recording apparatus to measure the speed of nervous conduction. Helmholtz used the graphic record produced by contraction of an isolated muscle as an indication of response to a nervous stimulus, the time on the graph between stimulus and response being the period of time required for conduction.¹⁶ Other physiologists and psychologists rapidly adapted Helmholtz's myograph to study nervous conduction,

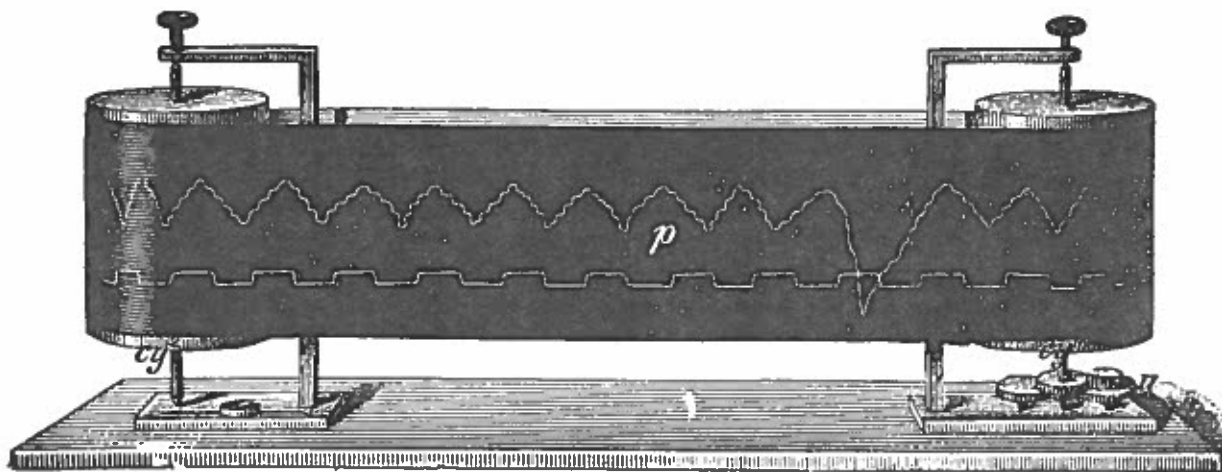


Figure 1. Continuous blood-pressure tracing on a long-paper kymograph [From Stirling, *Outlines of Practical Physiology*, 1902; reproduced courtesy of Charles Griffin & Co., Ltd.]

muscle contraction, muscular work, and reaction time. Marey, too, made many modifications of this apparatus in the 1860s and 1870s.

Thus, the simple act of recording rapidly transformed physiology from a primarily descriptive, vivisectional and anatomically-oriented activity to a quantitative experimental science. Concurring with the goals of physical scientists, physiologists increasingly and explicitly concerned themselves with the determination of biological laws and with the analysis of cause and effect in physiology. Yet, their initial preoccupation with quantification and precise measurement was gradually transformed. By the continued use of these instruments, an expressed desire to understand process and change within complex physiological systems emerged. As investigators recorded and correlated blood pressure, heart rate, nervous and muscular responses, they adapted the instrumentation to monitor these processes simultaneously. An intricate web of discrete events began to be studied using not only one or two but a whole series of levers and styluses. Long papers, held taut between two drums or a drum and roller, allowed for recording multiple phenomena over extended periods of time [Figure 1].¹⁷

Initially, use of the kymograph or "wave writer" opened up tiny periods of time and fleeting, transient events to measurement and analysis. However, by adjusting the speed of revolution of the drums, instruments could also be adapted to study very slight change over relatively long periods of time, as, for example, that evident in the process of plant growth [Figure 2].¹⁸ This extended the perceptions and senses of observers even further into previously unobservable phenomena. The study of each new process required only slight modification of the transmitting and recording apparatus. The kymograph drum remained unchanged in principle, although Ludwig's drive by a falling weight was gradually replaced by clockwork and then by motor.¹⁹ In the late nineteenth and early twentieth centuries, the possibility for electrical and eventually electronic translation of mechanical events was also realized. Twentieth century recording devices are but electronic transducers playing the same functional roles as their nineteenth-century mechanical ancestors.²⁰

This registration technology derived from eighteenth- and nineteenth-century physics and meteorology. In 1878 in *La méthode graphique dans les sciences*

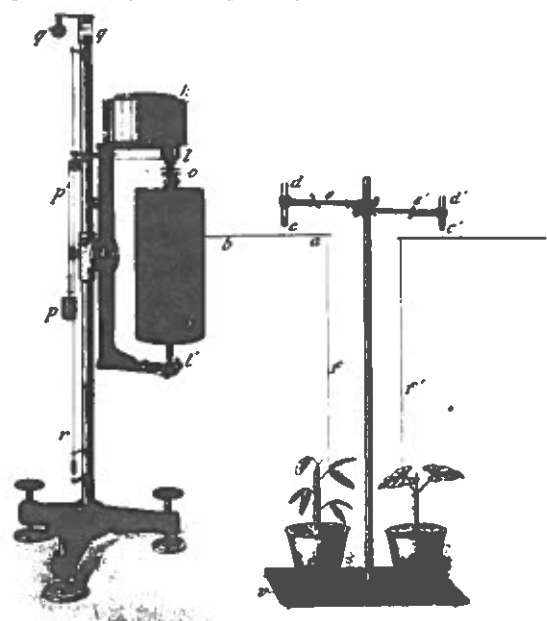
experimentales, Marey described earlier instruments, including Morin and Poncelet's apparatus for inscribing the positions of a falling body and numerous meteorological instruments used in thermometry, barometry, hygrometry, and the measurement of rainfall and wind. Hoff and Geddes have attempted to define the precise intellectual connections between physiologists and physical scientists at the mid-nineteenth century, yet these cannot be reconstructed entirely, because it is clear that the basic elements of the kymograph (the recording drum or its analogue the recording disc), were in use and available by the 1840s.²¹ A set of techniques was borrowed and a category of instruments adapted to meet the specific needs of physiologists. Investigators who sought to measure transient biological events with precision made use of this technology. What was not immediately evident in this process of adaptation of instrumentation from the physical sciences to the biological sciences was the tremendous intellectual and social transformations in laboratory organization and professional practice that would be set in motion by the introduction into physiology of these relatively simple instruments.

Cognitive Effects

The cognitive effects of the introduction of recording apparatus into physiology, and subsequently medicine and biology, were manifold and profound. Many of them were unanticipated. Initially, self-registration apparatus simply extended the senses of observers, making the invisible visible and allowing investigators to measure phenomena precisely. The motivations underlying these innovations in technique were, first, quantification *and*, second, elimination of the subjectivity of the observer from that process. Physiological investigators were explicitly following the experimental method so well developed by physicists and chemists. They wanted to discover the laws of a determined world. Visualization allowed precise measurement and eventual mathematical analysis of rapid, complex, interrelated events.

Registration instruments were remarkably adaptable. They were, in principle, infinitely expandable to the needs of researchers. First, the recording paper was lengthened; then it was replaced by a roll.²² Drums were enlarged, then stacked one atop another.²³ Pneumatic transmission (of changing pressures) and eventually electrical and electronic transducers replaced direct mechanical connection between the event and the recording stylus.²⁴ Nonetheless, the principle remained the same: the event self-recorded on a revolving drum, the period of revolution generating the abscissa, plotting the event or position of the stylus against time. Graphic registration, graphic recording, or graphic inscription as Marey later called it, allowed the physiological event to autograph itself. The permanent record was made either by using ink on vellum paper (Ludwig), ink on paper, or most frequently by scratching a line on sooted paper. The curves were generated, measured, and stored for re-

Aparato registrador para movimientos geotrópicos, con accesorios.
Nr. 2378. Registrierapparat nach Pfeffer, speziell konstruiert zur Aufzeichnung der Blattorgane und Registrierung der Zuwachsbewegungen.



Nr. 2375 u. 2396, ca. 1/2 nat. Größe

Durch ein starkes Uhrwerk K (mit Zifferblatt und Zeiger versehen) werden der Kuppeln mit einem der drei aus dem Uhrwerk hervorragenden Trichzapfen 1, 2 und 3 Windigkeiten gegeben. Uhrwerk und Trommel sind mittels der Führung n sowohl als auch im Gelenk m horizontal einstellbar. Zur genaueren Verikalstellung sind Stell

Figure 2. Recording apparatus modified for studying responses of sensitive plants (left) and plant growth (right) [E. Zimmermann catalogue, Leipzig, 1928; Smithsonian Institution Photo No. 86-10018].

measurement. The event was thus quantified.

Kymographic recording techniques effectively expanded or condensed perceived time frames, depending on the speed of rotation of the drum. All recorded events were timed by the drum's forward motion and by a time marker or chronograph, often a tuning fork in the early investigations. Rapid movement of the drum spread out the event and made minute fluctuations visible; slow movement of the drum condensed the event accentuating rhythm, rate or gradual change. Rapid events and small time intervals were thus brought to the visible range in ways analogous to microscopic enlargement. Slow events and longer time intervals were condensed in ways analogous to the telescope bringing the distant into view. In all cases, the event recorded itself, eliminating the "subjective" intervention of the observer, the "personal equation" that so troubled nineteenth-century astronomers, physicists, physiologists, and psychologists.²⁵

In this process the graph itself effectively became the phenomenon to be analyzed.²⁶ The tracing could be measured and evaluated at leisure, long after the original event was recorded. Marey emphasized, following

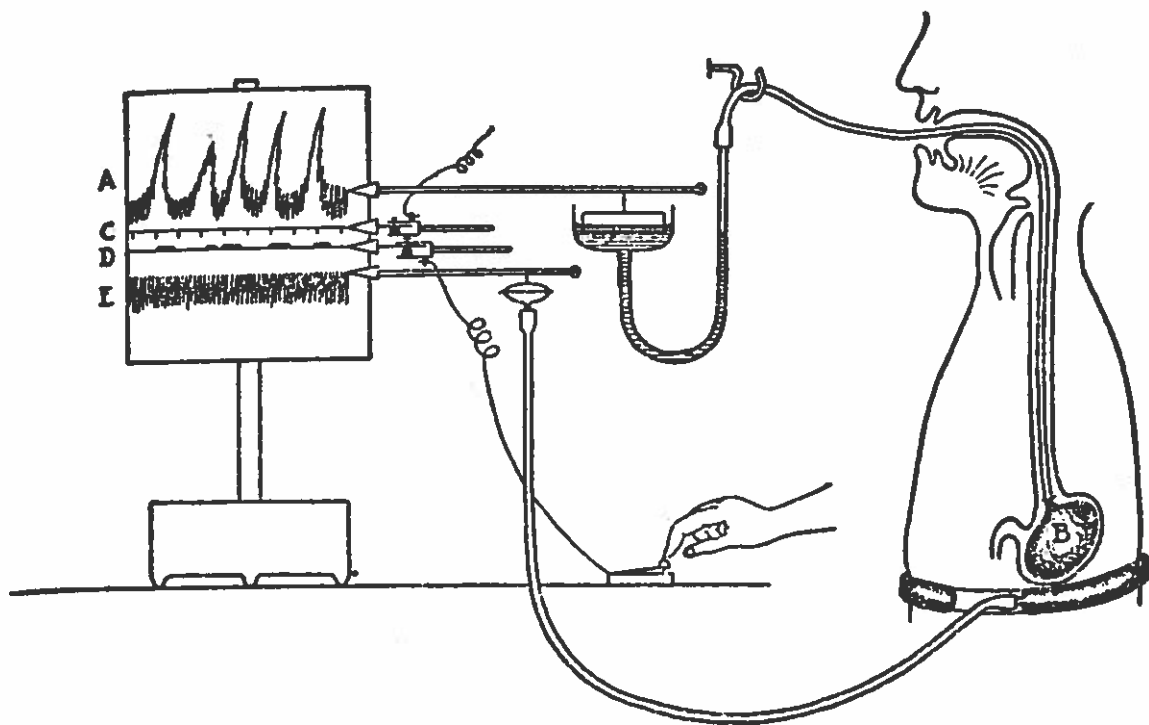


Figure 3. Simple kymograph with various transducers derived from modifications of nineteenth-century instrumentation. Tracing shows simultaneous recording of (A) gastric contractions, (C) time record in minutes, (D) subjective experience of hunger pangs, and (E) pneumograph recording respiratory movements [From W.B. Cannon, *The Wisdom of the Body*, 1932; reproduced courtesy of W. W. Norton & Company, Inc.].

Claude Bernard, that all life was movement and that the graphic method captured this movement.²⁷ Study of the trajectories of dynamic processes gradually replaced the mere measurement of static events as the primary focus of physiological investigation. The graph both portrayed and symbolized the dynamic process under study. By the end of the nineteenth century, physiologists were studying multiple variables and uncovering complex control mechanisms which operate within the body. The significance of this trend, the study of process and the regulation of that process, was underscored by the introduction in 1926 of the concept of biological homeostasis [Figure 3].²⁸

An interesting apparently unanticipated cognitive change accompanied the use of these instruments. Investigators increasingly began to recognize the value of the curve itself. That is, they increasingly displayed their quantitative data (some of it initially derived from kymograph tracings) in graphs rather than in numerical, tabular form.²⁹ This graphic presentation of data became a significant and conspicuous element of scientific communication in the 1860s during precisely those years in which graphic recording became a routine method of

physiological analysis.³⁰ Indeed, early physiological and medical graphs were often published in white on a black background rather than black on white, apparently imitating the kymographic record inscribed on sooted paper.³¹

Initially, physiologists used the graphic record to obtain precise numbers. They presented these numbers in tabular form in their publications.³² Gradually, physicians, physiologists, and psychologists began to construct graphs from those numbers, to present even their numerical data graphically. The physiological recording instruments used by nineteenth-century investigators made hidden processes visible and generated numerical data about those processes. Such data was visualized further by systematic mathematical manipulation and subsequent graphic presentation.³³

The graphic records from instruments subtly encouraged the graphic presentation of all numerical data, even data not originally gathered in graphic form. Routine statistical analysis, graphic recording, and graphic presentation became significant features of the scientific method in the 1860s and ought probably to be viewed as cognitively interrelated and mutually supportive de-

velopments.³⁴ Out of this context emerged the now universally-applied techniques that Tufte has recently called the "visual display of quantitative information."³⁵ Recording apparatus literally created an awareness of the value of graphs in physiology and medicine and accelerated scientific application of graphic methods, even though graphic presentation of data itself was not a new idea.

Especially interesting in this context is the development by German clinicians of temperature charting in the 1850s and 1860s. Ludwig Traube (1818-1876) utilized the kymograph and extended the application of graphic recording methods to pharmacology early in the 1850s.³⁶ During these years, he also suggested to Carl Wunderlich (1815-1877) the value of systematically noting human temperature change over time. Wunderlich's landmark book, *Das Verhalten der Eigenwärme in Krankheiten* (The Behavior of Body Heat in Diseases), appeared in 1868, a second edition in 1870. Wunderlich emphasized:

Whatever the nature of the thermometric observations, if they are to be of any use at all, it is essential that the *results obtained should be continuously recorded*. This can best be done and the course of the disease rendered most evident, by indicating it *on a chart, or ruled map, as a continuous curved line* . . . It is convenient to note the frequency of the pulse, and the number of the respirations in a similar manner, but in different colors . . . In this way the entire course of the disease, with all its fluctuations, complications, tendencies, and changes can be seen at a single glance.³⁷

Such numerical records presented visually as temperature curves or "charts" proved diagnostically useful in the study of diverse fevers, allowing physicians to distinguish between them and monitor their course. Such visual presentation of data secured the place of thermometry in medical practice from the early 1870s. Graphic recording techniques also affected thermometry directly. The thermometer which measured temperatures, that could themselves be displayed graphically, was supplemented by the thermograph which automatically inscribed its own curve.³⁸

These interrelated conceptual themes are evident in the organization of Marey's *La méthode graphique dans les sciences expérimentales* (The Graphic Method in the Experimental Sciences) published in 1878. By the 1870s many of the most important accessories to the recording apparatus had been invented or were being perfected. A network of instrument manufacturers was developing. Registration techniques had become an integral part of physiological research as evidenced by the plans of new laboratories. Moreover, graphic presentation of numerical data was an important analytical procedure that had already shown its value in thermometry and was being extended into other problem areas [Figure 4].³⁹ Marey was then advancing graphic techniques through the use of yet another recording method—photography. He later

developed chrono-photography (rapid photography) to record the successive positions of animals during locomotion.⁴⁰ For each of these variations on the theme of the graphic method, recording instrumentation provided the drive behind fundamental perceptual, conceptual, and cognitive change in physiology.

Recording apparatus encouraged physiologists to develop analytical techniques that are now firmly identified with the methods of modern science. It also provided the mechanism for translating the expressed experimental ethos of nineteenth-century investigators, i.e., the desire to discover the laws of nature, into common, routine, uniform laboratory experience and practice. Recording instruments not only symbolized a new experimentally-based science to physiologists, they also provided the material basis and heraldry both for transmission of that science and for its institutionalization and standardization in a wide variety of social and cultural contexts. The manufacture, distribution and utilization of these instruments tells us much about the perceived goals of experimental scientists at the end of the nineteenth century.

Social Effects

In following the spread of experimental physiology from Continental Europe to Great Britain, North America and Russia, it is useful to ask how new experimental techniques were transplanted from one context to another. The extensive work on the training of American students abroad in the nineteenth century suggests that in physiology much of this transmission was accomplished between 1860 and 1890 through the training of foreign students in German (and sometimes French or Italian) laboratories.⁴¹ Ludwig's laboratory at Leipzig especially was a focus for the development of skills in experimental design, experimental technique, and experimental analysis. Training by apprenticeship at the laboratory bench has been the primary mode of transmission of research skills. However, this process was changed considerably by the advent of the new nineteenth-century instrumentation of physiology. American students returned home not only with newly acquired research skills, but also with imported German instruments. Laboratories were built to house these important new tools of experimental science.⁴²

The physiological laboratory thus became the site for use and storage of instruments, as well as the site for transmission of knowledge made possible by them. From the 1860s in the German states and the 1870s elsewhere, the equipping of physiological laboratories became a major professional activity.⁴³ Initially, the laboratory was used for the professor's personal research and for the training of a few advanced students or colleagues from other fields. However, by the 1890s, the events which occurred within the laboratory began to acquire increasing pedagogical importance, particularly in the United States. Occasional experimental demonstrations were replaced first by routine demonstrations and finally by

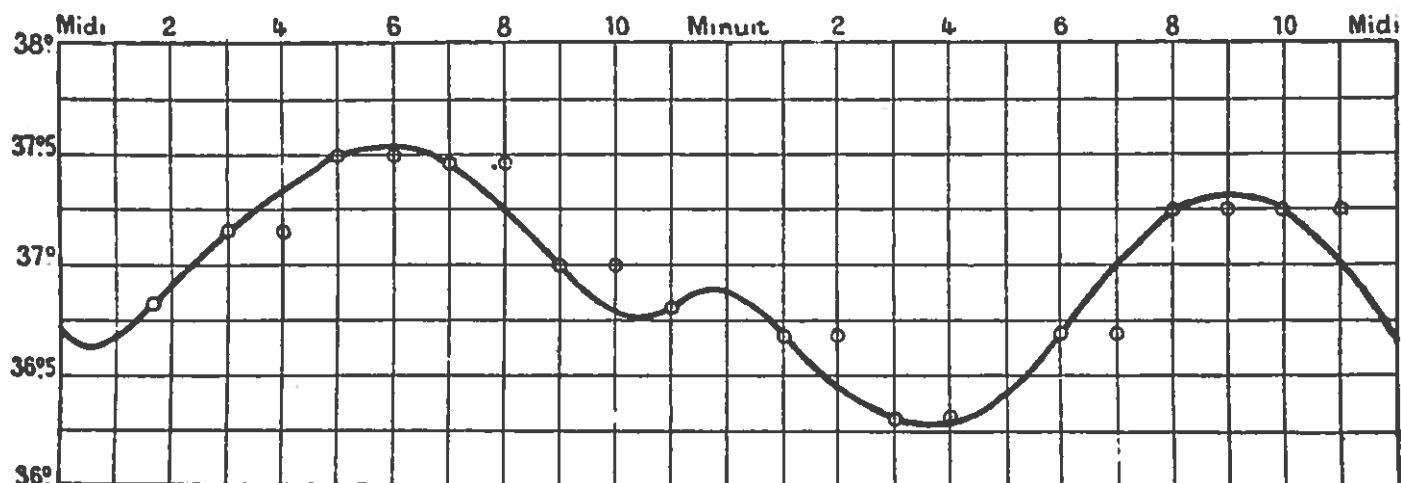


Figure 4. Graphic presentation of pulse data. Prompt's graphing (1867) of Boerensprung's data (1840) [From Marey, *La méthode graphique*, 1878; reproduced courtesy of Masson, Editeur s.a.].

hands-on experiments for all introductory students. The method, the technique, as opposed to the facts, of experimental physiology became increasingly important. All introductory students, not just the advanced students, learned to operate the kymograph and myograph, to record and measure physiological events, to manipulate and modify apparatus. In other words, all students learned to ask questions of nature rather than rely on the authority of didactic lectures. Scientific knowledge was to be gained directly by experience in the laboratory.⁴⁴ Students learned to manipulate as well as to observe and describe.

Such a major change in the process of physiological education required that hitherto rare and expensive apparatus be provided for all students. Recording instruments and related apparatus had initially been made by the researcher himself or a skilled mechanic in a university laboratory or nearby industrial shop. As the demand increased, instruments were made and sold by an increasing number of scientific instrument companies.⁴⁵ At first made one by one, the availability of these instruments was increased markedly by the introduction of quantity production and the substitution of cheap and durable materials like aluminum (in the case of kymographs) for nineteenth-century brass.⁴⁶ The Harvard Apparatus Company, which manufactured physiological apparatus in Boston from 1900, arose from the desire to provide high quality, reliable, inexpensive teaching apparatus to physiology students at the Harvard Medical

School. Within five years, it supplied kymographs and myographic equipment world-wide. In America, it transformed kymographic recording from an activity reserved for research investigators to a skill to be learned in introductory courses by all medical students and aspiring life scientists even those at small colleges.⁴⁷

The transformation from research laboratory to teaching laboratory required a major investment of personnel and capital by each college or university. Hands-on laboratory experience became the main emphasis in program building especially in the United States. Recording instrumentation became the centerpiece of these efforts in physiology, but experimental analysis also transformed the methodology of biology and botany and this instrumentation spread into those domains. As a result, laboratory development provided jobs for succeeding generations of American researchers. Quantity production of inexpensive research apparatus allowed even smaller colleges to hire experimentalists to provide hands-on training to introductory students. In the process, chairs, laboratories, and departments were created.

In physiology, the kymograph was the focal instrument of this institutional transformation. It represented both the major tool of experimental physiology and the intellectual power of physiology to transform medicine into a predictive law-bound science of health and disease. Work organization within physiology changed as physiologists built research groups and schools. They

constructed their science and their newly-acquired social authority largely on solution of the kinds of problems amenable to analysis by graphic registration.

Conclusions

By the end of the nineteenth century, physiology claimed a role as the foundational subject of scientific medicine and as the epitome of the experimental method in the life sciences. The campaign for creation of journals, societies, separate chairs, laboratories, and departments, so well studied by historians and sociologists, now needs to be correlated with the process of the introduction and application of the registration techniques and instrumentation set in motion by the introduction of the kymograph and other recording apparatus. Physiological journals reproduced kymograph tracings; societies specifically advocated the extension of the experimental and "practical" physiology symbolized by the kymograph; individual scientists pleaded for the creation of chairs, laboratories and departments of experimental physiology. Their European patrons, nineteenth-century governments, saw a role for physiology in the industrialization and modernization of the state and in the improvement of medical practice. American educators spoke of the need to train "for power," that is, to teach methods of scientific reasoning and analysis, rather than facts.⁴⁸ Instruments came to represent both the power and the substance of experimental physiology in each of these important discussions.

Continued analysis of these interrelated events will add important dimensions to our understanding of the role of instrumentation in the establishment of modern institutions of science. It will also clarify the intellectual origins of many of the analytical procedures now considered to be integral to the scientific method. Such analysis will connect more persuasively, through the addition both of texture and of fine detail, the intellectual content of science with its underlying and evolving social structure.⁴⁹

NOTES

1. For a review of this literature, see Steven Turner, Edward Kerwin, and David Woolwine, "Careers and Creativity in Nineteenth-Century Physiology: Zloczower 'Redux'," *Isis* 75 (1984): 523-529. Case studies of the development of specific physiological laboratories in German states will soon appear in articles by Arleen Tuchman ("From the Lecture to the Laboratory: The Institutionalization of Scientific Medicine at the University of Heidelberg") and Timothy Lenoir ("Science for the Clinic: Carl Ludwig and the Institutional Revolution in German Science") in William Coleman and F.L. Holmes, eds., volume tentatively titled *The Investigative Enterprise* (University of California Press, forthcoming). I am grateful to these authors for sharing their manuscripts with me. See also Arleen Tuchman, "Science, Medicine and the State: The Institutionalization of Scientific Medicine at the University of Heidelberg" (Ph.D. diss., University of Wisconsin-Madison, 1983).

2. Richard D. French, "Some Problems and Sources in the Foundations of Modern Physiology in Great Britain," *History of Science* 10

(1971): 28-55; Gerald L. Geison, "Social and Institutional Factors in the Stagnancy of English Physiology, 1840-1870," *Bulletin of the History of Medicine* 46 (1972): 30-58; idem, *Michael Foster and the Cambridge School of Physiology: The Scientific Enterprise in Late Victorian Society* (Princeton, N.J.: Princeton University Press, 1978); idem, "Divided We Stand: Physiologists and Clinicians in the American Context" in Morris J. Vogel and Charles E. Rosenberg, eds., *The Therapeutic Revolution: Essays in the Social History of American Medicine* (Philadelphia: University of Pennsylvania Press, 1979), pp. 67-90; and idem, ed., *Physiology in the American Context, 1850-1940* (Bethesda: American Physiological Society, 1987).

3. The standard history of physiology, Karl Rothschild's *Geschichte der Physiologie* (Springer Verlag, 1953) appeared in English translation in 1973 as *History of Physiology*, trans. and ed. by Guenter B. Risse (Huntington, N.Y.: Robert E. Krieger). On physiology's emergence as an independent experimental science, see J. Schiller, "Physiology's Struggle for Independence in the First Half of the Nineteenth Century," *History of Science* 7 (1968): 64-89. The content of these intellectual histories must yet be integrated with the new social and institutional histories cited above.

4. Merrile Borell, "The Kymograph and the Origins of the Graphic Method," *Electric Quarterly* 7 (Summer, 1985): 2-3.

5. See Paul F. Cranefield, "The Organic Physics of 1847 and the Biophysics of Today," *Journal of the History of Medicine and Allied Sciences* 12 (1957): 407-423; idem, "The Philosophical and Cultural Interests of the Biophysics Movement of 1847," *Journal of the History of Medicine* 21 (1966): 1-7; and idem, ed., *Two Great Scientists of the Nineteenth Century: Correspondence of Emil Du Bois-Reymond and Carl Ludwig*, trans. Sabine Lichter-Ayed (Baltimore: Johns Hopkins University Press, 1982).

6. Merrile Borell, "Extending the Senses: The Graphic Method," *Medical Heritage* 2 (1986): 114-121.

7. Stanley J. Reiser, *Medicine and the Rise of Technology* (Cambridge: Cambridge University Press, 1978): esp. pp. 91-121.

8. Merrile Borell, "Instruments and an Independent Physiology: The Harvard Physiological Laboratory, 1871-1906," in Geison, ed., *Physiology in the American Context*, pp. 293-321.

9. Erich Bauereisen, "Carl Ludwig as the Founder of Modern Physiology," *The Physiologist* 5 (1962): 293-299; Heinz Schræer, *Carl Ludwig, Begründer der messenden Experimentalphysiologie* (Stuttgart: Wissenschaftliche Verlagsgesellschaft M.B.H., 1967).

10. C. Ludwig, "Beiträge zur Kenntniss des Einflusses der Respirationsbewegungen auf den Blutlauf im Aortensysteme," *Müller's Archiv für Anatomie und Physiologie und wissenschaftliche Medizin* 1847: 242-302.

11. See E.J. Marey, *Du mouvement dans les fonctions de la vie: leçons faites au Collège de France* (Paris: Germer Baillière, 1868), pp. 130-131.

12. Ralph H. Major, "Karl Vierordt," *Annals of Medical History* new ser. 10 (1938): 463-473, esp. pp. 469-473. Vierordt demonstrated his sphygmograph in 1853. On its uses, see Karl Vierordt, *Die Lehre vom Arterienpuls in gesunden und kranken Zuständen gegründet auf eine neue methode der bildlichen Darstellung des menschlichen Pulses* (Braunschweig: Friedrich Vieweg und Sohn, 1855).

13. [E.] J. Marey, "Recherches sur le pouls au moyen d'un nouvel appareil enregistreur le sphygmographe présenté à la Société de Biologie," *Comptes rendus des séances et mémoires de la Société de Biologie* ser. 3, 1(1859): 281-309, and idem, *Physiologie médicale de la circulation du sang basée sur l'étude graphique des mouvements du coeur et du pouls arteriel avec application aux maladies de l'appareil circulatoire* (Paris: Adrien Delahaye, 1863).

14. See E.J. Marey, *La méthode graphique dans les sciences expérimentales et principalement en physiologie et en médecine* (Paris: G. Masson, 1878), esp. pp. 112-113.

15. On Marey's contributions, see Merrile Borell, "Marey and d'Arsonval: Two Visions of the Exact Sciences in Late Nineteenth-Century French Medicine," in J.L. Berggren and Bernard Goldstein, eds., *From Ancient Omens to Statistical Mechanics: Studies on the History of Science in Honor of Asger Aaboe*, volume 39 in the series *Acta historica scientiarum naturalium et medicinalium* (Copenhagen: University Li-

brary, 1987), and H.A. Snellen, *E.J. Marey and Cardiology: Physiologist and Pioneer of Technology 1830-1904—Selected writings in facsimile with comments and summaries, a brief history of life and work and a bibliography* (Rotterdam: Kookyer Scientific Publications, 1980). Robert G. Frank considers Marey's contributions to cardiac physiology in "The Tell-Tale Heart: Physiological Instruments and Clinical Hopes, 1855-1912," forthcoming in Coleman and Holmes, *Investigative Enterprise*.

16. See the translation of this work in Thomas S. Hall, ed., *A Source Book in Animal Biology* (Cambridge: Harvard University Press, 1951), pp. 313-320. On the origins of Helmholtz's techniques in muscle and nerve studies by Theodor Schwann and Emil Du Bois-Reymond, see John Gray M'Kendrick, *Hermann Ludwig Ferdinand von Helmholtz* (New York: Longmans, Green & Co., 1899), pp. 35-38.

17. The figure is taken from William Stirling, *Outlines of Practical Physiology being a Manual for the Physiological Laboratory*, Fourth ed. (London: Charles Griffin, 1902), p. 451. Similar illustrations may be found in most other late nineteenth-century texts. For citation of popular texts, see Borell, "Instruments and an Independent Physiology."

18. A chronology of plant growth registration techniques is found in William F. Ganong, *A Laboratory Course in Plant Physiology*, Second edition (New York: Henry Holt and Company, 1908). Ganong (p. 200) credits Sachs with the invention of the "auxograph" about 1872: "[Sachs' device] consisted of a large vertical eccentrically [sic] placed cylinder turned by a weight-and-pendulum clock, and having on one side a smoked paper, which was scratched by a pointer carried on a wheel connected with the plant by a thread." I am grateful to Gene Cittadino for directing me to Ganong's text.

19. The easiest way to follow these changes is through trade catalogues. I have used those found at the Bakken Library, Minneapolis (catalogues of Breguet, Paris; Verdin, Paris; and Boullite, Paris); the National Museum of American History, Smithsonian Institution, Washington, D.C. (especially the catalogues of Rudolph Koenig, Paris; Cambridge Scientific Instrument Company, England; E. Zimmermann, Leipzig & Berlin; C.H. Stoelting, Chicago; and C.F. Palmer, London); and the Harvard University Medical School, Boston (catalogues of Eugen Albrecht, Tübingen; the Harvard Apparatus Company, Boston; and Baird & Tatlock Ltd., London).

20. See Bauereisen, "Carl Ludwig," p. 297, and Hebbel E. Hoff, L.A. Geddes, and W.A. Spencer, "The Physiograph - An Instrument in Teaching Physiology," *Journal of Medical Education* 32 (1957): 181-198.

21. Marey, *La méthode graphique*, pp. 111-112 and 168; Marey, *Du mouvement*, pp. 111-112; H.E. Hoff and L.A. Geddes, "Graphic Recording before Ludwig: An Historical Summary," *Archives internationales d'histoire des sciences* 12 (1959): 3-25; and idem, "Graphic Registration before Ludwig, the Antecedents of the Kymograph," *Isis* 50 (1959): 5-21. For a critique of Hoff and Geddes' interpretation of Ludwig's innovation, see Schröer, *Carl Ludwig*, pp. 111-113. See also Robert P. Multhauf, "The Introduction of Self-registering Meteorological Instruments," *Contributions from the Museum of History and Technology, Paper 23, United States National Museum Bulletin* 228 (Washington, D.C.: Smithsonian Institution, 1961), pp. 95-116. Multhauf's paper shows the ubiquity of related instruments during this period. I am grateful to Deborah Jean Warner for introducing me to this very interesting work.

22. An "endless paper" kymograph is described in the Zimmermann catalogue of 1912 (*Psychologische - und Physiologische Apparate. Illustrierte List 25 und Nachtrag*), p. 151, item 2220. The paper is fed from a large roll onto the recording drum.

23. The Double Paper Brodie Starling Kymograph listed in the 9th Edition (1953) of the C.F. Palmer (London) Ltd. catalogue (p. 12) was designed by I. de Burgh Daly and described in the *Journal of Physiology* in 1930. The catalogue shows two long paper kymographs stacked on top of one another. I am grateful to Chris Lawrence for sending me photocopies of this apparatus.

24. See Borell, "Extending the Senses."

25. See Edwin Boring, *A History of Experimental Psychology*, Second ed. (New York: Appleton-Century-Crofts, Inc., 1957), Ch. 8, "The Personal Equation," pp. 134-153.

26. I thank Robert R. Pool, Jr. for this observation.

27. Marey, *Du mouvement*, p. vi.

28. The figure is from Walter B. Cannon's classic account *The Wisdom of the Body* (New York: W.W. Norton, 1932), p. 71. In it (p. 24) Cannon defined "homeostasis" as "[t]he coordinated physiological processes which maintain most of the steady states in the organism." For an account of his life and work, see Donald Fleming, "Walter B. Cannon and Homeostasis," *Social Research* 51 (1984): 609-640.

29. See Borell, "Marey and d'Arsonval," and later discussion in this paper.

30. Note correspondence with H.G. Funkhouser, "Historical Development of the Graphical Representation of Statistical Data," *Osiris* 3(1937): 269-404, and Multhauf, "Self-Registering Meteorological Instruments," 106.

31. Hughes Evans made this interesting observation during our joint investigations this past year. See the graphs in C.A. Wunderlich and E. Seguin, *Medical Thermometry and Human Temperature* (New York: William Wood & Co., 1871) and P. Lorain, *De la température du corps humain et de ses variations dans les diverses maladies*, 2 vols. (Paris: L'Imprimerie Nationale, 1977). In contrast, the early temperature charts by Wunderlich are displayed on white paper. However, both Ludwig and Traube used ink and paper for kymograph tracings, as discussed later in this paper.

32. See the tabular data of Ludwig in "Einfluss der Respirationsbewegung auf den Blutlauf," 269-302.

33. This transformation can be observed in the early volumes of the *American Journal of Psychology*. I thank Gail Hornstein for this evidence.

34. See Borell, "Marey and d'Arsonval," and Note 30.

35. E.R. Tuft, *The Visual Display of Quantitative Information* (Cheshire, Conn.: Graphics Press, 1983).

36. Traube's investigations are reprinted in L. Traube, *Gesammelte Beiträge zur Pathologie und Physiologie* 3 vols. (Berlin: August Hirschwald, 1871-1878), Band I: *Experimentelle Untersuchungen* Band II: *Klinische Untersuchungen* and Band III: *Klinische Untersuchungen*. See especially Traube's experiments from 1851-1852, on the effects of digitalis. For these experiments, he used Ludwig's kymograph as modified by Volkmann (I, 252-273). Here he presented data from kymograph tracings in tables. In earlier papers from 1850-1851, on the clinical effects of digitalis in fevers, he presented daily pulse, respiration, and temperature data from patients in tabular form (II, 97-204). He used graphic presentation (white on black), however, for similar data from 1851-1852 (II, 235-269, on pp. 258-260).

37. C.A. Wunderlich, *On the Temperature of Diseases: A Manual of Medical Thermometry*, translated from the second German edition by W. Bathurst Woodman (London: The New Sydenham Society, 1871), pp. 78-79.

38. Wunderlich did not find Marey's thermograph (1865) clinically useful because it did not fit well on the body. Christopher Lawrence considers physicians' resistance to the introduction of instruments in "Incommunicable Knowledge: Science, Technology and the Clinical Art in Britain 1850-1914," *Journal of Contemporary History* 20 (1985): 503-520, and "Moderns and Ancients: The 'New Cardiology' in Britain 1880-1930" in W.F. Bynum, C. Lawrence and V. Nutton, eds., *The Emergence of Modern Cardiology* Supplement 5 to *Medical History* (London: The Wellcome Institute for the History of Medicine, 1985).

39. Figure 4 shows the value of the graphic method in the study of pulse rate. It is Marey's figure showing P.J. Prompt's 1867 graphic presentation of Boerensprung's data originally published in 1840. The original caption read (*La méthode graphique*, p. 45): "Hourly variations in the rate of the pulse put onto a curve by Dr. Prompt after the numbers published by Boerensprung."

40. See E.J. Marey, "La chronophotographie: nouvelle méthode pour analyser le mouvement dans les sciences physiques et naturelles," *La revue générale des sciences pures et appliquées* 2 (1891): 689-719.

41. For a review of this interesting literature, see Robert G. Frank, Jr., "American Physiologists in German Laboratories, 1850-1900" in Geison, *Physiology in the American Context*, pp. 11-46.

42. For a case study of this process at Harvard, see Borell, "Instruments and an Independent Physiology."

43. On the British response to the rise of German laboratories, see Geison, *Michael Foster*, esp. p. 78.

44. I describe this process fully in "Instruments and an Independent Physiology." This process began earlier in chemistry and physics. On such pedagogical reform, see especially Albert E. Moyer, "Edwin Hall and the Emergence of the Laboratory in Teaching Physics," *The Physics Teacher* 114(1976): 96-103, and Owen Hannaway, "The German model of Chemical Education in America: Ira Remsen at Johns Hopkins (1876-1913)," *Ambix* 23 (1976): 145-164.

45. Hughes Evans and I have compiled a preliminary list of companies supplying major physiological and psychological apparatus in the period c. 1870-1905. Our list includes approximately forty-six in Germany, an additional ten in other German-speaking countries, and sixty more elsewhere. While we have some duplication of companies (their names changed during this thirty-five year period), our list clearly shows the rise of an important instrument manufacturing industry.

46. On the introduction of quantity production by the Harvard Apparatus Company, see Borell, "Instruments and an Independent Physiology."

47. The Harvard Apparatus Company made high-quality precision physiological apparatus available at cost to small colleges and state uni-

versities which otherwise could not have afforded the expense of equipping physiological laboratories. See *ibid.* Other American companies (we are aware of about twenty) also produced or supplied related equipment, but not as cheaply.

48. See especially Tuchman, "Science, Medicine and the State" and the articles by Tuchman and Lenoir in Coleman and Holmes, eds., *Investigative Enterprise* for a discussion of the specific goals of the German states in promoting physiology. On the origin and significance of the phrase training "for power," see Borell, "Instruments and an Independent Physiology," p. 300, n. 35, and p. 314, n. 98 and 99. The phrase, attributed to Harvard President Charles W. Eliot, was used by both William Townsend Porter and Henry Pickering Bowditch in their promotion of laboratory physiology. See *ibid.* and Kenneth M. Ludmerer, *Learning to Heal: The Development of American Medical Education* (New York: Basic Books, 1985), p. 52, n. 19.

49. The Boston Working Group, consisting of myself, Deborah Coon, Hughes Evans and Gail Hornstein, is currently examining these important relationships in physiology, psychology, and medicine.

Endorsing Referee: Timothy Lenoir

Will the Next Kepler Be a Computer?

John H. Holland, Keith J. Holyoak, Richard E. Nisbett, and Paul R. Thagard, *Induction: Processes of Inference, Learning and Discovery*. Cambridge: MIT Press, 1986, 384 pp. \$24.95.

Pat Langley, Herbert A. Simon, Gary L. Bradshaw, and Jan M. Zytkow, *Scientific Discovery: Computational Explorations of the Creative Processes*. Cambridge: MIT Press, 1987, 357 pp. \$9.95.

Both of these books are attempts to show how inductive reasoning can be modeled on computers. *Scientific Discovery* focuses on examples drawn from the history of science, though the discussion of historical cases is brief and very limited. *Induction* discusses inductive reasoning across a wide range of phenomena, including learning, attributions of cause in social situations and, in a next-to-final chapter, scientific discovery. Neither book discusses the role of social factors in discovery. Therefore, both are particularly relevant to those interested in the cognitive psychology of science.

Scientific Discovery

This book focuses on heuristics as the main ingredient in scientific discovery. Heuristics are "rules of thumb" that guide the search for a solution to a problem. Most of the book is spent describing heuristic-based computer programs designed to model famous scientific discoveries. For example, a program called BACON was designed to re-discover several scientific laws from data, including Kepler's third law. The earliest version of BACON used three heuristics to find Kepler's third law:

If the values of a term are constant, then infer that the term always has that value. If the values of two numerical terms increase together, then consider their ratio. If the values of one term increase as those of another decrease, consider their product. (p. 66)

Later versions of BACON added heuristics to increase generality, e.g., ones to express the ratio in the second heuristic as a linear function and check that function by gathering further data. BACON is given data on the distance between the sun and each planet and the angle found by using a fixed star and the planet as two endpoints with the Sun as the pivot at different times. From this data, BACON finds an inverse relation between the distance (D) and the slope (s) which it eventually uses to discover that D^3s^2 is a constant.

Then the program stops, and Langley et al. claim that it has "discovered" Kepler's third law, with a simplified version of his second law embedded within. To anyone familiar at all with Kepler's actual discovery process (see Koestler, 1959 for a good account) the program falls ludicrously short. No one presented Kepler with a table of

data, neatly arranged, and told him to discover an arbitrary mathematical relationship. Kepler started with a belief that geometric solids could be fitted into the orbits of the planets with the sun at the center of the solar system. When this belief did not accord with Tycho Brahe's data, he had to go through the agonizing process of altering his framework, deciding which data were worth using to frame and check his new model. His success was due, in part, to persistent calculations based on Brahe's data, but no one else would have known that such calculations were worth doing. As Koestler says,

He had been searching for this Third Law, that is to say, for a correlation between a planet's period and its distance, since his youth. Without such a correlation, the universe would make no sense to him; it would be an arbitrary structure. If the sun had the power to govern the planet's motions, then that motion must somehow depend on their distance from the sun; but how? Kepler was the first who saw the problem—quite apart from the fact that he found the solution to it, after twenty-two years of labour. The reason why nobody before him had asked the question is that nobody had thought of cosmological problems in terms of actual physical forces. (p. 395)

To put this in the language of modern cognitive science, Kepler's mental representation of the solar system was more important than the heuristics he used to discover his laws. (For a similar argument regarding the performance of subjects in scientific reasoning experiments, see Gorman, Stafford, and Gorman, 1987.) BACON has no mental representations; it merely searches for quantitative relationships between arbitrary terms supplied by the programmers, who interpret its results to be "discovery" of Kepler's third law. One feels they have missed the point of Kepler's genius entirely.

There are several points in *Scientific Discovery* where Langley et al. issue disclaimers, pointing out that programs like BACON—and the book includes many other programs designed to model historical discoveries from different sciences—were not intended to model the history of particular discoveries. Rather, these programs present "ways in which historical discoveries could have been made with the help of a few simple heuristics and with moderate amounts of computation" (p. 170). But this disclaimer does not answer the problem raised in the Kepler example. There is no way Kepler could have found his laws "with a few simple heuristics"; he had to represent the solar system in a way that suggested where to look for lawful relations.

In a final chapter, the authors attempt to address this problem of mental representations, including pictorial images. Their claim is that the same sort of programs that

produced Kepler's third law could be adapted to form representations. But they regard these sorts of representations as relatively simple (see p. 318) and also talk very little about how a machine could form them from the kind of information and beliefs Kepler had available. Certainly we could find a way to program a computer to begin with the assumption that the planets' orbits can be filled by geometric solids, then provide it with data that shows the inadequacy of that view and heuristics that would guide it to Kepler's solution. But this isn't what Kepler did. He had to decide that geometric solids must fit into the planetary orbits, figure out how to test this assumption, and develop radical new assumptions, e.g., the idea that some kind of force emanating from the sun controlled the motion of the planets. Historians and philosophers may find some of Langley et al.'s other cases of greater interest than their account of Kepler. But there is no danger that their programs, even if modified to create representations of the form they suggest toward the end of the book, will be able to make novel discoveries of the sort that Kepler made.

Langley et al. close by arguing that scientific creativity should not be regarded as mysterious, as fundamentally different from other human cognitive processes. I agree—we need to try to understand the cognitive aspects of scientific and technological creativity. But it seems to me that their account of scientific problem-solving is as limited as early behavioristic accounts of learning—a necessary and halting first step, perhaps, but one which we must go beyond as rapidly as possible.

Induction

Fortunately, more promising approaches are already at hand, and *Induction* represents one of them. Although it has only one chapter on scientific discovery, it is of far more interest to psychologists of science than *Scientific Discovery*. Whereas most of the latter book is detailed description of computer programs, the former spends only one chapter describing the authors' computer implementation of their model and the rest applying the model to important areas of psychology, including scientific discovery.

Induction's approach relies far more heavily on mental representations than heuristics. Holland et al. try to demonstrate that mental representations are best viewed as sets of rules, including rules for transitions from one state to another. They use the example of "Jennifer" (p. 58-62), who notices that an "object in her visual field is small and black, has a long axis in the horizontal direction, and is an animal" (p. 59). Then they show how her "system" computes values for several mutually exclusive alternatives, e.g., black dog, black cat, and black squirrel; dog gets the highest value, but "the tentative characterization . . . does not reach some implicit confirmation threshold" (p. 60). So the system tells Jennifer to move her eyes "along the animal's horizontal axis," which leads to the observation that the animal has a round head. This new information adds strength to the squirrel and cat categories;

cat passes the "confirmation threshold" and Jennifer realizes she is looking at a cat. One of the strengths of *Induction* is that its framework allows for competing rules or representations, whereas the programs in *Scientific Discovery* can consider only one rule at a time.

Sociobiologists hold that each organism acts as if it were a sort of "genetic calculator," calculating whether kin are sufficiently close to warrant sacrificing its own life to save their (Dawkins, 1976). Similarly, Holland et al. construct a model of thinking based on rules which involve complex calculations. Unlike the sociobiologists, there is no "as if" involved in Holland et al.'s analysis; they assume that these sorts of calculations are being carried on by a computational cognitive system. Lest the analogy seem too far-fetched, it should be pointed out that Holland et al. mention biological systems as one area in which the mathematics of induction should have its greatest application. Critics of the whole computational perspective would argue that this mathematical level of analysis is unnecessary (see, for example, Searle, 1984). For the present, it is best to say that Holland et al. provide substantial evidence that in many situations the human or organism acts *as if* it were doing computational calculations before recognizing or acting.

Holland et al.'s more sophisticated computational model comes a step closer to Kepler than BACON's simple heuristics. In fact, Holland et al. use the Kepler example to criticize BACON along the lines I sketched above (pp. 324-5—but I developed my critique independently of theirs). They also present a more sophisticated philosophy of science than *Scientific Discovery*, due in part to the fact that one of the co-authors, Thagard, is a philosopher. A quotation will illustrate:

How do scientists arrive at new laws? A common view, misattributed to Francis Bacon, is that scientists generally start with the data and then by "induction" directly derive laws that describe those data. More typically, we will argue, the discovery of laws requires the kinds of reconceptualizations that are central to our account of problem solving; synchronic search among concepts is as important as the search for diachronic realities. We will argue that this is true even for observational laws, or laws that cover observable events; it is even more obviously so for theoretical laws that require top-down concept-formation. (p. 323)

In other words, theory formation involves searching for relationships among concepts as well as search for regularities in the data. Holland et al. incorporate both types of search in their model and they recognize that, "Although he started with relatively accurate data furnished by Tycho Brahe, Kepler's discovery of the three famous laws would have been impossible without several dramatic reconceptualizations of the problem domain" (p. 324). The amount of reconceptualization is far greater for a Newton or an Einstein; Newton, for example, took Kepler's and Galileo's laws and forged them into a single system.

Holland et al. don't say how exactly their model would

explain Kepler's discoveries, but clearly numerical computations concerning the relative strength of various rules would be involved on some level. For example, Kepler's system of geometric solids might be represented as a set of rules; the system would then seek data, e.g., Brahe's, to see if the geometric-solids model reached a "confirmation threshold." Inconsistencies between the data and the model would lead to rules that embodied exceptions and also to reconceptualizations until Kepler's set of rules gradually yielded his three laws.

Perhaps Kepler did sit in his study poring over Brahe's data while his computational system assigned weights to different rules, then sent him back to look for more data. Certainly, subconscious or semi-conscious processes of this sort do go on in scientists—and all of us—much of the time. But this model still misses the heart of what it meant to be Kepler. While these calculations may be relatively automatic in a case where a child perceives a cat in the distance (although even that is debatable), they are not automatic in a case like Kepler's, where a scientist feels he or she has seen through Nature's veils and is trying to transform the ecstasy of discovery into sober fact. He did not compute passively; he made conscious decisions about where to seek information and what to do with it. The program outlined in *Induction* cannot be a Kepler until it develops metacognitive awareness—until it can consciously evaluate alternatives and not just automatically perform calculations.

For example, Kepler's decision to abandon the Copernican assumption that planets had circular orbits was not a simple, rational response to calculations based on the evidence that Mars' orbit could not be a circle. Instead, Kepler had to cope with a result that was in drastic conflict with his representation of the way the universe worked: the orbit of Mars was an oval.

The oval . . . has an arbitrary form. It distorts the eternal dream of the harmony of the spheres, which lay at the origin of the whole quest. Who art thou, Johan Kepler, to destroy divine symmetry? All he has to say in his own defence is, that having cleared the stable of astronomy of cycles and spirals, he left behind him "only a single cart-ful of dung: his oval." (Koestler, p. 329)

To understand Kepler, one must do more than construct a complex set of competing rules. One must understand how his emotional reaction to the "cart-ful of dung" drove him on to a year of fruitless calculations, lead him even to repudiate his own second law—all while using ellipses in his calculations without realizing that the ellipse was the key to the orbit of Mars. I don't see how even Holland et al.'s computer program could replicate this "wild goose chase" unless it were programmed in advance to follow these exact steps, in which case it would be modeling nothing but the programmer's ingenuity in setting up the steps.

Conclusion

From *Scientific Discovery*, we learn the strengths and limitations of a heuristic-based approach that virtually

ignores mental representations. From *Induction*, we learn the prospects and problems associated with translating mental representations into sets of rules. My own view (see Gorman, 1987) is that a complete view of the creative process will have to include both mental representations and heuristics; a unique mental representation is a major ingredient in scientific genius, but creative scientists also utilize or develop powerful heuristics—including some of the ones discussed by Langley et al.—that allow them to test and modify their representations.

Like many cognitive scientists, the authors of both books are working up from lower-order cognitive processes, e.g., in *Induction*, how you recognize a cat in the distance and in *Scientific Discovery*, how you perceive relationships in data already ordered so as to make the discovery of the relationships easy. What the psychology of science needs is more cognitive psychology of higher-order processes. (Holland et al. briefly recognize this problem on p. 350.)

Specifically, both books ignore the problem of consciousness, or what Searle (1984) calls intentionality. Cognitive scientists are not alone in their failure to adequately address this issue; sociologists and social historians, for example, frequently treat the individual as merely a product of social forces. Certainly, social factors play a major role in shaping an individual scientist's representations and choices of heuristic; Kepler's preference for circular orbits is, in part, a result of his times. However, Kepler is no more a mere product of social circumstances than he is a complex calculator or a set of heuristics. A successful psychology of science must incorporate consciousness as well as mental representations and heuristics. Computer models may be useful in other ways, but even the most sophisticated parallel-processing architectures do not incorporate consciousness.

One of the most hopeful aspects of the book *Induction* is that the authors come from different disciplines. Only through genuinely interdisciplinary collaboration can we hope to recapture the whole Kepler, including his conscious intentions as well as the subconscious social and cognitive factors that shaped his discoveries.

References

- Dawkins, R. *The Selfish Gene*. Oxford: Oxford University Press, 1976.
- Gorman, Michael E. "A Framework for Understanding the Cognitive Style of Inventors." Paper presented to the Society for the History of Technology, Pittsburgh, October 24, 1986.
- Gorman, Michael E., A. Stafford, and Margaret E. Gorman. "Disconfirmation and Dual Hypothesis on a More Difficult Version of Wason's 2-4-6 Task." *The Quarterly Journal of Experimental Psychology* 39A (1987): 1-28.
- Koestler, A. *The Sleepwalkers*. New York: Grosset & Dunlap, 1963.
- Searle, J. *Minds, Brains and Science*. Cambridge: Harvard University Press, 1984.

Reviewed by Michael E. Gorman

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Workshop on Scientific Discourse and Scientific Applications

A workshop on scientific discourse was held on 22-23 April 1987 at the University of Bradford. There were nine papers, representing a wide range of methodologies (from sociology, linguistics, philosophy, psychology, and literary theory) and materials (such as texts from health economics, molecular genetics, personal construct psychology, news reports on AIDS, and texts from discourse analysis itself). Discussions focused on three areas of methodological and theoretical controversy: how discourse analysis should itself be presented in texts, how various theories of language relate discourse and science, and linguistic approaches to textual analysis.

Reflexivity and New Literary Forms

Recently, analysts of scientific discourse have begun to discuss what would happen if their approach to scientific knowledge were applied to their own discipline, and if their own texts could point up the variability of interpretation they saw in scientific texts. This issue was raised, for instance, in a paper by Michael Mulkay, Malcom Ashmore, and Trevor Pinch, entitled "Measuring the Quality of Life: A Sociological Invention Concerning the Application of Economics to Health Care." This study of a health economics article did not just analyze the article to reveal its rhetorical construction; instead, it presented a dialog between two voices, a "sociological voice" which critiques the methods of health economics, and a questioning voice that critiques the approach of the sociologist. The paper concludes with a discussion between these two voices of the kind of authority the sociologist uses to challenge the health economist. Discussion of the paper focused on how the readers of *Sociology*, in which the paper will be published, would interpret these textual devices.

Other participants addressed similar issues in different ways. Steve Woolgar and Malcom Ashmore presented an introduction to a forthcoming collection of such papers that experiment with new forms *Knowledge and Reflexivity* (Sage, 1987). The introduction itself had two voices, and raised questions about the neat context it presented for this turn in sociological discourse. Jonathon Potter developed his own earlier analysis of a transcript from a conference of psychologists to show how discourse analysis could not lead to a definitive reading of a text. One of the liveliest discussions of the workshop was on a paper by Trevor Pinch that presented arguments against such experiments with forms, saying they just distracted the readers from the important points being made in the sociology of scientific knowledge. But Pinch's paper took the form of these experiments (or parodied them) with a "senior author" making the criticisms and a "junior author" (also called Trevor Pinch) chiming in with remarks on the senior author's critique.

Philosophies of Language and Science

Several papers took up the issues of the relations between philosophies of language and the claims of discourse analysts within the sociology of scientific knowledge. All of them challenged, in different ways, the underlying assumptions of participants. Charles Bazerman's paper, "How Language Realizes the Work of Science," defended a (largely North American) tradition in the sociology of science that is rejected by most British sociologists, a tradition in which science cannot be treated as entirely a social construction. He drew on two theorists from the 1920s and 1930s, Vygotsky on language and Fleck on the genesis of scientific facts, to outline a theory in which the constraints put on science by nature would be crucial in the work of scientific texts. (The paper is a chapter in Bazerman's forthcoming book, *Shaping Written Knowledge*, University of Wisconsin Press). Andy McKinlay responded to the claim of many discourse analysts in the sociology of scientific knowledge that any text could have a radical multiplicity of meanings, examining several philosophical positions on the interpretation of texts, and arguing that there could be a basis for finding one interpretation preferable to others. Teri Walker defended reflexivity as an element in inquiry into knowledge, but argued that the exemplary case of such reflexivity is to be found, not so much in the sociologists who champion it, but in the practice of scientists. Thus her paper, which is based on an analysis of ideas from Piaget and Garfinkel, is titled "Reflexivity: The Appliance of Science."

Linguistic Approaches

The approach of Brian Torode, drawing on linguistic methods of discourse analysis, attempted a synthesis that was new to many of the participants. Torode situated his paper within debates in sociology about whether there could be an objective analysis of news reporting, and argued that objective analysis of such surface textual features as relative clauses could be linked to a broader analysis of the discourses within an article. His texts in this case were newspaper reports on AIDS in Africa, in which he traced four interwoven discourses. Greg Myers, in another paper that applied linguistic analyses to a sociological issue, traced narrative structure, verb structure, and thematization in accounts of a scientific discovery. He argued that the actors and actions of the discovery were constructed in later retelling, in news articles, popularizations, and textbook accounts, rather than in the first announcement in scientific articles.

Greg Myers, Modern Languages Centre
University of Bradford

A New Association for Studies of Biology

The Society for the History, Philosophy, and Sociology of Biology was formed at a June conference held at Virginia Tech. Approximately 200 attended the meeting, divided about equally among historians, philosophers, and biologists. There were about seven sociologists in attendance, and a scattering of participants from other disciplines. About ten percent of attendants came from abroad.

Scholars interested in the history, philosophy, and sociology of biology have met biennially since 1983. Attendance at these meetings has grown very rapidly, and the formation of a formal association was a natural outgrowth of this interest. Membership in the Society is open to anyone interested in the history, philosophy, or sociology of biology.

Interest in sociological perspectives and institutional issues has grown rapidly in the last four years. The program of the meeting reflected this, with a plenary session on "Discipline transformation" and workshops on biology at the University of Chicago; biotechnology; the organization of natural history; organization of biological research, 1900-1940; and reproductive science and technology. Several work-in-progress papers also reflected these concerns. An impromptu session on methods was of particular note.

The Society elected Richard M. Burian of Virginia Tech Honorary President in recognition of his outstanding

work in providing a forum for scholars in the field. An Executive Committee was elected and charged with organizational matters. Walter Bock (Department of Biological Sciences, Columbia University, New York, New York 10027 USA) was elected Chair of the Committee. A Program Committee was elected to prepare for the next conference of the Society, and James R. Griesemer (Department of Philosophy, University of California, Davis, CA 95616 USA, Bitnet: JRGRIES@DAVIS) was elected chair.

The next meeting of the Society will be in 1989. The place has not yet been determined, and the Executive Committee welcomes suggestions and offers of facilities. These should be sent to Jane Maienschein, Department of Philosophy, Arizona State University, Tempe, AZ 85287 USA. The Program Committee is eager to have suggestions for sessions at the 1989 meetings.

Further information is available from Richard Burian, Department of Philosophy, Virginia Tech, Blacksburg, VA 24061 USA, Bitnet: RMBURIAN@VTVM2; or from Elihu Gerson, Tremont Research Institute, 458 29 Street, San Francisco, CA 94131 USA, Bitnet: TREMONT@UCSFVM)

Elihu M. Gerson
Tremont Research Institute

Prizes

The Lehigh University Press will award a prize of \$1500 and a publication contract for the best original book-length manuscript in the field of Science, Technology and Society Studies. Application forms and submission rules may be obtained from the Lehigh University Press, Chandler-Ullmann Hall #17, Lehigh University, Bethlehem, PA 18015 USA. Manuscripts will be due by April 1, 1988.

The University of Delaware Press has announced a \$1000 award for the best book-length manuscript submitted in the fields of history of science, medicine, and technology. The scope encompasses studies of specialized developments and their intellectual histories, as well as the social and cultural aspects of these fields. Manuscripts must be submitted before 30 September 1987. For details and entry form write: History of Science, Medicine and Technology Manuscript Competition, University of Delaware Press, 326 Hullihen Hall, Newark, DE 19716 USA. Telephone 302-451-1149.

SOCIETY FOR SOCIAL STUDIES OF SCIENCE

1987 MEETING

Worcester, Massachusetts

November 19-22

PRELIMINARY PROGRAM

Thursday, November 19, 7-9:00 pm

Derek Price Memorial Plenary

Don Beaver (Williams College), Organizer & Chair

Friday, November 20, 8:30-10:30 am

1. Property, Power, and Ideology in Science & Technology

Chair: **Rick Worthington** (Rensselaer Polytechnic Institute).

Interests, Ideology and Control over Risk Communication. **Stephen Hilgartner & Dorothy Nelkin** (Cornell University).

Scientific Information and Intellectual Property: The Case of the Hybridoma/Monoclonal Antibody Technique. **Michael MacKenzie**, (Ottawa University) & **Alberto Cambrosio** (University of Québec at Montreal), and **Peter Keating** (Harvard University).

Scientific Communities and Control in Science. **Tove Thagaard** (University of Oslo).

Science-Ads. **Tom F. Gieryn** (Indiana University) and **Elizabeth Hunt** (University of Pennsylvania).

2. Writing, Citing, Publishing, and Science

Chair: **Harry Paul** (University of Florida, Gainesville).

The Development of Scientific Publishing in the United States. **Elisabeth S. Clemens** (University of Chicago).

Compelling Argument: The Emergence of the Form of Book One of Newton's Opticks. **Charles Bazerman** (Baruch College, CUNY).

Citation Life Cycle: Longitudinal Use and Classification. **Virginia Cano** (University of Western Ontario).

The Senses of Significance in Scientific Articles **Olga Amsterdamska & Loet Leydesdorff** (University of Amsterdam).

3. Patterns of Scientific Work

Theories and Models in Scientific Work. **Michael J. Moravcsik** (University of Oregon), **Adolf Hohenester** (Universitat Graz) & **Leopold Mathelitsch** (Universitat Graz)

Complex Integration of Science: The Case of Modeling Acid Rain in Europe. **C. Schulte Fishedick** (University of Amsterdam).

Beyond Serendipity: Factors Affecting Performance in Condensed Matter Physics. **Diana Hicks** (University of Sussex).

Toward an Ethnographic History of Molecular Biology: Interpreting the 50th Anniversary of the First X-Ray Protein Photograph. **P.G. Abir-Am** (Harvard University).

10:30 am-12:30 pm

4. Social Studies of Medicine and Medical Knowledge

Chair: **Susan Leigh Star** (Tremont Research Institute).

The Uses of Science: The Invention of the Medical Profession. **Julia Loughlin** (Syracuse University).

Intervention and Mortality: Physicians and the Birth Process in the Nineteenth Century. **Janet Carlisle Bogdan** (Syracuse University).

Comparative Origins and Development of Reproductive Science in the U.S. and Great Britain, c. 1900-1950. **Adele Clarke** (University of California, San Francisco) & **Merriley Borell** (Tufts University School of Medicine).

The Necessarily Social Character of Medical Knowledge: Vitamin C and Cancer. **Evelleen Richards** (University of Wollongong).

5. Scientific Mobility and Technological Innovation

Organizers: **Carole Ganz** (National Science Foundation) and **H. Roberts Coward** (SRI International).

Chair: **H. Roberts Coward** (SRI International).

Researcher Mobility in Biotechnology: A Bibliometric Approach. **Carole Ganz** (NSF) and **H. Roberts Coward** (SRI International).

'Big Science' Facilities and International Scientific Mobility. **Catherine P. Ailes** (SRI International).

Foreign Students and Foreign Born Scientists and Engineers in the U.S. Workforce. **Michael G. Finn** (Oak Ridge Associated Universities).

Personnel Flows and Modernization in Manufacturing. **John E. Ettlie** (University of Michigan).

1:30-3:30 pm

6. Risk, Receptivity, and Technological Change

The Impact of New Technology in the Fire Safety System. **Robert W. Fitzgerald** (Worcester Polytechnic Institute).

Technological Change and Institutional Crisis in the Fire Service. **Gregory K. Doerschler** (Worcester Polytechnic Institute).

Audiences and Problem Choice in Toxicological Research. **Peter Groenewegen, Karin van den Berg, & Philip Vergragt** (State University of Groningen).

University Labs and Industrial Needs. **Larissa Lomnitz, Delia Lon, & Rodrigo Diaz** (National University of Mexico).

7. Philosophical Social Studies of Science

Chair: **Michael Zenzen** (Rensselaer Polytechnic Institute).

Reservations About Reflexivity and New Literary Forms. **Trevor Pinch & Trevor Pinch** (University of York).

The Eighteenth Century Problem: Natural Philosophy, Experiment, and Discourse in Post-Kuhnian Perspective. **Graeme Watchirs & John Schuster** (University of Wollongong).

The Copernican Revolution as a Counterexample to both Lakatosian 'Rational Reconstruction' and Strong Programme 'Social Construction'. **Howard Margolis** (University of Chicago).

The Observer's Frame of Reference in Natural and Social Science. **John Schumacher** (Rensselaer Polytechnic Institute).

3:30-5:30 pm

8. Perspectives on the Social Psychology of Science

Organizers: **John Wilkes** (Worcester Polytechnic Institute) & **Gerald Gordon** (Boston University).

Chair: **Gerald Gordon** (Boston University).

The Cognitive Styles Perspective and its Implications for the Nature of Science. **Gerald Gordon** (Boston University).

An Overview of Science Careers Research Using the Myer-Biggs Type Inventory. **Mary McCaulley** (University of Florida).

Comparative and Connective Research on the Cognitive Styles Topology. **John Wilkes** (Worcester Polytechnic Institute).

Left and Right Brain Orientation as They Affect the Management of Industrial Science and Engineering. Speaker to be announced.

9. Academic, Industrial, and Military Relations of Science and Technology, I.

Organizers: **Henry Etzkowitz** (SUNY Purchase) and **Everett Mendelsohn** (Harvard University).

Chair: **Everett Mendelsohn** (Harvard University).

Before the Manhattan Project: The Decision to Build the Bomb. **Stanley Goldberg** (Smithsonian Institution).

The Emergence of Military Interest in Biotechnology, 1980-1986. **Susan Wright** (University of Michigan).

G.W. Beadle and His Program in Biochemical Genetics During World War II. **Lily E. Kay** (American Philosophical Society).

A Quantitative Study of Structural Relationships Between Academic Scientists and the Biotechnology Industry. **Sheldon Krinsky & James G. Ennis** (Tufts University).

ROUNDTABLES

3:30-4:30 pm

IA. Current Research and Education in Science Ethics (Part 1).

Organizer: **Terry Russell** (American Chemical Society). Participating with him: **Rachelle Hollander** (NSF), **Mark Frankel** (AAAS), **Rosemary Chalk** (Cambridge, MA), and **Kenneth Bechtel** (Wake Forest).

IB. Data Sources on the Scientific and Engineering Professions in the United States. **Bob Jones** (American Chemical Society).

IC. Challenges in Computers and Education. **Leonard Goodwin** (Worcester Polytechnic), "Long Term Effects from a Short Term Innovation in Computer Science Instruction"; **Eileen Traut** (Northeastern), "What is College Level Computer Literacy?"

ID. Establishing Priority in Industrial Research: A Case Study. **Joop Schopman** (Utrecht).

IE. Collaboration vs. Isolation—the Scientific Paradigm. **Raphael Sassower** (University of Colorado).

IF. The Politics of Space Science: The Case of the International Solar-Terrestrial Physics Program. **Pascal Fung**.

ROUNDTABLES

4:30-5:30 pm

IIA. Current Research and Education in Science Ethics (Part 2).

Organizer: Terry Russell. Participants: same as Roundtable IA.

IIB. Data Sources on the Scientific and Engineering Professions (international). Bob Jones (American Chemical Society) and John de la Mothe (Canadian Research Manpower Planning Office).

IIC. General Education in Natural Science. Martin Krieger (USC).

IID. Cultural Imperatives in the Development of British Traffic Technology (or, the Breakdown of the Technological Mentality) Bruce Harley.

IIE. Social and Governmental Implications of Environmental Release of Genetically Engineered Organisms. Joseph Amatucci, Eric Arn, Colleen Burgess, David Hanlon.

IIF. Video Tape Showing (Part I). Computers in Context. From California Newsreel.

Saturday, November 21

8:30-10:30 am

10. The Social Relations of Artificial Intelligence

Chair: James Coggins (University of North Carolina).

Interaction as a Basis for Artificial Intelligence. Les Gasser (University of Southern California).

Artificial Intelligence and Real War. Chris Hables Gray (University of California, Santa Cruz).

Engineering Knowledge: An Anthropological Study of an Artificial Intelligence Laboratory. Diana Forsythe (Stanford University).

11. Measuring the Effects of Public Policy for Science

Organizer and chair: Lawrence Burton (National Science Foundation)

12. The Sociology of Technological Design, I.

Chair: Elihu Gerson (Tremont Research Institute)

Instruments and Values in Science. Larry Busch & Paul Marcotte (University of Kentucky).

Centralization in Russian Electrification: The Convergence of Politics and Technologies in the Early Twentieth Century. Jonathan Coopersmith (Massachusetts Institute of Technology).

The Alienating Character of Technique in Science. Chandra Mukerji (University of California, San Diego).

The Organization of Computer Chip Design Work. Susan Leigh Star (Tremont Research Institute).

10:30 am-12:30 pm

13. Communication Patterns in Science

The Reception of Extraordinary Claims in Science: Lessons From the 'Memory-Transfer' Episode. Larry Stern (Texas A & M University).

The Communication of Research-Related Information in Genetics. Katherine McCain (Drexel University).

The Integration of Cognitive Neuroscience. Edward Manier (University of Notre Dame).

Heterogeneous Problem Solving in Sociology and Artificial Intelligence. Susan Leigh Star (Tremont Research Institute).

14. The Social Structure of Scientific Fields

Factors Affecting Scientific Accomplishment. Sally Brewster Moulton (Boston University).

Mainstream and Individual Research Strategies in Agricultural Sciences. Yvon Chatelin (University of Kentucky) & Rigas Arvanitis (Universidad Central de Venezuela).

Age and Scientific Productivity: Field and Organizational Differences. Sharon G. Levin (University of Missouri, St Louis) & Paula Stephan (Georgia State University).

Intertemporal Changes in the Field Choices of Elite and Rank-and-File Economists. Arthur M. Diamond, Jr. (University of Nebraska) & Donald Haurin (Ohio State University).

15. Interests, Negotiations, and Controversies in Science and Technology

Chair: Ned Woodhouse (Rensselaer Polytechnic Institute).

A Science and Technology Studies Perspective on the Formaldehyde Controversy. Deborah Mayo (Virginia Polytechnic Institute and State University).

Scientists' Professional Interests in the Acid Rain Controversy. Stephen Zehr (Indiana University).

Controversy in Science: The Case of Dioxin. Rosa Haritos (Columbia University).

The Reproduction of Actor Identity in Negotiating Nuclear Waste. Gary Downey (Virginia Polytechnic Institute and State University).

12:45-2:15 pm Luncheon Plenary

Keynote Address: **Evelyn Fox Keller** (Northeastern University).
Feminist Perspectives on Science Studies (tentative title).

2:30-3:30 pm

PANEL: Commentaries on Keller.

ROUNDTABLES

IIIA. Video Tape Showing (Part 2). *Computers in Context*. From California Newsreel.

IIIB. *Artificial Intelligence*. **Steve Woolgar** (Brunel).

IIIC. *How might the 4S benefit from computer conferencing? (A report on the EIES experiment)*. **Steve Fernandez & Brian Winter**.

IIID. *Student Pugwash Comes of Age. (Presentation on Developments at the Chapter Level)*. **Jim Bennett & Scott Saleska**.

IIIE. *Public Knowledge and Public Opinion in the Nuclear Power Debate. (A review of ten years of comparative studies of college campuses)*. Organized by **Chuck Carleton**.

IIIF. *Between Science and Art: Understanding Motion. (Review with slides of a Worcester Art Museum College Gallery exhibition)*. **Hilde Hein** (Holy Cross).

IIIG. *Useful Convergences between Science Studies and Current Policy Issues*. **Peter Healey & John Ziman** (Science Policy Support Group).

IIIH. *Issues in the Social Psychology of Science*. Organizer: **Ron Westrum**. Chair: **Ryan Tweney** (Bowling Green).

3:30-6:00 pm

16. Quantitative Science Studies

Chair: **A.F.J. van Raan** (University of Leiden).

Discipline Integration, Citation Patterns, and the Plate Tectonics Revolution. **John Stewart** (University of Hartford).

Policy and the Interpretation of Co-Word Data: Notes on Acid Rain Research. **Serge Bauin** (Centre de Documentation Scientifique et Technique), **Jean-Pierre Courtial** (Centre de Sociologie de l'Innovation), & **John Law** (University of Keele).

Intellectual Foci in Research Programmes: The Use of Word and Co-Word Linkages Among Articles as an Indicator. **Loet Leydesdorff** (University of Amsterdam).

The Question of Mapping Science. **Jean-Pierre Courtial** (Centre de Sociologie de l'Innovation).

A Validation Study of Bibliometric Indicators: The Comparative Performance of Cum Laude Doctorates in Chemistry. **A.J. Nederhof & A.F.J. van Raan** (University of Leiden).

17. Visual Dimensions of Science

Organizer and chair: **Roger Krohn** (McGill University)

Practical-Representational Activities in Science: The Artful Work of Astronomers at High-Tech 'Imaging Facilities'. **Michael Lynch** (Boston University) & **Samuel Edgerton** (Williams College).

The Visual Presentation of Theory and Data: A Cognitive View. **Ronald N. Giere** (University of Minnesota).

Graphs and Photos as Mediating Devices Between Environmental Practice and Public Discourse. **Roger Krohn** (McGill University).

Scientific Imagery and Popularized Imagery: Differences and Similarities. **Bernard Schiele** (University of Quebec at Montreal) and **Daniel Jacobi** (CRELEF-CNRS, Université de Besançon).

Words and Pictures: A Comparison of Visual and Verbal Representations of Natural Phenomena. **David Gooding** (University of Bath).

18. Perspectives on Science Policy.

Chair: **Paul Hoch** (University of Warwick).

Radon: A Case Study in Scientific Knowledge and Science Policy. **Leonard A. Cole** (Rutgers University).

Is EUREKA Strictly Non-Military? **Johannes M. Becker** (University of Marburg/FRG).

Changing Policy Conditions and their Effects on the Dutch Research Community. **Jack Spaapen** (University of Amsterdam).

The State-Centered Dimensions of Science Policies and the Internationalization of the Science. **Vittorio Ancarani** (Università di Torino).

Swedish Science Policy and the Two Cultures. **John Hultberg** (University of Goteborg).

19. The Sociology of Technological Design, II.

Chair: **Elihu Gerson** (Tremont Research Institute).

Accidents, Technological Systems, and the Allocation of Blame: The Train Crash at Hinton, Alberta. **Gary Bowden** (University Toronto).

Fashioning an Acceptable Technology: Biodegradable Synthetic Detergents in the United States. **William McGucken** (University of Akron).

The Social Construction of Three Ultracentrifuges: (About the Evolution of Dinosaurs and Dolphins). **Boelie Elzen** (University of Twente).

How do Diagnostic Images Become Meaningful? **Stuart Blume & Bernike Pasveer** (University of Amsterdam).

Beyond Debunking and Deconstruction: Towards a Critical of Technology and Society. **Richard Badham** (University of Wollongong).

Sunday, November 22

8:30-10:30 am

20. The Power of Science

Organizer and chair: **Susan Cozzens** (Rensselaer Polytechnic Institute).

Power and Sociotechnical Control: The Case of the TSR2 Project. **John Law** (University of Keele).

Research Policies and Scientifico-Political Coalitions: Laboratories and Their Actor Networks. **Michel Callon** (Ecole Nationale Supérieure des Mines).

The Power of Science. **Susan Cozzens** (RPI).

Scientists as Policy Influentials. **John M. Logsdon** (George Washington University).

21. Social Organization and Social Change in Scientific Communities

Chair: **Tom Carroll** (Rensselaer Polytechnic Institute).

Bandwagons in Science. **Joan Fujimura** (Tremont Research Institute).

Standardization Processes in an Emerging Scientific Research Community: The Case of Network Analysis. **Sam Gilmore** (University of California, Irvine).

The Carbon Dating of the Shroud of Turin: The Closure of a Debate? **H. Laverdiere** (University of Bath).

Discipline and Profession. **Elihu Gerson** (Tremont Research Institute).

22. Comparative and Historical Methods in Science Studies

Organizers: **Thomas F. Gieryn** (Indiana University) & **Ellsworth Fuhrman** (Virginia Polytechnic Institute and State University).

Chair: **Ellsworth Fuhrman** (Virginia Polytechnic Institute and State University).

Comparative Historical Sociology of Science. **Ellsworth Fuhrman** (Virginia Polytechnic Institute and State University).

Episodic Inquiry or Long-Haul History? **Thomas F. Gieryn** (Indiana University).

To be announced. **Randall Collins** (University of California, Riverside).

11:30 am-1:30 pm

23. Rhetoricians on the Rhetoric of Science

Organizer and Chair: **Charles Bazerman** (Baruch College CUNY).

Rhetorical Considerations in The Construction and Justification of Science Policy Decisions: The Case of the Cambridge Experimentation Review Board. **Craig Waddell** (Rensselaer Polytechnic Institute).

Arguing in Different Forums. **Jeanne Fahenstock** (University of Maryland).

Monologue and Dialogue in the Rhetoric of Science: The Case of the Double Helix. **Michael Halloran** (Rensselaer Polytechnic Institute).

Discourse as Text—Discourse as Practice. **Carolyn R. Miller** (North Carolina State University).

24. Academic, Industrial, and Military Relations of Science & Technology, II.

Organizers: **Henry Etzkowitz** (SUNY Purchase) and **Everett Mendelson** (Harvard University).

Chair: **Henry Etzkowitz** (SUNY Purchase).

The Strategic Defense Initiative and American Universities. **Ann Lenier** (University of Colorado, Boulder).

The Social Construction of Militarism: Deterrence, Coercion and Strategic Thought. **Jim Falk** (University of Wollongong).

Technology Transfer from Military to Civilian Markets: Case Studies of Munitions and Helicopters. **David Dyer** (The Winthrop Group).

Crystallization of a Strategic Alliance: American Physics and the Military in the 1940s. **Paul K. Hoch** (University of Warwick).

25. Scientific Practice and Scientific Knowledge.

Can Experiments Be Used to Study Experimental Science? **Michael E. Gorman** (Michigan Technological University).

Constructivism and Structuration Theory. **Rob Hagendijk** (University of Amsterdam).

Can the Durkheim-Mauss Thesis be Resuscitated? **Warren Schmaus** (Illinois Institute of Technology).

Science as a Self-Organized System—A New Approach to the Dynamics of Scientific Theories. **Wolfgang Krohn & Gunter Koppers** (Bielefeld University).

1:30-3:30 pm

26. Critical Theory in Mathematics Education

Organizer and chair: **Ubiratan D'Ambrosio** (Universidade Estadual de Campinas).

Ubiratan D'Ambrosio (Universidade Estadual de Campinas).

Marilyn Frankenstein (University of Massachusetts, Boston).

Herbert Bernstein (University of Massachusetts, Amherst).

Jean Lave (University of California, Irvine).

27. Political Dimensions of Science and Technology.

Audiences and Problem-Choice in Toxicological Research. **Peter Groenewegen** (University of Groningen).

Political Creativity in Science: The Antebellum State Geo-

logical Surveys. **Stephen Turner** (University of South Florida).

Modelling the Grasslands. **Chunglin Kwa** (University of Amsterdam).

The Politics of Genetic Diversity. **Lawrence Busch, William B. Lacy, & Jubel Moraga-Rojel** (University of Kentucky).

28. Perspectives on Science, Technology, R&D, and Policy

How Much Can Science Contribute to Technology? **David Collingridge** (Aston University).

Military R&D in the Federal Republic of Germany. **Rainer Rilling** (University of Marburg).

New Materials: Governmental Policy for Stimulation of R & D and Monitoring the Possible Consequences. **Philip Vergragt & Peter Groenewegen** (State University of Groningen).

The Social Construction of Military Space Technology in West Germany. **Johannes Weyer** (University of Bielefeld).

4S Business Meetings

Publications Committee
Council
S&TS Editors Meeting
4S Business Meeting

Thursday, 1-2:30 pm
Thursday, 2:30-5:30 pm
Saturday, 7 am
Sunday, 10:30-11:30 am

Abstracts

Friday, November 20

8:30 - 10:30 am

Session 1

Property, Power, and Ideology in Science & Technology

Interests, Ideology and Control over Risk Communication. **Stephen Hilgartner**, Columbia University, and **Dorothy Nelkin**, Cornell University

Scientific information about risk is conveyed to the public in many ways: through direct statements of scientists, media reports, advertising claims, and government reports and recommendations. This information is often controversial as public health organizations, scientists, government officials, industrial interests and consumer groups assert their right to control the public discourse.

This paper develops a framework for analyzing controversies over risk communication. Drawing examples from disputes about the relationship of diet to cancer, we suggest that communication controversies take place in four contexts: news reports, scientific advice and recommendations, promotional claims, and disclosures from industrial interests. In each context we will document the controversies that develop over the timing of reports, the adequacy of technical evidence, the proper role of government, and the competence of the public to make informed choices. And in each case we will suggest how disputes over these issues become struggles for power and control over the information conveyed to the public.

Scientific Information and Intellectual Property: The Case of the Hybridoma/Monoclonal Antibody Technique. **Michael Mackenzie**, Ottawa University, **Alberto Cambrosio**, Universit  du Qu bec Montral, and **Peter Keating**, Harvard University

We would like to present a study of the process by which the Hybridoma/Monoclonal Antibody technique, from Milstein and Kohler's seminal publication in 1976 (and despite its affirmation as a profoundly important scientific advance by the 1984 Nobel Prize), has become increasingly territorialised—in terms of aspects of the technique, applications of the technique, and specific antibodies—as private property. Though this has been (and will continue to be) a highly volatile situation, there have been actions which appear to impinge upon the use of monoclonals even in a pure research situation.

In the presentation, after giving a brief description and history of the technique, we will examine in detail three separate disputes which have taken place in the scientific literature and/or the legal arena.

The first of these concerns the two 'basic' patents granted to the Wistar Institute in late 1979/early 1980 for antibodies against viral and tumour antigens. Though these have yet to generate any legal dispute they have provoked a great deal of argument in the scientific literature especially with respect to Wistar's position vis   vis Milstein and Kohler's priority. The patents have also been refused by patent offices outside the U.S.

The second concerns the Tandem diagnostic kit patented by Hybritech and, after a series of lengthy court disputes, successfully defended by them against Monoclonal Antibodies Inc. This case has had a major impact on the highly lucrative diagnostic kit business affecting not only the smaller biotech start-up companies but also major multi-nationals such as Abbott.

The third dispute concerns eight patents for the OKT monoclonals (for differentiation of lymphocyte subsets) developed by Ortho (a subsidiary of Johnson and Johnson). After warning about two dozen scientists (including NIH researchers) that their use of monoclonals from the American Tissue Culture Collection could infringe on Ortho's patent, even if used in a pure research situation, Ortho went on to sue Becton Dickinson for infringement and received a very favourable out-of-court settlement.

Finally we hope to present what we think are the central features linking these disputes (including the legal concepts) and draw preliminary conclusions concerning the open science vs. private property issue.

Scientific Communities and Control in Science. **Tove Thagaard**, The University of Oslo

What is the importance of scientific communities for development and control in science? What is the impact of scientific communities compared to local factors? The present paper attempts to answer these questions. Interviews with 140 scientists, mostly in physics and chemistry, lead to the following conclusions:

1) Scientific communities have directive effects on scientific work. Research issues defined as relevant on the international research frontier influence the choice of research issues within the local contexts.

2) Within the range of problems defined as relevant on the international research frontier, local factors influence the choice and design of research. In the interviews, scientists referred to the impact of available technology, financial resources and local interests.

3) The directive effects of scientific communities are important for control in science. The shared knowledge of relevant research problems implies that similar research is performed in different countries at the same time. Thus different findings will either verify or contradict each other. In the interviews, the control effect of scientific communities was emphasized. Influence from scientific communities increases control in science, especially in disciplines with a cumulative knowledge structure such as we find in physics and chemistry.

Science-Ads. **Thomas F. Gieryn**, Indiana University, and **Elizabeth Hunt**, University of Pennsylvania

Science is business in disguise. Scientists sell knowledge-products as commodities in a competitive market; their livelihood depends on it. A variety of buyers (industry, the state, venture capital) choose to buy science, and their decisions are

rooted in vague assumptions about what it is that they are buying. Scientists are (unlike the typical manufacturer of consumer-goods) selling the future rather than an extant product; they exchange promises for cash (facilities, jobs, credibility, etc.). To increase sale of science, scientists *benefit from* the advertisement of their wares (though they themselves only rarely make the pitch). Science-ads appear everywhere—our illustrations come from mass market magazines. These ads provide images of science that justify or legitimize the purchase of science; science-ads are sites for the cultural reproduction of what “everyone” believes about science. We focus on one paradox of science-ads appearing in consumer magazines: though science is typically used here as a ploy to sell deodorant or mouthwash, the effectiveness of the ploy depends on the “de-commercialization” of science (that is, scientists and their goods are detached from the commercial nexus of buying and selling). This is paradoxical because science is a commodity not unlike deodorant or mouthwash. Our empirical analysis centers on graphic and textual accomplishments of “de-commercialization” in science-ads.

Session 2 Writing, Citing, Publishing, and Science

The Development of Scientific Publishing in the United States. Elisabeth S. Clemens, University of Chicago

Over the past century, the quantity of print related to the natural sciences has grown enormously. Established journals have expanded, the number of scientific publications has multiplied, and new genres such as scientific journalism have become institutionalized. At one level, these developments have been incorporated in systems of evaluation and stratification within the scientific professions, but as specific genres become institutionalized they also influence the shape and success of scientific analysis and argument. The interrelated development of publishing institutions, genres, and audiences is suggested by a comparative historical study of selected general, specialist, and popular scientific publications. Particular attention is paid to the appearance and disappearance of specific genres of scientific writing and the changing relations among different types of publications. Rather than reifying the distinction between the popular and the professional, the historical study of these publications contributes to a critical understanding of the conditions of production of scientific texts. ✎

Compelling Argument: The Emergence of the Form of Book One of Newton's Opticks. Charles Bazerman, Baruch College CUNY

Newton presented the material of Book One of the *Opticks* in at least seven substantially different forms, from private notebook through university lecture and journal article to the final book form. Each of these presentations reflected Newton's sense of the rhetorical situation and task at hand. In particular journal publication presented special challenges to Newton. He first tries a plausible, but ultimately unsuccessful, rhetorical strategy of establishing his ethos as a proper Baconian through a discovery narrative, and then simply laying out his findings as a doctrine, upon the authority of his ethos. When this fails and controversy erupts, he comes to reevaluate the character of the agonistic forum of the Royal Society and the Philosophical Transactions. In the course of the public controversy he develops new rhetorical strategies that compel the reader down a line of perception, experience, interpretation, and thought. By establishing an empirical/intellectual juggernaut that controls both experience and meaning, he finds a compelling form of argument that shapes the final presentation in the *Opticks*.

Session 3 Patterns of Scientific Work

The Concept of Theory and Model in the Working Scientific Community. Michael J. Moravcsik, University of Oregon, Adolf Hohenester, Universitat Graz, and Leopold Mathelitsch, Universitat Graz

Using two specific topics in high energy physics (the so-called Regge-approach and quark physics) as foci, a group of high energy physicists were polled to find out how such active researchers use the terms of “theory” and “model” and how they conceptualize scientific information in general. Although most respondents expressed their ideas in very definite terms, there was a large variety of different answers. One might conclude that while the “Gestalt” of the scientific methodology in the head of a scientist is rather definite, the semantics of verbalizing it are vague. Two kinds of characteristic play a role in describing “theory” and “model”: the epistemological structure of a new idea and its predictive power. It appears that the former serves more as a guideline initially when a new theoretical approach is assessed, but later the latter attains an at least equally important role. In general, the concept of a theory appears to have a greater prestige among scientists than that of a model, but in the specific examples used in this study, opinions differed considerably as to whether a certain approach was a theory or a model. A particularly interesting phenomenon that emerges from the responses is the process of *demoting* a theory to a model. The results suggest that studies of this kind might possibly help us to critically evaluate a new theoretical approach in science even at the time of its conception.

Beyond Serendipity: Factors Affecting Performance in Condensed Matter Physics. Diana Hicks, SPRU—University of Sussex

This paper examines the extent to which the relative success and failure of condensed matter physics experiments in exerting an impact on their field is related to the local, contextual factors which shape the production of those experiments. Both quantitative and qualitative data are used to investigate two areas in condensed matter physics, superfluid helium three and spin glass research. The quantitative data consist of bibliometric statistics covering each field from its beginnings in the early seventies to the present, relatively stable state of research. The qualitative data are drawn from interviews with British and American authors of selected papers.

The study is comparative, contrasting the production of more and less important papers. “More-important papers” are those which not only were judged important to the field by the authors, other interviewees, and a wider sample of researchers in the field, but also were among the top 10% most highly cited papers in the field. The publications were selected for comparison not only on the basis of their impact. In order to make the comparison as broadly based as possible, the selected papers also span many years and represent the work of groups with both strong and weak publication records.

Among the contextual factors analyzed in the comparison are these: abundance or shortage of funds; problems and possibilities created by equipment which was available or could be obtained; extent of communication within the group, department and field; the working relationship between the authors, including the intangible “atmosphere” in the lab and the division of labor; motivation of researchers; prior record of the group in the field; relative number of senior scientists, post-doc's and Phd students among the authors; prestige of the group's institution, and prestige of the authors' Phd institutions.

Toward An Ethnographic History of Molecular Biology: Interpreting the 50th Anniversary of the First X-Ray Protein Photograph. P.G. Abir-Am, Harvard University

This essay describes the performative interaction among participants at the 50th anniversary of the first X-ray protein photo, and interprets it as a social ritual.

Part I deconstructs two key "documents," the invitation and the program, as textual strategies designed to mobilize the "tribal assembly" for a succession rite in which three successor heroes divide among themselves the legacy of a venerated "ancestor"—the principal producer of the first protein X-ray photo.

Part II describes the performative interaction of the three successor-heroes who shared their recollections of their respective "initiation rites," by the "ancestor"/discoverer, into the early X-ray crystallography of proteins, hormones and viruses, with the "tribal assembly" while reemerging as legitimate spokespersons-for-a-field's history.

Part III interprets the performative action of the keynote speakers as a ritual of fusion between the recalled "ancestral realm" and the "ongoing present." This fusion is performatively accomplished by alternating diverse layers of constructed historical authenticity with concrete representations of scientific progress, especially increasing resolutions of X-ray protein patterns, which symbolize the "tribe's" *raison d'être*.

The essay concludes with a theoretical discussion of such fusion rituals as a mechanism for maintaining the social authority of the protein X-ray crystallographers' disciplinary clan. Specifically, it is argued that historically oriented scientific rituals are designed to modulate the built-in contradiction between the relativity of past science and its continuous processes of taboo observance or social control; and the absolutist claims of frontier science's discontinuous outcomes of totemic "discoveries" or objective knowledge.

10:30 am-12:30 pm

Session 4 Social Studies of Medicine and Medical Knowledge

Intervention and Mortality: Physicians and the Birth Process in the Nineteenth Century. Janet Carlisle Bogdan, Syracuse University

Maternal deaths in nineteenth century New York City remained at approximately one per one thousand women of childbearing age throughout the nineteenth century. In New York City populations for which we are able to calculate it, maternal mortality (maternal deaths divided by number of births multiplied by 1000) varied from as low as eighteen per 10,000 to as high as 3,200 per 10,000. Rates varied widely from year to year depending on the extent of septic deaths. In this paper, I examine maternal deaths in New York City and explain why the average rate of septic death remained higher than one would expect, given the decline in the infectious death rate in the City especially during the second half of the century. For the lack of decline in the septic portion of maternal deaths I found cause in physicians' interventions in the birth process coupled with their relative unwillingness to consider their agency in introducing or spreading puerperal (childbed) infection.

During the century the number of ways physicians intervened increased. Drug and instrument use increased, especially after midcentury when anesthesia was introduced as a childbirth aid, an innovation that rendered intervention less painful. Willingness to intervene heroically was not accompanied by willingness to take preventative measures against the puerperal infection rampant during the century, however. Until

they were pushed by the threat of public scrutiny, doctors argued among themselves about the possible cause of childbed fever epidemics and the best methods of treating the scourge. Maternal deaths continued to be high. By the 1880s, methods of preventing puerperal fever, imported from Britain and Europe, began being tried out in New York City hospitals where resistance had met them earlier. As these methods succeeded and spread to other hospitals and to private practitioners, the septic maternal death rate began to fall.

Comparative Origins and Development of Reproductive Science in the U.S. and Great Britain, c1900-1950. Adele Clarke, Tremont Research Institute and Stanford University, and Merriley Borell, Tufts University

This paper compares early twentieth century British and American reproductive research initiatives and relations among cognitive frameworks, institutional arrangements and national scientific traditions. While modern reproductive science has been an intersection of efforts among biological, medical and agricultural investigators, the relative contribution of each has varied over time and geography as has the relative social stature of these professions. Turn of the century initiatives were made by British investigators embedded in physiology and agricultural science. After c1910, American scientists began investigating explicitly reproductive problems, but, in contrast, tended to be embryologically-oriented and based in anatomy and zoology departments. Agriculturally-trained scientists did not significantly participate in reproductive research in the U.S. until after c1925. Valid as these assertions are, they raise important questions about the boundaries, territories, approaches and problem structures which actually characterized research in these fields. Were there essential differences between problems addressed and approaches taken in physiology, anatomy, zoology and agricultural science? What relations did these bear to institutional sponsorship? What relations did institutional sponsorship bear to national traditions of scientific work organization? The paper explores these questions by examining the established British tradition of agricultural science as well-sponsored and prestigious basic research, the development of British physiology with an "anatomical bias," the development of American zoology and anatomy with a "physiological bias," the emphasis of American agricultural science on plants and contagious disease, and the relation of biochemistry to reproductive science in both Britain and the U.S.

The Necessarily Social Character of Medical Knowledge: Vitamin C and Cancer. Evelleen Richards, University of Wollongong
In recent years, the privileged epistemological status of medical knowledge has been eroded from various directions: by those critics who have questioned its efficacy and autonomy; by social anthropologists and historians who have explored its cultural contingency; and by medical sociologists, through their delineation of medicine as an institution of social control and the locus of professional power struggles for cognitive authority and control. To date there has been little attempt to tie these various approaches into coherent explanation of the social character of contemporary medical knowledge, and bring it into line with recent work in the sociology of scientific knowledge.

This paper, which focuses on the contemporary vitamin C and cancer controversy, represents an attempt to examine and explain medical knowledge in the light of this revised view of scientific knowledge.

In brief, the content and context of the debate over the efficacy of vitamin C in the treatment of cancer are reconstructed and analyzed, and compared with medical responses to and evaluations of two other cancer drugs: the cytotoxic drug 5FU (conventionally used in the treatment of gastro-intestinal cancers), and the "naturally occurring" (but recombinant DNA pro-

duced) drug interferon. Some revealing comparisons are drawn between these three areas where professional, economic and political interests differ in kind and degree. This comparative analysis is designed to show that such interests inevitably intrude on the "objective" processes by which knowledge claims about therapeutic efficacy are evaluated by the powerful adjudicating medical community. It is argued that the assessment of medical therapies is inherently a social and political process; that the idea of neutral appraisal is a myth; that clinical trials, no matter how rigorous their methodology, inevitably embody the professional values or commitments of the assessors, and that judgments about experimental findings may be structured by wider social interests such as consumer choice or market forces. It is concluded that the necessarily social character of medical knowledge cannot be eliminated by methodological reform, and that this has important implications for the social implementation of medical therapies and technologies.

Session 5 Scientific Mobility and Technological Innovation

Researcher Mobility in Biotechnology: A Bibliometric Approach. Carole Ganz, National Science Foundation and H. Roberts Coward, SRI International

This paper explores use of state-of-the-art techniques in bibliometrics to capture mobility of researchers between industrial firms, between academic institutions, between the industrial and academic sectors, and among countries. The techniques also are used to assess government and corporate mobility-related policies which aim at ensuring an adequate supply of researchers in critical and strategic fields.

The paper looks at biotechnology and the mobility of its researchers for the years 1978-1982, during which a tremendous expansion in research activities took place. In the United States, it was accomplished through expansion in the activities of large industrial companies, and through establishment of small firms dedicated to applying the new biotechnology. In other countries, the route was solely through expansion of activity in established companies, and here at a diminished pace from the United States. Further, government funding for applied work in this area in the United States did not expand, while there were increases in other countries. The paper looks at changes in the country and organizational addresses of authors in the bibliometric model of biotechnology-related research. The following hypotheses are explored:

First, the new, small firms would be in the best position to attract manpower from established companies, from universities, and from abroad. Second, mobility among companies would rise, and universities would increasingly be unable to compete with industry for personnel. Finally, the United States would be in the best position to attract researchers from abroad.

The paper also compares results of the bibliometric analysis with anecdotal and survey evidence that these trends did emerge, and with national and corporate policies to encourage retention of employees and to attract new ones.

Foreign Students and Foreign Born Scientists and Engineers in the U.S. Workforce. Michael G. Finn, Oak Ridge Associated Universities

The United States scientific and technical workforce is increasingly composed of foreign born scientists and engineers. This research tracks recent increases in foreign student enrollment at U.S. universities and provides estimates of the proportion of these students who stay to work in the U.S. This is shown to account for most of the immigration of scientists and engineers

to the U.S. during recent years. In addition to estimating initial stay rates for foreign students, I also estimate emigration rates during 1981-1986 for foreign-born scientists and engineers working in the U.S. at the start of this decade. Stay rates and emigration are estimated for several disciplines. Also examined are the relative importance of different countries and geographic regions as sources of immigrants and destinations of emigrants.

The paper also provides a brief summary of characteristics of foreign-born scientists and engineers in the U.S. including citizenship status, level of education, type of employment, and earnings relative to comparable U.S. born scientists and engineers. Finally, the paper gives a brief review of some of the costs and benefits associated with the increasing U.S. utilization of immigrant scientists and engineers.

Personnel Flows and Modernization in Manufacturing. John E. Ettlie, University of Michigan

Innovative manufacturing firms manage the movement of people for the successful use of advanced manufacturing technology. Rather than leaving personnel changes across organizational boundaries to chance, progressive firms evaluate the human resource needs in plants and respond to these needs when possible.

Recent data from domestic plants and business units that have launched significant modernization programs indicate that this process includes the orchestration of technology vendors, as well, which can be considered a temporary personnel flow.

We have found that there is no significant relationship between turnover in operating personnel for a new technology system and the amount of personnel flow on the modernization project from the time it began. This suggests that movement of key project personnel is a managed action and is related to other business unit characteristics and outcomes.

About 65% (20) of 31 reporting plants in this domestic plant study report the movement of at least one important modernization project member at some time during the course of the program. The report of at least one personnel flow of this type was correlated significantly with the following:

- an aggressive manufacturing technology policy ($p < .05$);
- a business unit strategy dominated by new product introduction ($p < .01$); and
- the use of at least one administrative innovation specifically to deploy the new technology system ($p < .05$).

These results suggest that management of the personnel flow process for modernization is a strategic issue in a large number of domestic manufacturing firms. Further, the specific pattern of personnel movement implies the emergence of an evolving human resource policy that is consistent with a new modernization philosophy in these firms. For example, the movement of manufacturing engineers working on advanced manufacturing planning projects to area supervision in these modernizing plants was an interesting trend in these data.

1:30 - 3:30 pm

Session 6 Risk, Receptivity, and Technological Change

Technological Change and Institutional Crisis in the Fire Service.

Gregory K. Doerschler, Worcester Polytechnic Institute
Although emphasis on building fire protection in the United States has been steadily increasing, the nation's fire depart-

ments still largely maintain their traditional "reactive" role emphasizing fire suppression. However, evidence suggests that the fire service may have difficulty justifying itself in this "traditional" role in the near future. Recent developments in fire protection engineering offer the promise of greatly reducing the incidence and severity of fires by incorporating the primary fire defense of a structure within the design of that structure itself, rather than in an external fire suppression force. Such a developing technology could spell disaster for a fire suppression service which depends on a steady incidence of fire as a means of justifying its existence on a large scale.

This presentation reports on a survey study which was carried out as an undergraduate thesis project at Worcester Polytechnic Institute. Survey forms were distributed to high ranking fire officials in 166 large city fire departments nationwide. The survey was designed to assess the extent to which these fire officials perceive a potential threat to their service's existence in the near future, and whether they would be willing to recommend major role changes for the fire service to deal with any perceived threat.

Essentially, this study examined leadership perceptions in a major social institution facing organizational crisis brought on by an external technological development. The fact that a strong consensus emerged among the respondents that the fire service needs to alter its role in accordance with changing times was rather surprising, since individuals familiar with the service had previously suggested that the service would be reluctant to shed its traditional fire suppression orientation.

Audiences and Problem Choice in Toxicological Research. **Peter Groenewegen, Karin van den Berg, Philip Vergragt**, University of Groningen, Netherlands

Risk has been an issue for both policy and science analysts in the last couple of years. In social studies of science discussion has focussed on a small number of themes. One of the issues has been the emergence of controversies between various scientific experts. In the paper we will evaluate the involvement of different scientific actors in the definition of health risks of the use of Diethylstilbestrol (DES). The main emphasis is on the existence of positive images and negative images of one chemical compound. DES is seen by some as a drug and by others as a health hazard, still it has been used for a long period before the negative image led to action restricting the use of it as a drug. The questions we try to answer in the paper is whether any of the scientific or professional images of DES played a role in the process of continued use of a possibly risky drug. One possible explanation of such developments is the existence of mutually exclusive scientific communities one specialized in risk issues and one specialized in use of drugs.

University Labs and Industrial Needs. **Larissa Lomnitz, Delia Leon and Rodrigo Daz**, National University of Mexico

Although the National University of Mexico is, in general terms, the largest institution of research of our country, there have been very weak ties with the productive sector in order to transfer the technological innovations developed there. Just to solve these problems, in 1985 was created The Centro Para la Innovacion Tecnologica (Technological Innovation Center).

Broadly speaking, the task commended to the CIT has been to link the University with industry.

In this paper we analyze different cases in which the CIT has funnelled technological transference. We treat technological transference as more than a mere administrative problem: the main obstacles for an uninterrupted process of transference are related to differences in language, values, interests, expectations and rhythms of work among the three kinds of actors involved, scientists, bureaucracy (where we include financial

support, legal advice, the university power structure and the CIT), and entrepreneurs. In order to fully understand the nature of transference of technology between UNAM and the productive sector we have constructed a profile of the institutional system involved.

Our profile is based on an analysis of the 160 projects of technological innovation that the CIT promoted until 1986. There are interrelations between, at least, two of the three institutional sectors in each of these experiences. The history of these projects—where institutions are confronted—casts light on the main problems for the development of that irregular relationship: technological transference.

Session 7 Philosophical Social Studies of Science

Reservations about Reflexivity and New Literary Forms. **Trevor Pinch and Trevor Pinch**, York University, England

This paper examines the claims made for reflexivity and the use of unconventional texts (known as New Literary Forms) in the sociology of scientific knowledge. The paper engages in particular with the recent arguments of Mulkay, Woolgar and Ashmore. It is suggested that there is no necessary connection with a concern for reflexivity and the adoption of unconventional literary forms. Some of the difficulties involved in trying to establish a self-consistent reflexive position in the sociology of science are explored. It is shown that arguments for reflexivity which claim to show that earlier forms of sociology of science, such as for example relativism, and discourse analysis, are in some way incomplete and/or inconsistent—difficulties which can be resolved with the addition of reflexivity—must also themselves suffer from the same problems of incompleteness and/or inconsistency. New Literary Forms is the latest way of trying to address this paradox.

Much of the text is concerned with exploring the practical difficulties of arguing and reading an unconventional text. In order to tackle these issues in a self-exemplifying way the paper has been written as a dialogue between two voices (hence the joint authorship of Pinch and Pinch). One voice argues for reflexivity and New Literary Forms, whilst the other contests this position. The text as a whole ends in paradox. If it has been successful in showing the limitations of New Literary Forms it has only done so through the use of a New Literary Form!

The Eighteenth Century Problem: Natural Philosophy, Experiment and Discourse in Post-Kuhnian Perspective. **Graeme Watchirs and John Schuster**, University of Wollongong

The eighteenth century problem has alternatively been seen as the problem of sorting out the meaning and scope of Newtonianism, or, by Kuhn and Bachelard, as the problem of understanding the proliferation of new effective fields of experimental science—electricity, chemistry, heat, physical optics—emerging from a morass of 'pre-paradigmatic' or 'pre-scientific' thought. Recently, Cantor, Schaffer, Shapin and others have stressed the category of 'natural philosophy' per se as a key to understanding the eighteenth century. Agreeing that this is a necessary step beyond the ambivalences of Kuhn and Bachelard concerning natural philosophy, we observe that natural philosophy is still a confused category of historical analysis. We contend that a fruitful conception of eighteenth century natural philosophy involves a critique and development of Bachelard's notion of experimentation and 'phenomeno-techniques', enriched by perspectives drawn from the newer sociology of experiment and from literary theories of discourse. Along the way we suggest that experimenta-

tion, and instruments themselves, are discursive phenomena, and we explore how the 'essence seeking' and hence 'two-place' (before and after) historiographies of Kuhn, Bachelard and others, may be overcome. Our conclusions are played off against some existing interpretations of the period, and are illustrated by the case of the 'maturation' of electro-statics.

The Copernican Revolution as a Counterexample to Both Lakatosian 'Rational Reconstruction' and Strong Programme 'Social Construction'. **Howard Margolis**, University of Chicago
If we look at how astronomers behaved, we find very good evidence that they abandoned Ptolemy neither soon after the Copernican argument was spelled out in 1543, nor around 1610 (with the invention of the telescope). Rather, the decisive turn came around 1590: 40 years after *de Rey* appeared, but still 20 years before new evidence of any real consequence was available. This makes poor sense in terms of either version of Lakatos' (1975) 'rational reconstruction' of the episode.

But the episode also resists a 'strong programme' social construction. Confronted with three observationally identical schemes (Ptolemaic, Copernican, Tychonic) astronomers abandoned the Ptolemaic option two decades before new evidence appeared to warrant that; then, with two schemes left which were consistent with what the telescope revealed, they abandoned the Tychonic decades before Newton finally provided arguments that went beyond what Kepler and Galileo were discussing by 1620. This behavior seems downright clairvoyant unless we suppose that beliefs were shaped by things stubbornly 'out there' in the physical world. But that such shaping of beliefs could occur is not itself magical for creatures whose cognitive instincts were themselves (imperfectly) shaped by selection for acquiring beliefs that fit the world.

The argument is based on the account of the discovery and contagion of the Copernican idea in my *Patterns, Thinking & Cognition*, University of Chicago Press, 1987.

The Observer's Frame of Reference in Natural and Social Science. **John A. Schumacher**, Rensselaer Polytechnic Institute
Bruno Latour argues that, in coming to understand how Einstein used frames of reference in relativity theory, we can come to a better understanding of the role of frames of reference in sociology. He contends as well that in the process we can improve Einstein's notion of a frame of reference. Although Latour does us a service in opening such comparative theory work, he is not always faithful to Einstein. Latour sees clearly how "the transversal path established in between frames" is crucial to relativity theory, but not clearly how these frames themselves must be remarkably different from any previously touted classical frame. By recognizing the existence of invariant space-time intervals across space-time frames—an invariance that goes hand in hand with the relativity of space or time intervals across such frames—we call into question Latour's distinction between relativism and relativity for space-time frames. Einstein was not just "fiddling with time and space." However, Latour rightly argues that the loss of a privileged frame in relativity theory—the loss of the ether—must have implications for the role of the observer in sociology: "social context is our ether." Or again, as I would put it, we cannot establish a privileged reference body for social observation. But what exactly is the invariance across frames of reference in sociology? Moreover, another practice in physics, that of quantum mechanics, may well suggest an entirely different way of establishing an analogy between the physical context in physics and the social context in sociology. If we clarify Latour's analysis and supplement it with what we can learn from quantum mechanics, what do we learn about an observer's frame of reference in sociology?

And in turn can we improve our understanding of an observer's frame of reference in physics? The aim of this paper is to answer these questions.

3:30-5:30 pm

Session 9 Academic, Industrial, and Military Relations of Science and Technology, I.

Before the Manhattan Project: The Decision to Build the Bomb. **Stanley Goldberg**, Smithsonian Institution

At the end of 1941, a blue ribbon committee of members of the National Academy of Sciences examined the state of research on fission and the technology of isotope separation. At the time, many of those doing the research were skeptical that a fission bomb was feasible. Nevertheless, the NAS committee report concluded that such a device could be built and their recommendation was that American efforts should turn immediately from research on fundamental processes to investigation of production alternatives. The driving force behind these conclusions was Vannevar Bush, who was convinced that there was no other choice than to proceed. Throughout his career, Bush used oversight committees to advise him on the proper course of action. It was one of several administrative devices that allowed him to keep a low profile. In this instance, the committee report was far more optimistic than the situation warranted. Whether or not we knew how to build a fission bomb, Bush and those closest to him were sure that the Germans were going all out to make such a device. The fear that a fission bomb might be possible was the engine which drove Bush to call back the NAS committee three times until he got a recommendation he found acceptable.

The Emergence of Military Interest in Biotechnology, 1980-1986. **Susan Wright**, University of Michigan

This paper is an examination of the development of military interest in genetic engineering and other forms of biotechnology, with particular attention to a) the relation between military policy and the larger context of international relations; and b) changing perceptions of the military potential of genetic engineering. It offers a critical assessment of the likely impact of the militarization of biotechnology on the international legal regime prohibiting biological warfare.

A Quantitative Study of Structural Relationships Between Academic Scientists and the Biotechnology Industry. **Sheldon Krimsky and James G. Ennis**, Tufts University

The paper reports on a study of scientific advisory boards (SAB) in the biotechnology industry. Membership of academic scientists on SABs is a proxy measure for "having a formal relationship with a firm." A survey of over 500 public and private biotechnology companies reveals patterns of academic-commercial networks in the context of a rapidly growing industry. The data provide answers to some key questions about the patterns of affiliation of university expertise. These include: Which universities are providing the major sources of expertise to the industry? How is the intellectual capital distributed among biotechnology firms? What diversity of industry affiliation is found in universities? Social network analysis is used to explore the aggregate pattern of dual-affiliated scientists. Network analysis characterizes the involved actors (corporations, universities and scientists), the rules of their interaction, and the global pattern or structure of their associations. Through a

market-segmentation model, the stratification of universities and firms are examined along with the bases of their affinities. The data are also analyzed to determine potential conflicts of interest among the population of dual-affiliated scientists. Academic scientists on biotechnology advisory boards are cross-correlated with peer reviewers of the National Science Foundation, public advisory panels of the National Institutes of Health and membership in the National Academy of Sciences.

Saturday, November 21

8:30-10:30 am

Session 10

The Social Relations of Artificial Intelligence

Interaction as a Basis for Artificial Intelligence. **Les Gasser**, University of Southern California

Engineering "intelligent" problem-solving systems and explaining social behavior (i.e., the coordinated activity of a collection of individuals) seem like two radically different problems, and in some senses they are. But a number of researchers have begun to raise the question of how to conceive intelligent behavior as anything but an enterprise of coordination. From this perspective, "intelligent" action is "coordinated" action because either 1) it depends upon coordination among individuals, in the sense that problems are posed by some, for others, resources are distributed, interpretations of data and events are jointly constructed, etc., or 2) it depends upon coordination among competing representations of a problem or solution, in the sense that one individual faces many representational options.

In this case, it makes plenty of sense to examine what our conceptions of social behavior and our steps toward machine intelligence have in common. I suggest in this paper requirements for modeling turn out to be surprisingly similar. To address the core question of sociology—how people manage to act together—we must address some of the core problems of machine intelligence.

Since science is a branch of human activity overtly concerned with problem-solving, it makes sense to examine scientific enterprise in the context of coordinated problem-solving action. All collections of scientists, then, are concerned with the following questions:

- How to represent problems, in ways that facilitate decomposing high-level tasks into simpler ones, and allocating them among an array of scientists.
- How to coordinate the activities of scientists or scientific groups (e.g., how to achieve coherence and "distributed control" in problem-solving);
- How individual scientists or groups represent their environment, other scientists, etc., and use these representations as resources for action.
- How scientists recognize, mediate, and exploit conflicts in goals and disparate frames of reference.

Formalized approaches to answering these questions will be a necessity for developing intelligent machines, but they will also give a stronger foundation to social research. Our approaches to these questions promise to illuminate both collective problem solving, as it occurs in scientific enterprises and other human social situations, and to provide the basis for artificially intelligent machines capable of reasoning in uncertain and dynamic worlds populated with other intelligent agents.

Artificial Intelligence and Real War. **Chris Hables Gray**, University of California, Santa Cruz

The commitment by the U.S. military to artificial intelligence (AI) can be understood as the culmination of a process, crystallized during World War II, for the utilization of formal systems and logics to transform the "art" of war into a "science."

Computer-dependent technologies have unbalanced the distinction in war between the rational and the emotional, recognized from Sun Tzu and Clauswitz to Morris Janowitz and contemporary military theorists.

U.S. strategy is based on two assumptions: 1) That it can achieve battlefield superiority through high technologies dependent on computerization; 2) Humans cannot be trusted to fight efficiently on the hyperlethal modern battlefield. Where machines cannot fulfill the mission alone they must be intimately connected to humans.

The success of applying formal logics by the U.S. military to strategy and tactics, as opposed to logistics, ballistics or cryptanalysis, has been poor. Current AI projects will probably be equally unsuccessful. Many computer scientists argue that they are impossible because of the very nature of computation.

The domination of "rationality" in U.S. military discourse means that powerful and dangerous emotions are being repressed. While latent, they may exercise real and unfortunate effects on military doctrine and decisions through denial and aggression. As can be seen in current military doctrine that makes nuclear war winnable, and that assumes that the holocaust of a near-nuclear battlefield will be manageable if humans are replaced or controlled by intelligent machines.

This system, the repression and elimination of the emotional coupled with continual technological innovation and permanent military mobilization, has been called: technological war, technowar, war without end, and pure war. Perhaps it isn't war at all.

Implications include decreased effectiveness for the U.S. military, continued illusions about managing war, the dehumanization-cyborgization of the human self-image and, most serious of all, increased changes of war and conflict beyond war.

Engineering Knowledge: An Anthropological Study of an Artificial Intelligence Laboratory. **Diana Forsythe**, Stanford University
This paper presents research in a Silicon Valley Artificial Intelligence laboratory that produces expert systems. Expert systems are built by knowledge engineers, specialists in the task known as "knowledge acquisition." Typically, this involves the following steps: 1) collecting information from a human informant (the "expert") and from documentary sources; 2) ordering that information into rules and constraints relevant to the operations that the prospective system is intended to perform; and 3) designing or adapting a computer program to apply these rules and constraints in performing the designated operations.

From an anthropological standpoint, this is an extremely complex sequence of tasks. Not only must the knowledge engineer understand the expert's decision-making process, but that information must then be rendered into machine-usable form. The sequence requires the knowledge engineer to undertake two distinct translations. First, the knowledge engineer must construct his own model of what the expert does, combining his understanding of the expert's explicit model with implicit knowledge and contextual information that the expert may not have articulated. Second, the knowledge engineer's model must then be translated into the language and categories of a particular computer environment in such a way that the information can be used to generate useful statements about the ex-

ternal world. Not surprisingly (from the standpoint of a social scientist), knowledge acquisition is widely seen as a problem by the AI community.

However, knowledge engineers seem to have quite a different perspective on *why* knowledge acquisition is a problem. Typically technically-oriented and positivist in approach, they tend to perceive the process of knowledge acquisition as conceptually straightforward. They view it as difficult not because of the nature of knowledge itself, or because of the complex double-translation process described above, but because it typically takes place through the medium of extended face-to-face interaction between knowledge engineer and expert. This interactional process is seen as inexact and difficult to control. In the belief that automation will "get around" this problem, knowledge engineers look forward to the day when knowledge acquisition can be automated. This search for a technical solution to a possibly non-technical problem provides insight into the world view of the scientists involved.

10:30 am-12:30 pm

Session 13 Communication Patterns in Science

The Communication of Research-Related Information in Genetics. Katherine W. McCain, Drexel University

The dissemination of research results and research-related information (experimental materials, instruments, and methods) is central to the progress of genetic research. Free and open exchange of research methods and materials has been a tradition in genetics in the last decade. However, reports of secretive behavior, refusals to provide research materials, and conflicts concerning the "ownership" of biomedical experimental materials have become more frequent. Information gathered in an ongoing series of interviews with experimental geneticists is used to construct a set of basic scenarios for the exchange of genetic materials. The discussion emphasizes factors which affect individuals' behavior and expectations as information requesters (choice of information source and communication channel) and information providers (refusals, use restraints). Choice of source and channel is directed by the degree of social or professional relationship and knowledge of the stage of the source's research, as well as the similarity between the two researchers' work. Sources' willingness to provide (and associated use constraints) also speak directly to issues of ownership rights to intellectual property--both acceptable (protection of graduate students' research) and unacceptable refusals (competition, secretive behavior) are reported. The size of the research "window" may be related to the general secretiveness or openness of a research specialty, as may the potential commercial value of the materials themselves.

The Integration of Cognitive Neuroscience. Edward Manier, University of Notre Dame

In a recent issue of *Science and Technology Studies* 4(1986): 16-28, I presented a simulated dialogue involving representatives of two scientific laboratories with different approaches (cell biological and psychological) to the phenomena of associative learning. Questions concerning the relationship of cell biological and cognitive studies of the learning process are of general interest because they touch on a contemporary version of a perennial philosophical question, "Can mental processes be reduced to molecular processes?" The philosophical issue of reduction displayed by that question is inextricably

linked with the social processes of intra-field and inter-field communication and consensus formation. I agree with Mulkay (1985) and Gilbert and Mulkay (1984) that when one examines the varied details of the form and content of these social processes, customary assumptions concerning consensus formation in science come unglued. My 1986 article dramatized this situation by juxtaposing research programs which had not yet confronted each other in any public inter-field context.

At the July, 1987 meetings of the Experimental Psychology Society at Oxford University, a day long symposium, "Parallel Distributed Processing: Implications for Psychology and Neurobiology," will bring together the neuroscientist and the psychologist who were the central characters of my dialogue, together with other leading neuroscientists, psychologists, and computer modellers. The participants in this symposium have formally been invited to note that

"It may . . . be useful for . . . criticisms of these ideas to be brought out into the open. For example, Rumelhart and McClelland's (1985) approach has been criticized on the grounds of mixing 'different levels of explanation' (Broadbent, 1985). Kandel and Hawkins (1984) intriguing idea that 'sensitization' might be part of the 'alphabet' of associative conditioning is not without its problems in accounting for contingency effects in classical conditioning."

Since reciprocity in the exchange of criticism and requests for reformulation of investigative priorities, research design, and explanatory goals is often absent from formal scientific literature, the present paper will describe and analyze its role in a symposium specially designed to elicit and facilitate such inter-field communication. The British symposium's success in this respect will be compared with materials in LeDoux and Hirst (1986) *Mind and Brain: Dialogues in Cognitive Neuroscience*.

Session 14 The Social Structure of Scientific Fields

Factors Affecting Scientific Accomplishment. Sally Brewster Moulton, Boston University

Scientific accomplishment typically is defined in terms of creativity, productivity, or innovation and may be analyzed at the individual, institutional, or national level. Significant social, economic, and political outcomes depend upon the frequency and quality of scientific achievement. It is thus a major modern concern, not only for scientists and their respective fields, but also for educators, policy makers, and others who fund and benefit from the results of scientific endeavor. Not surprisingly, the importance of scientific accomplishment has stimulated a number of studies devoted to identifying the factors involved in its occurrence. Factors that have been investigated include the characteristics of the scientist, the research setting and process, the developmental stage of the particular discipline, and the scientist's adherence to the values of science.

The study presented here is both a review of recent research concerning factors that affect scientific accomplishment and a structural analysis of this field of research. Data on the content and structure of the field were obtained by reviewing, coding, and analyzing a subset of the scientific accomplishment literature published in English language journals between 1979 and 1983. Data gathering and analysis were patterned on the work of Folger and Gordon (1962) and Levy, Cairl, and Gallagher (1968). Current findings are combined with the results of these earlier investigations to trace the development of this field since the early 1950s. Trends in the identification of factors affecting scientific accomplishment and the research methodology used in such studies are discussed.

Age and Scientific Productivity: Field and Organizational Differences. Sharon G. Levin, University of Missouri-St. Louis, and Paula E. Stephan, Georgia State University

The aging of the American scientific community has generated renewed interest in the popular hypothesis that science is a young person's game. To date, the empirical evidence bearing on this question is limited and largely inconclusive. Part of the difficulty stems from the inadequacies of the models and methodologies used to investigate the relationship between age and scientific performance. But a major stumbling block has been the lack of a comprehensive database containing measures of productivity for scientists in academia as well as in industry, government, and other organizations. To remedy this, we have assembled such a database by linking by computer the journal publication data contained in the Science Citation Index (SCI) with the largest and most comprehensive longitudinal study of scientists in the U.S., the biennial Survey of Doctorate Recipients (SDR). This paper uses this output-enriched database to explore the age publishing productivity relationships found for scientists trained in four fields of science—physics, geology, biochemistry, and animal and plant physiology—who are employed in different sectors of the economy.

First, issues arising in measuring scientific output are discussed and four quantitative measures of output for large datasets are proposed. Next, the computer procedure used to link the productivity measures with the SDR records is briefly described and issues concerning the reliability of the resulting output counts are addressed. Our verification procedures and statistical analyses suggest that the match technique produced output measures with an acceptable level of reliability. Finally, age publishing productivity profiles by field and sector of employment are presented. Preliminary analysis of the data indicates substantial differences among sectors and fields. These results call into question whether generalizations can be made as to whether science is a young person's game. Furthermore, they suggest that extreme care must be taken in drawing generalizations about the scientific process from case studies of science that limit their scope to one or two fields in the academic sector.

Session 15 Interests, Negotiations, and Controversies in Science and Technology

Scientists' Professional Interests in the Acid Rain Controversy. Stephen C. Zehr, Indiana University

Studies of technical controversies identify the importance of scientists and their expertise as ideological resources for adversarial interests and decision-makers. In a case study of the acid rain controversy, this paper explores how the presentation of scientific expertise in political decision-making contexts serves the professional interests of scientists. Scientists have a professional stake in the acid rain controversy. Because of the social, political and scientific controversy over acid rain scientists have been provided with resources for research. Their appearance in Congressional hearings and other political arenas is an opportunity to reinforce the need for continued funding of acid rain research and to reestablish the epistemic authority of science. To succeed in defending and enlarging their professional stake, scientists' knowledge-claims must be perceived as both *objective* and *useful*. However, the adversarial nature of the acid rain controversy makes it difficult to achieve both simultaneously. To be useful, knowledge-claims must support the position of an interest group or set of interests. But scientists are then open to the charge that their knowledge-claims are not objective because they are tailored to support specific interests. Scientists solve this dilemma through the flexible presentation of

scientific knowledge, making it appear both objective and useful.

This paper illustrates the flexibility of knowledge-claims about acid rain through an exploration of scientists' presentation of the relationship between scientific knowledge and political policy issues. Scientists present scientific knowledge about acid rain as both compatible with politics and different from politics to achieve the two goals of utility and objectivity.

Controversy in Science: The Case of Dioxin. Rosa Haritos, Columbia University

In today's high tech society attempts at quantification have come to be perceived as vital to the character of the scientific enterprise as well as critical for its success. Scientific knowledge plays a larger role in social discourse and enhances rationality in human affairs. In the context of public policy, use of scientific information is often justified by the belief that it furnishes the "basis for a more informed and enlightened forecasting and action by both public and private agencies." (National Academy of Sciences, 1969). However, adoption of quantifiable methods have not been devoid of controversy; the idea of making quantitative indicators of anything at all fascinates some people and repels others as being dangerous or absurd. Purists claim that only when you measure something do you really know what you are talking about, while others hold that quantification distorts the full, natural sense of things by confining them in a straightjacket. Further, specific attempts to extend quantification from the scientific to the social sphere have, at the very least, encountered an ambivalent response. One explanation for such an attitude can be traced to the fault of uncertainty.

Uncertainty is a common factor in most public policy decisions that require technical risk. Trying to remove this veil of uncertainty is not an easy task. Information necessary to decide a case may not always be obtainable. If available, information may not always be complete. Necessary research needed to resolve a situation may take too long to complete or simply is too costly. Further, additional information for substantiating a more rational approach to policy decisions is often diminished if such decisions involve fundamental value conflicts: as is often found in a clash between individual and group interests. Conflicts arise because 'facts' of a case require extrapolation from incomplete information. Conflicts may arise when experts from diverse disciplines review a case and develop different perspectives of the technical problems. Such differing views derive largely from their professional training which influences experts on how to choose methodologies and how experts draw conclusions and make recommendations. Construction and deployment of scientific information is therefore unlikely to be guided by intellect alone, since the making of public policy often involves many competing and incommensurable goals. Ultimately decision-makers must assess all conflicting recommendations and reach a decision on what they feel is 'acceptable' or 'safe'.

This paper aims to identify problems associated with the development and utilization of scientific information. It examines a specific case: the controversy surrounding the toxicity of 2,3,7,8-tetrachlorodibenzo-p-dioxin, or more commonly referred to simply as dioxin. In trying to reach a conclusion, four areas or subtopics are examined, each topic constituting a controversy concerning the related symptoms associated with dioxin exposure. This last area concerns itself with causal linkage and extrapolation of symptoms from the animal to the human population.

After reviewing the events of this case and describing problems conflicting perspectives can cause, this paper will make recommendations on how to improve regulatory decision-making. It may also shed some light on the unintended effect of casting doubt upon the integrity of the scientific enterprise.

Session 16 Quantitative Science Studies

Discipline Integration, Citation Patterns, and the Plate Tectonics Revolution. John Stewart, University of Hartford

Scientific revolutions should have profound impacts on not only the theories used within a discipline, but also on other aspects of scientific research, such as the degree research in different specialties is integrated together and perhaps the bases by which recognition is distributed within the discipline. This article uses data on a random sample of geoscience articles published in 1963 before plate tectonics was developed and an equivalent sample published in 1970 after plate tectonics was widely accepted. Bibliometric techniques, such as bibliometric coupling and co-citation analyses, are used with information on the articles' references to develop several measures of discipline integration, which are then compared for the 1963 and 1970 data. All measures show increased levels of "integration." The second set of analyses will compare the relative importance of article and author characteristics as predictors of the number of citations to the articles published in 1963 and 1970. It is expected that article content ("cognitive" factors) will be more important predictors of citations than author characteristics ("social" factors) both before and after the revolution, but the revolution will result in an increase in the relative importance of "cognitive" factors compared to "social" factors. The results will be interpreted through two different perspectives on science: the "functionalist" concepts of universalism and the Matthew Effect and the "relativists" emphases upon the importance of social and cognitive resources for presenting persuasive communications in science.

Intellectual Foci in Research Programmes: the Use of Word and Co-Word Linkages Among Articles as an Indicator. Loet Leydesdorff, University of Amsterdam

Co-word linkages among documents have been proposed as an alternative to citations (and co-citations) as an indicator of how documents are related. In this study I examine co-word linkages among a set of biochemistry documents which were subjected to a citation analysis in another context. The co-word structures in the titles of both these document sets are compared with that in all other documents of the *Science Citation Index* in the same period. (A similar analysis of the words used in the abstracts of these documents has also been made, to check whether the analysis should start from titles or abstracts.)

The structure of co-words in the titles of the citing documents significantly resembles that of the co-word structure of the cited documents. These co-word structures reflect the internal intellectual structure of the document set and the intellectual work organization of the related groups of authors, as can be validated against the results of an independent document analysis. On the other hand, citations are much more specific when searching for the links between a given document set and its external bibliographic environment.

If co-word structures are specific to intellectual lines, they provide us with a tool to describe finer structures within particular specialties, i.e., with a means of identifying cognitive structures at a lower level of aggregation than the previously explored journal-journal structures.¹ However, in this connection co-words should be considered as a special case of co-occurrences of words, and hence the co-word matrix should be replaced by the matrix of documents over words which—since it is asymmetrical—can be analyzed both in terms of the words themselves (the substance of the intellectual focus) and in terms of the documents in which they appear. In this way, documents

can be sorted according to words in their titles, intellectual foci within specialties.

The fruitfulness of this approach will be illustrated.

¹L. Leydesdorff, 'Various Methods for the Mapping of Science,' *Scientometrics* 11 (1987) 291-320.

A Validation Study of Bibliometric Indicators: The Comparative Performance of Cum Laude Doctorates in Chemistry. A.J. Nederhof and A.F.J. van Raan, University of Leiden

The validity of bibliometric indicators as a monitor of the impact and usefulness of scientific research was examined by comparing the scientific performance of cum laude and non-cum laude degree holders in chemistry. An analysis is made of the productivity and impact of graduate students from five years before their graduation to four years afterwards. It was hypothesized that peer review outcomes and bibliometric assessments would render mutually supportive results. Alternatively, sceptics of both peer review and bibliometric approaches would predict no significant differences in impact and productivity between cum laude and non-cum laude graduates. Also, predictions based on, among others, the Ortega hypothesis and the Matthew effect were tested. Data were collected concerning the productivity of 237 graduate students in chemistry as well as of the impact of their work outside their university. The design of the study makes it highly unlikely that a number of potential biases, such as ceremonial citations, influence results.

Results indicate that the number of citations per paper produced by cum laudes doubled or tripled those of non-cum laudes in the third and second year before graduation, and continued to exceed significantly those received by non-cum laudes until one year after graduation. However, two or three years after graduation, the impact per paper was no longer different for both groups. Cum laudes were significantly more productive than non-cum laudes from three years before graduation to two years after that event.

Little evidence was found in favor of the Ortega hypothesis and the Matthew effect. The data compiled well with the hypothesis that the quality of the research project is the most important determinant of both productivity and impact.

The results indicate that oeuvres judged to be important and of high quality by local peers, as witnessed by the relatively rare award of a cum laude degree, are also cited more extensively by nonlocal scientists than work which is locally judged to be of relatively less importance and less high quality. The data offer support for the concurrent validity of both the peer review process as well as for bibliometric indicators.

Session 17 Visual Dimensions of Science

Practical-Representational Activities in Science: The Artful Work of Astronomers at Hi-Tech "Imaging Facilities." Michael Lynch, Boston University, and Samuel Edgerton, Williams College

This study is a collaboration between a sociologist and an art historian, and concerns the methods scientists use to make visual representations of their data. We believe that the images that scientists create are more than pictures of natural objects, since their visual features depend upon the careful and selective use of available instrumentation, and their composition and interpretation can be influenced by taken for granted conventions on what looks "right" or "natural". In order to discover whether this belief was well founded, and to substantiate it with ethnographic detail, we conducted interviews and observations at two sites. Both sites are "image processing facilities" used for astrophysical research where photographs and electronic data are digitally represented, displayed on TV monitors and analyzed with specially designed computer hardware and soft-

ware. The equipment enables users to vary the color schemes, contours and contrasts of their visual data.

The flexible way in which digitized images can be composed and rearranged enables scientists to easily design images for specific audiences. By observing groups of scientists manipulating the visual features of digitized images and discussing the merits of particular compositions, we are able to discern what they consider contextually significant.

Astronomers acknowledge that they deliberately try to make "nice" or "pretty" pictures for public presentations, but they insist that the relevant consideration for using images in a disciplinary context is "seeing the physics." When we examine what this means in practice, it seems that "seeing the physics" consists in variety of techniques for showing and hiding image constituents. These include practices for "finishing" an image: practices of "cleaning" the image of artifacts, of "smoothing" selected constituents, of enhancing and exposing contrasts to better "show the physics." It is apparent that such practices constitute resolution of figure-ground relations, in a hands-on, instrumentally mediated and collectively enacted process.

The Visual Presentation of Theory and Data: A Cognitive View. Ronald N. Giere, University of Minnesota

Graphs and diagrams are a prominent feature of many scientific communications, whether within formal publications or as part of more informal presentations. Yet, except for Martin Rudwick's 1976 study of "visual language" in 19th Century geology, this aspect of scientific communication has until recently received very little attention from the science studies community. Philosophers of science, in particular, have generally thought of theories and data primarily in propositional terms, so the philosophically important relations between theory and data have been analyzed as relations among statements.

Recently Michael Lynch and Samuel Edgerton have investigated artistic aspects of visual materials in science while Bruno Latour has emphasized their role in forming widespread alliances among scientists. While not rejecting these aspects of visual presentations, I should like to emphasize their role as aids to individual scientists in judging the relevance of data to theory and in choosing among rival theoretical models. I am thus directly challenging the philosophical doctrine that judgments about theory or data are primarily propositional in nature. I also suggest that visual presentations cannot play the alliance building role emphasized by Latour unless they first act on the cognitive faculties of individual scientists.

These theoretical points will be illustrated by examining the development of particular forms of visual representation used by geologists during the "revolutionary" decade of the 1960s.

Graphs and Photos as Mediating Devices Between Environmental Practice and Public Discourse. Roger Krohn, McGill University

In a project directed to articulating the implicit research practices of limnology, a biological field and laboratory science, observation has required a recasting of the official understanding of science. Other observers have argued that research methods are better seen as 'tinkering', or improvisation, but all elements of the research process, bio-emotional, cultural, organizational, and environmental, must be recast as well.

Biological field research practice is an opportune research site, requiring the articulation of implicit aspects in order to reconstruct the work in sociological terms.

Before recognized significant work there is an earlier 'promising' or 'interesting' observation or theme. Before a solved problem, we find a less formulated 'sense of problem' and before that a suspicion, curiosity, or other 'arousal'. For the cultural element, behind the published graph or photo is a series of 'rough' or 'working' graphs or photos, and behind that was a visual image of what such a graph or photo might, or should, look like,

which allowed the selection, cropping, scaling, interpretation of the informative, workable ones. And behind the visual image lies the direct vision of the environment. It is this last step of tracing back to the implicit that proves the most difficult, in fact has typically been declared or assumed impossible by sociologists, but will be attempted in this paper.

The first translation of environmental materials to cultural traces is often implicit even to practitioners, and thus appears to be non-social. But examination shows everything we do in examining bits of our environment is interactive with cultural and social elements. Studying highly instrumentalized sciences has allowed us to overlook this first translation, and to beg the question of the relation of the environment to what we say about it. In limnology one deals constantly with water, sediments, plants, and animals, and much of the first counting, measuring, weighing is done in full view.

Words and Pictures: A Comparison of Visual and Verbal Representations of Natural Phenomena. David Gooding, University of Bath

Recent studies of visual and other non-verbal sources of scientists' construction of nature have drawn attention to the importance of visual modes of representation of phenomena. However, it is not clear how visual modes interact with the more familiar verbal forms of description and argumentation. This talk presents examples of their independence and of their interdependence and argues the importance of the interaction of verbal and visual modes of understanding to the construction of new renderings of phenomenal domains.

The invention, development and maturation of images and concepts of the magnetic field theory is taken as a case study in which we can see how scientists draw innovative representations from a wider culture of images and practices and how both visual and verbal images become accepted as representing real phenomena. In particular, I want to consider situations in which images have functioned in ways that words and symbols could not and suggest a theory of the interaction of these two modes of thought and representation.

If time permits, I will also examine the further translation of electro-magnetic images into fully-linguistic entities (i.e., theoretical concepts) and consider how a theory of imagery may clarify the notion of "style" in science.

Session 18 Perspectives on Science Policy

Radon: A Case Study in Scientific Knowledge and Science Policy. Leonard A. Cole, Rutgers University

Determining when public policy should be aimed at technical activities or scientific phenomena that might threaten the public's well-being is at the core of risk research. Increased concern about the issue is reflected in the burgeoning literature on risk assessment and risk management. Radon is a naturally occurring radioactive gas that is suspected of accumulating in millions of homes across the country. How this suspected hazard should be handled highlights the risk assessment/management dilemma.

With emphasis on experiences in New Jersey, this paper suggests that current calls by public authorities for action on radon seem premature. The argument is based on a scientific, technical, and political assessment of the issue. The paper critically examines claims that radon in the home poses a danger to millions of citizens; it reviews uncertainties about techniques that measure and supposedly reduce the gas's concentration in buildings; and it questions the value of attendant governmental policies.

Despite a long history of information about radon and its hazards, a surprisingly thin body of knowledge exists about the

gas's geographic concentrations, health effects on homedwellers, and ability to reduce accumulations in residential and other structures. Nevertheless, federal and state officials have been urging with increasing frequency that homedwellers test for radon and have concentrations reduced below a specified "action" level. A framework for analysis is proposed that recognizes the need for a better inventory of information, including anticipated consequences, before policies are pronounced.

Is EUREKA Strictly Non-Military? **Johannes M. Becker**, Philipps University

The European Research Coordination Agency (EUREKA) was created in 1985 by the socialist French government as a response to the North-American SDI.

After four summit-conferences European industry and university-institutes collaborate within over a hundred projects financed by their own sources or by governmental investments.

EUREKA-projects realize research-work of two or more European countries in for example biotechnology, energy, traffic, transport, new materials, information and laser.

For scientific workers confronted with strong tendencies of militarization within their sciences the question is to be answered how to let EUREKA become a non-military project:

- high-tech projects can often be used for military purposes;
- there are several French and West-German interests to strengthen European cooperation in military spheres;
- the disappointment for the West-European countries concerning the non existing SDI-cooperation with the USA is the reason for the rapid progress of the EUREKA-idea in 1986 and 1987;
- for the peace movement with its interest in a strictly non-military project there are three objectives:
 - to establish a system of democratic control by the scientific workers, the engineers and their trade unions;
 - to open EUREKA to neutral countries which do not have an interest in a militarization of EUREKA;
 - to vote for the participation of the socialist countries.

Changing Policy Conditions and their Effects on the Dutch Research Community. **Jack Spaapen**, University of Amsterdam

Since 1982 Dutch university research is largely financed through a system called 'Conditional Finance.' In this criteria of scientific and socio-economic relevance were introduced to form the basis for future allocation. Before 1982 research funds were allocated through enrollment.

The Conditional Finance System can be seen as part of a more general attempt by government to reform the university structure to make it more accountable, productive and relevant. To achieve these goals the system introduced new mechanisms for evaluating and programming research. Although in general the academic community recognized the need for more accountability and efficiency, the system was criticized by many. It was expected to have several unintended consequences such as a greater bureaucracy, less autonomy for academics in planning their own research and discouraging multi-disciplinary projects and new initiatives.

The paper will focus on the effects of 'Conditional Finance' on the research process and the ability of academics to determine their own research-future.

We studied various disciplines at various universities and in both 'spheres' we found differences in the ways in which the system was handled. It is interesting to see which consequences this may have for the setting of research-priorities, a policy-item which only since recently became immanent in Dutch higher education.

Through a survey conducted amongst 3000 academics we tried to measure effects on the research-process through three

intermediate' variables: (i) changes in evaluation-procedures, (ii) changes in research-orientation and (iii) changes in interaction patterns between researchers. Results of the survey will be presented in the paper where we try to answer the question whether this system will become a relevant research-context, in the sense that it influences degree of cooperation, time spending etc.

The State-centered Dimensions of Science Policies, and the Internationalization of the Science. **Vittorio Ancarani**, Universit di Torino

In a direct or indirect way scientific and technological growth has a big impact on the increased volume of transactions in many areas of the international relations and activities, growing linkages across borders, appearance of new organizations and actors other than states participating in international arena. These changes are directed toward what Keohane and Nye call "condition of extended interdependence" and Rosenau "transnationalization of world affairs."

Even if the level of global interdependence is not the same across countries, and effectiveness of international actors are quite various in different issues areas and even if the ultimate answers to the nature and direction of these changes are not clear, what it seems sure is the decline of the state-centered view in international relations and politics.

In a companion paper (Strasburg, 1986), I have discussed some implications of these changes concerning the role of science and experts in the international policymaking, and particularly in the growing problem area generated by the transnational impact of science and technology.

The purpose of the present paper is to focus on the obstacles and resistances stemming from the dominant State-centered view in international relations affecting government capabilities to fully understand national science advantages and appropriate policies in international science cooperation and competition.

The paper assumes that two main sets of constraints affect the government management or control of science: the first belongs to the international cooperation and competition, and the second one attains to the domestic scenario, in which dominate the consensus building.

The paper will try to discuss how government should be able, at the same time, to attain strategic goals and to protect the community-wide interests.

Swedish Science Policy and the Two Cultures. **John Hultberg**, University of Goteborg

Science is usually seen as the guiding principle for acquiring knowledge while artistic expressions or the humanities are seen as expressing either wisdom or personal experience. There have been many discussions whether this really is the case or not. One of these discussions was the debate between C.P. Snow and F.R. Leavis in the end of the 1950's and the beginning of the 60's. This debate came to be known as "the two cultures," where the question was whether science really should be seen as the path to the future, while literature mostly didn't want the future to arrive at all.

In Sweden there has been a similar discussion in the middle of the 1980's about the two cultures. In the 1980's the concepts of science and the humanities change key; in the 1950's the ideological battle was to promote science, in the 1980's the debate is how to promote the humanities as part of an industrial revitalization. The debate of the two cultures changes with the idea of the high-technological society, here the humanities suddenly become an interesting investment object through which new markets can be opened.

The Swedish science policy proposition 1987 can be seen as one example of how to look upon the relations between "the two cultures" where the aim has been to look for the utility of the humanities according to technological developments and stan-

dards. In the case of C.P. Snow the integration is dealt with through education. What I want to promote is the idea of knowledge (both scientific, technological, and humanistic, artistic) not only as a utility, but also as a critique, where literature, as one example, can be seen as a way to understand science and society (as Snow in fact has shown with his novels), and technology as a cultural activity.

Session 19 The Sociology of Technological Design, II

Accidents, Technological Systems and the Allocation of Blame: The Train Crash at Hinton, Alberta.. **Gary Bowden**, University of Toronto

On the morning of Saturday, February 8, 1986, a westbound Canadian National freight train collided with an eastbound VIA Rail passenger train. A Commission of Inquiry blamed the accident on operator error. These findings were criticized by the United Transportation Union (representing conductors and brakemen) and by the New Democratic Party (one of Canada's major political parties). In essence, the critics claim 1) that human error was never proven and 2) that the possibility of mechanical failure was not disproven.

The paper utilizes the accident, the Report of the Commission of Inquiry and the subsequent controversy as a vehicle for examining aspects of the public policy process. It is shown that the Commission's conclusions about the cause of the accident and the associated allocation of blame were not forced upon the Commission by the existing evidence. The Commission's findings are more accurately viewed as a social construction aimed at organizing the facts into a coherent package. The position of the Commission's critics represents an alternative social construction. Each construction emphasizes the importance of certain facts while minimizing or discrediting other facts and testimony. Neither construction successfully organizes the entire spectrum of facts and testimony presented before the Commission.

If the Commission's findings on the cause of the accident were not forced upon them by the evidence, why did the Commission reach the conclusion that it did? The paper argues that two major factors influenced the choice of construction. First, the operators lacked the social, economic and political power of the other major participants (i.e. rail companies, government regulators, etc.). Thus, they were easier to blame than the other parties and, once blamed, lacked the resources to seriously contest the Committee findings. Second, blaming the operators provides an easier and more economical "fix" for the problem than a finding that the accident was caused by a design or equipment failure.

The final section of the paper compares the above analysis to contemporary research in the sociology of scientific knowledge dealing with the closure of scientific controversies.

Fashioning an Acceptable Technology: Biodegradable Synthetic Detergents in the United States. **William McGucken**, University of Akron

One of the conclusions I reached in my recent book (*Scientists, Society, and State: The Social Relations of Science Movement in Great Britain, 1931-1947* (1984)) is that all members of society and not just scientists and engineers are responsible for the uses to which science and technology are put. (This is how British scientists and engineers argued in their own self-defense during the 1930s.) Accepting this conclusion, the question then is how that responsibility is met in practice. The question may be explored through the case-study approach and this is the path I have followed in selecting synthetic detergents.

My study sketches the origins of detergents and their widespread substitution for soap in the immediate post-WW II per-

iod; discusses the foaming problems in sewage and water-treatment plants, rivers, and drinking water caused by the most commonly used surfactant, alkylbenzene sulfonate (ABS), and their analysis; considers the detergent industry's response to the problems; examines the parallel British and German experiences; looks at the actions of the U.S. congress with respect to water pollution by detergents; and finally relates how the problems were solved c.1964 by the production of a biodegradable substitute for ABS. I conclude with a consideration of the forces that led to the introduction of biodegradable detergents.

The Social Construction of Three Ultracentrifuges (About the Evolution of Dinosaurs and Dolphins). **Boelie Elzen**, University of Twente

The Scot-programme Recently a new perspective has emerged in the field of technology studies: the 'Social Construction of Technology' program (SCOT).¹ Some of the focal points in Scot are:

Artefacts are being considered as social constructs. Artefacts as such do not exist; various actors will have different definitions of what the artefact 'is'. Furthermore the actors are considered to be members of so-called 'Relevant Social Groups' (RSG); the member of such a group share the same set of meanings with respect to a specific artefact and will share a definition of the artefact. This constitutes the socially constructed artefact.

Because different RSG's have different definitions of an artefact they see different aspects of it as problematic. Also they will have different strategies for solving these problems. This constitutes a variety of possible lines of development in which social factors play an important role.

—In order to understand the developmental processes of technological artefacts we need to analyze both the success and failure of artefacts in a symmetrical way. Success and failure are categories that should be explained.

—A 'thick description' of various historical cases is needed in order to identify relevant factors in the development of technological artefacts. The SCOT approach is applied to the study of a variety of artefacts which have in common that they all are called 'ultracentrifuge'. In the final section of the paper some theoretical claims will be made which are supported by material from these case studies.

*The development of ultracentrifuges*² The Swedish chemist T. Svedberg constructed the first ultracentrifuges in the mid-1920's. These were used by him as a research tool in colloid and protein chemistry and the scientific results obtained were widely acclaimed in the scientific world.

The American experimental Physicist J.W. Beams started constructing ultracentrifuges in the early 1930's. He aimed at making artefacts, usable for scientific research in general. One of his lines of development resulted in an ultracentrifuge which could be used for the same type of research Svedberg was doing.

The American colloid chemist J.W. McBain developed his first ultracentrifuge in the early 1930's as a cheap alternative to the Svedberg machinery. He claimed that his apparatuses could produce the same results as Svedberg's while costing only 1% of those. He published various papers on research done with them.

Svedberg's ultracentrifuges were never copied by others. In the 1940's they were produced commercially but only a few were sold. The McBain types were also marketed unsuccessfully. The Beams ultracentrifuge was produced commercially in the late 1940's and bought by many scientists in the field.

¹See e.g.: T.J. Pinch and W.E. Bijker, 'The Social Construction of Facts and Artefacts: or How the Sociology of Science and the Sociology of Technology might Benefit Each Other', *Social Studies of Science*, Vol. 14 (1984), pp. 399-441. See also W.E. Bijker, T.P. Hughes and T.J. Pinch (eds.), *The Social Construction of Technological Systems, New Directions in the Sociology and History of Technology* (Cambridge, Mass: MIT Press, 1987, forthcoming).

²A more detailed description of two of these case histories, using the SCOT perspective, is given in B. Elzen, 'Two Ultracentrifuges: A Comparative Study of the Social Construction of Artefacts', *Social Studies of Science*, Vol. 16 (1986), pp. 621-662.

How Do Diagnostic Images Become Meaningful? **Stuart S. Blume and Bernike Pasveer**, University of Amsterdam

Wilhelm Röntgen's announcement of the 'x-ray', at the beginning of January 1896 took the world by storm. New medical possibilities were rapidly envisaged: to the extent that Reiser speaks of the 'intoxication' of the medical profession (Reiser 1978). The x-ray, he tells us, "obliterated one distinction between the outer and inner spaces of the body—both were now susceptible to visual examination". In a short space of time the new technology was deployed for a wide variety of medical purposes, including in the diagnosis of tuberculosis. In the succeeding 90 years the original Rntgen technology has been much improved, combined with the computer, and now competes with many other diagnostic imaging techniques. A recent article in the *National Geographic* magazine describes some of the more 'miraculous' of them. The development validation, diffusion (constitution) of these instruments suggest a variety of interconnected questions for the social studies of science. Each in its turn, is potentially an instantiation of the power of technology-in-medicine. Yet the images it produces have to be validated, have to be accorded authoritative meaning. A new language of diagnosis, a new nosography, has to be established, such that the new representations are accepted as 'normality' and the various 'abnormalities'. In the case of many recent imaging technologies a particular concern has been the diagnosis of cancer of the breast. At the turn of the century one kind of strange new image came to be accepted as signifying tuberculosis; today another kind of strange new image is accepted as signifying cancer of the breast. How do these representations acquire their meanings, their power? Has the process changed historically, from the time of Röntgen to today?

Sunday, November 22

8:30-10:30 am

Session 20 **The Power of Science**

Scientists as Policy Influentials. **John M. Logsdon**, George Washington University

This paper presents a framework for understanding the relationship between the U.S. academic science community and federal political and policy authorities with respect to the allocation of resources for the support of academic science. The allocation decisions that are the focus of the paper are those related to "medium" or "big" science, i.e., projects, instruments, and facilities with costs in the millions (or billions) of dollars. The particular questions that are explored are the degree of influence over such public decisions by the scientific community (or its representatives), and the situations in which that influence is likely to be greatest, and least.

The basic finding of the paper is that the systemic character of the government-science relationship is such as to set limits on changes in the patterns of federal resource allocation for academic research. Those patterns will shift slowly, if at all, over time. Major changes in resource allocation patterns will occur only if the basic characteristics of the system are modified.

In particular, stability is maintained because potential conflicts over the allocation of resources are minimized and contained within the scientific community. There is a general agreement among resource recipients to allow top-level scien-

tific committees and the government bureaucracies with which they cooperate to manage the allocation process on the apparent basis of scientific merit as determined by peer review, rather than to appeal decisions made through such cooperation to broader political authorities. Underpinning this agreement is a belief that the interests of academic science are best served by a "corporate" relationship in which government leaders share control over resource allocations for science with scientific leaders in return for the political support by those scientific leaders of the government bureaucracies providing such allocations. This merging of scientific and political interests is the basis for system stability.

Session 21 **Social Organization and Social Change in Scientific Communities**

Bandwagons in Science. **Joan H. Fujimura**, Tremont Research Institute, Stanford University

This paper analyzes factors in the development and maintenance of the molecular genetic bandwagon in cancer research in the United States from the mid-1970s until 1986. A bandwagon is the sum of commitments of many scientists in many laboratories and organizations to one set of approaches to a problem *plus* the cumulative effect of these commitments as an infrastructure for gaining new commitments ("bootstrap"). I first summarize a larger argument for the development and maintenance of this particular bandwagon. I then focus on the construction, marketing, and importing/buying of the theory-technology packages which act as interfaces or bridges between a particular line of research and potential "jumpers-on" in other lines of research. I conclude with discussions about interfaces and about the molecular genetic cancer research bandwagon as a case study for students of social movements and mass behavior.

There are four broad stages in the development and maintenance of the molecular genetic cancer research bandwagon: Package/interface development, marketing and distribution, buying/jumping on, and maintenance through the bootstrap effect.

Problem-solving in basic science is rarely standardized and therefore requires enormous amounts of work to deal with all the combinations of many variables. Under these conditions, standardization of any step in the basic research process is valued highly. The package of theory and standardized technologies provided standardized procedures which reduced ambiguity and consequently the requirements for tacit knowledge, discretion, and articulation in constructing doable problems.

A bandwagon in science develops when one set of problems or one approach to a problem becomes highly doable across laboratories. A set of problems becomes doable across laboratories when scientists succeed in building a package and "enrolling allies" to that package. The *package* includes their *representation* of a phenomenon (a) and an attached set of highly transportable technologies for examining that re-represented phenomenon (a).

In the case of the molecular genetic bandwagon in cancer research, molecular biologists had to re-represent cancer as a disease of the DNA. They also needed to construct the tools with which DNA in higher organisms could be handled, since cancer occurs only in higher organisms. The bandwagon developed when researchers in one line of work convinced (enrolled) researchers in other lines of work that they could use their molecular genetic package to construct doable problems. In this situation, then, the theory-technology package acted as an interface between (at least) two lines of work.

The Carbon Dating of the Shroud of Turin: The Closure of a Debate. **H. Laverdiere**, University of Bath

During the next year the Turin Shroud (TS)—a piece of cloth held by some to be the burial cloth of Jesus Christ—will be dated, using the radiocarbon technique. The TS has given rise to a lot of studies, scientific tests, debate, etc. However the carbon dating test is often seen as the ultimate test, the one which will settle the matter.

Carbon dating is usually thought of as a very precise scientific test. Yet in this case it is entangled with emotional and religious questions. Thus its differences and its similarities with other episodes of scientific negotiation should be of particular interest. The carbon dating of TS also provides a unique opportunity to follow a controversy from the very beginning to the end.

This paper will deal with both the history of the debate about the TS and the expected impact of the radiocarbon test: the rhetoric of argumentation; the definition and use of the 'scientific ethos'; the attempts to interpret opponents' claims as based on magic rather than science, and how this 'accusation' is used equally by both sides in the debate; the relative value and soundness assigned by the participant to the different specialties involved in the controversy; arguments which can diminish the dating test's importance (contamination problems, date of the image rather than the cloth, etc.); the possible challenging aspects of the pressure from 'outsiders' for a technique more empirically than theoretically grounded, etc.

Results of interviews with experts who are to conduct this test will be presented and few hypotheses concerning the future of the debate will be expressed. Later papers will discuss further aspects of the test as the controversy unfolds.

Session 22 Comparative and Historical Methods in Science Studies

Comparative Historical Sociology of Science. **Ellsworth Fuhrman**, Virginia Polytechnic Institute and State University

This essay explores the unique contributions that comparative historical sociology of science can make to science studies. Unlike some current research strategies comparative historical sociology of science does not claim uniqueness on the basis of any particular brand of history, methodological gadgetry or theoretical framework. However, it does claim to ask a different set of research questions which are not answerable within the confines of discourse analysis, ethnostudies of the laboratory, or traditional citation studies. Moreover comparative historical sociology of science provides interesting answers to a number of questions being asked by researchers in other areas of science studies.

Episodic Inquiry or Long-Haul History? **Thomas F. Gieryn**, Indiana University

Recent work in science studies has drawn attention to the contextual contingency of "meaning." Whether analysis focuses on the meaning of a belief (say, a scientific fact or theory) or on the meaning of science *tout court*, the methodological rule now is *local*. "Local" has both spatial and temporal dimensions; however, most studies use local only in its spatial sense: the meaning of a statement is specific to a designated laboratory. My paper proposes "episodic inquiry" as a second methodological rule for interpretative studies of science, in order to highlight the temporal dimension of "local." Attempts to do "long-haul history" of science, according to my prejudices, often fail by assuming universal/definitive meanings of "science" that transcend temporal episodes. The liabilities of long-haul history and the virtues of episodic inquiry are illustrated by an empirical analysis

of shifting boundaries between natural and social science in (U.S.) Congressional debates over the creation and later reorganizations of the National Science Foundation.

11:30 am-1:30 pm

Session 23 Rhetoricians on the Rhetoric of Science

Rhetorical Considerations in the Construction and Justification of Science Policy Decisions: The Case of the Cambridge Experimentation Review Board. **Craig Waddell**, Rensselaer Polytechnic Institute

In the fall of 1976, the Cambridge City Council created the Cambridge Experimentation Review Board (CERB) and asked the eight non-scientists who comprised the board to decide whether or not recombinant DNA experiments proposed by Harvard University would have any adverse effects on public health within the city of Cambridge.

In their four-month effort to inform themselves about the relevant issues, CERB members read scores of documents, heard over seventy-five hours of testimony from over thirty-five experts, and spent over twenty-five additional hours in planning and deliberation.

Although arriving at their decision was a complex process involving many variables—such as presuppositions and the social, political, and economic context—as Sheldon Krinsky, a CERB member, says in *Genetic Alchemy* "One may be aware that external factors shape the nature and distribution of viewpoints in a scientific debate while also finding value in a structural analysis of the key arguments that enter into a controversy." (1982, 8)

During the heated exchanges of the June and July public hearings, where the audience was anonymous, not accountable, and not especially well informed about the issue, appeals to emotion and authority were relatively effective. However, when delivered to a small, responsible, accountable, informed audience—an audience that not only had to make a decision, but to *defend* it—appeals to reason proved more effective.

Even if they were predisposed to decide for or against the experiments due to strong, underlying presuppositions, CERB members could not allow such a decision to surface without rationally reconstructing it. Thus, the arguments they were presented with served two functions: in the context of discovery, they helped to form the board's decision; in the context of justification, they helped to legitimize it. In the context of justification, CERB members were especially concerned with appeals to reason.

Arguing in Different Forums. **Jeanne Fahnestock**, University of Maryland

Archaeologists who study the prehistory of North America generally agree that the first humans migrated from Siberia, but they disagree about the earliest date for this migration. While the canonical view dates the first migration at 12,000 years ago, some archaeologists argue for a date twice as remote. At the heart of this controversy is a fundamental disagreement about whether a particular class of bone artifacts provides evidence of human use. Rhetoricians using a classical scheme of invention would recognize this controversy as a first stasis confrontation about the very nature of evidence in a discipline. As such, this dating controversy offers a case study of a field in the throes of accepting or rejecting a major realignment.

Recently the magazine *National History* has provided a forum for North American prehistorians. While their magazine articles addressed to non-specialists may seem to be simple informative pieces, they can be read as arguments in the context of the

current controversy. As such, they reveal persuasive appeals which are only implicit in disciplinary contexts such as appeals to the professional status of the arguer and his years in the field, or to the narrative account of his own persuasion as his doubts were gradually overcome. When addressing a broader audience in a popular forum, the scientist can also portray collaboration as corroboration and personalize the whole endeavour of research. This study compares the tactics of persuasion used in articles on the artifact controversy that have appeared in learned journals like *Science* with those that have appeared in *Natural History*. The rhetorical differences created by addressing different audiences reveal two systems of justification.

Discourse as Text—Discourse as Practice. Carolyn R. Miller, North Carolina State University

This paper will consider the general question of what sociologists and composition/rhetoric specialists can learn from each other about their common subject, discourse. The question has become of interest recently with converging developments in the two disciplines. Discourse can be considered, in Aristotelian terms, as *poesis* (production) or *praxis* (practice), the former representing the approach typical of composition studies, the latter representing that typical of sociological studies of discourse. A productive approach focuses on discourse as text or artifact and requires the form of reasoning Aristotle called *techné*, skill or technique. A practical approach to discourse focuses on symbolic action as conduct and requires the form of reasoning Aristotle called *phronesis*, prudence or judgment.

The differences between these two approaches are illustrated by an examination of the discourse by means of which a decision was made by the manufacturer not to change some operating instructions for reactors of the type that subsequently experienced the accident at Three Mile Island. Had the revised instructions been in effect, the accident would probably not have happened. The memoranda exchanged between two divisions within the manufacturing firm, when read as produced texts, suggest that the decision was based on inadequate attention; when read as practical conduct, they suggest that the decision was based on incompatible beliefs about the appropriate justification for action. This examination suggests the advantages and limitations of treating discourse as practice and as production and suggests what each approach can contribute to the other.

Session 24 Academic, Industrial, and Military Relations of Science & Technology, II

The Social Construction of Militarism: Deterrence, Coercion and Strategic Thought. Jim Falk, University of Wollongong
Militarism is both an ideology and a set of social relations. This paper deals with the ideological role of a range of central militarist concepts in mediating the discourse of not only opponents but also proponents of nuclear disarmament. It is argued that a reliance on a shared set of assumptions, arguments, categories and metaphors in the nuclear debate plays a critical role in systematically limiting the boundaries of what appears to be rational, and consistently distorting conclusions about what steps towards peace are possible.

Many of the concepts in question play a central role in, and in part derive from, strategic theory—a body of analysis with a long history which has in recent years become the framework upon which the discipline of Strategic Studies has been constructed. The use of these concepts is not confined to the strategic studies community or academic specialists but, most notably in debate over nuclear weapons issues, is shared by politicians and media analysts, and may be found in the discus-

sions amongst the broader community. Whilst strategic theory has received some critical attention (Green 1966, Booth 1979, O'Neil 1981, King 1982, Gray 1982), the role that its categories play in structuring contemporary thought has not received the systematic attention it deserves. In this paper, the underlying conceptual structure and assumptions of Strategic Studies, their role within contemporary discourse over nuclear weapons, and the historical reasons for their adoption are examined. Debate surrounding the Strategic Defense Initiative will be used as a case study.

One central theme which will be developed is that the discourse which derives from strategic theory plays an important social role in allowing the interests of particular institutional actors in the nuclear debate to be advanced (for example, by providing categories for use in legitimating funding claims, a rationale for technological development within the weapons research and development community, and in general a form of discourse through which the competing interests of different agencies can be mediated). It is argued that in general the discursive structures and strategies adopted are of particular utility in serving the perceived interests of nation states, but act to obscure alternative perspectives which may well be more conducive to success in the search for strategies for developing a safer world.

Crystallization of a Strategic Alliance: American Physics and the Military in the 1940s. Paul K. Hoch, Warwick University

This paper will discuss the origins and interactions of the various scientific, industrial and military elites that were party to the war-time alliance, and how which have since extended their collaboration into the generations of the Cold War. It also considers war-time and post-war debates about the terms and vocabularies of this alliance—the predominant preoccupations with 'secrecy' and 'security'—and their effect on the relative positions of the alliance participants, as well as—indirectly—on the rate and direction of scientific advance over the past two generations, the general shape and functions of the American university in the Cold War era, and the role of the military and the National Security state in the creation of Big Science in this period. The security investigations of the 1950s are seen as having long-term effects on particular universities, and more pervasively on the general functions and ideologies of American university and American science—in particular the continuing search for National Security through a science ultimately geared to providing ever more sophisticated weapons systems—a rationale for big scientific appropriations which has a continuing relevance in the era of the Strategic Defense Initiative. This was also the climate in which the dominant American liberal arts college gave way to the 'multi-university,' professors as teachers were partly obscured by professors as researchers and consultants, and the traditional liberal arts curriculum began to give way to 'hard' sciences and engineering as a factor of national import (and relative factor size and support). The most important universities in this period became those with the largest government and military contracts. Attention is also paid to the social context in which these developments occurred, and especially the social position (and funding) of American universities—which differs fairly substantially from those in Europe.

Session 25 Scientific Practice and Scientific Knowledge

Can Experiments Be Used to Study Experimental Science? Michael E. Gorman, Michigan Technological University

This paper begins by reviewing laboratory studies designed to determine under what conditions subjects and scientists falsify

on problems that simulate scientific reasoning. The implications of these studies for philosophy-of-science are discussed. Results indicate that confirmation is a useful heuristic during what Laudan calls the pursuit phase of inquiry; Popper's disconfirmation is a useful heuristic later in the inference process, when tentative hypotheses that produce positive results have been selected for further testing. Experiments show that the ability to apply these heuristics depends on the individual's mental representation of the problem. In some cases, these representations can come to resemble the unfalsifiable 'hard core' assumptions that underly Lakatos' research programs, particularly when there is the possibility of random error in the data.

These experimental and philosophical observations are related to two historical case studies. The first is Tweney's analysis of Michael Faraday's cognitive processes. Like experimental subjects, Faraday began by searching for confirmations in the pursuit phase of the inference process; unlike most of them, he was then able to justify his own hypotheses using a disconfirmatory strategy. But, despite disconfirmatory evidence regarding the identity of gravity and electricity, he never abandoned his 'hard core' assumption that all the forces were unified.

The second case is Carlson's description of the etheric force controversy. In 1875, Edison discovered what he thought might be a new force; he did a few confirmatory experiments and then called in the newspaper reporters. It was left to Thomson and Houston to disconfirm Edison's hypothesis by showing that the new force was simply an instance of the familiar phenomenon of induction. This case illustrates the role of competing research teams in disconfirmation.

Finally, this paper makes connections between sociology of science and the experimental, historical and philosophical work discussed so far. Social factors played an especially important role in the etheric force controversy; the participants were not simply 'rational actors' but were using what Latour and Woolgar call investment strategies to advance their careers. Moscovici's experimental work on minority influence is used to explain the difference between this controversy and the more recent 'discovery' of the Prion.

The paper concludes with specific suggestions for how psychology experiments can complement the work of philosophers, historians and sociologists of science and technology.

Constructivism and Structuration Theory. **Rob Hagendijk**, University of Amsterdam

The paper investigates the ways in which 'constructivist' analysis deals with the ordered character of science as a social phenomenon. The two branches of 'constructivism' to be compared in the paper are labelled "micro-constructivism" and "macro-actor theory." The exemplary author for the first branch is Karin Knorr-Cetina, for the second branch Bruno Latour is representative.

Both approaches will be shown to give unsatisfactory answers to the questions of what actors consider negotiable and what they regard as not open to dispute, what they accept as knowledge to be 'taken-for-granted' or as 'given' institutional arrangements. Due to their radical 'constructive' point of departure, both varieties of constructivism have difficulty with the idea of a social structure or historical processes which go beyond the actors involved in it. Nevertheless elements of a social order beyond the interaction-episode must be invoked in order to say something about why certain actors succeed where others fail to get their knowledge-claims accepted. Generally, constructivists, whether Latourians or microconstructivists, do not give a satisfactory account of accepted knowledge, established research practices and taken-for-granted routines in relation to the constitution of new knowledge claims and their fate.

The second part of the paper discusses structuration theory

as a possible alternative for the 'constructivist' approaches dealt with in the first part of the paper. Structuration theory as it has been developed by Anthony Giddens covers most of the critical objections of 'constructivism' to established sociological traditions while offering a new perspective on a range of issues in social studies of science. To what extent does structuration theory provide a basis for dealing with the questions raised above? The paper will explore a number of themes in Giddens' theorizing in order to investigate its potential relevance for social studies of science as well as some of its problems.

Can the Durkheim-Mauss Thesis be Resuscitated? **Warren Schmaus**, Illinois Institute of Technology

Durkheim and Mauss hypothesized that primitive classifications of things in nature reproduce classifications of people in societies. Bloor has attempted to provide this hypothesis with new evidential and theoretical support and to generalize it to include the systems of classification of modern science. In this essay I argue that because Bloor in the end fails to provide such support, he gives us no reason to accept this generalization of the Durkheim-Mauss thesis.

The most important empirical evidence claimed by Bloor concerns the assumption of the passivity of matter in Boyle's corpuscularian philosophy, which is said to "reproduce" the social and political passivity of Boyle's social class. Ironically, even if this highly debatable interpretation of this historical episode were correct, Bloor's use of this evidence is still subject to Durkheim's methodological suspicions against the use of evidence that does not result from a crucial experiment.

Theoretical difficulties arise from Durkheim's use of arguments that rely on the identification of classificatory concepts with "collective representations" that result from the "fusion" of individual mental imagery. Cognizant of the philosophical problems with such a picture theory of meaning, Bloor attempted to replace it first with Hesse's model of a socially negotiated network of concepts, and more recently with Wittgenstein's identification of the meaning of a word with its use in a social context. However, these theories give us no reason to believe that a society's classification of things in nature *must* reproduce its social categories. Furthermore, Bloor fails to recognize that Wittgenstein's arguments against the identification of meanings with individual mental representations apply with equal force against their identification with collective representations. Wittgenstein was opposed to any kind of representational theory.

In sum, Bloor has provided insufficient empirical and theoretical warrant for adopting the Durkheim-Mauss thesis in the sociology of science.

Science as a Self-organized System—A New Approach to the Dynamics of Scientific Theories. **Wolfgang Krohn and Gnter Koppers**, University of Bielefeld

The theory of self-organization is used to "construct" a model of science as a social system which explains the dynamics of theories. The model has three advantages:

1. The relationship between science and its environment is re-formulated. The classical distinction of "internal" and "external" is no longer used as a theoretical description of observation. The system itself makes this distinction and regulates this difference along the boundaries thus created.

2. In contradiction to the so-called laboratory studies which show how an individual scientist behaves in laboratory we can list the mechanisms through which the individual action of scientists is translated into collective action. We can show how action in a research group leads to rules about action, which influences further actions. The central theoretical category here is that of the eigenvalues of operationally closed processes (Heinz von Foerster).

3. The theory of self-organizing systems finally opens up the

possibility of modelling the social construction of scientific knowledge. The classical approach is based on the non-sensical opposition between "social" and "rational" and forgets that the "rational" itself is a social construction: Only the system is in a position to decide which social behavior can be treated as rational. The key to the modelling of the social construction of scientific knowledge is to be found in the self-reference of the system of science, that is in the fact that not only the knowledge produced but also the production of knowledge is thematized: That means that knowledge (new facts) and the method of their production (construction of knowledge) are subject matters of social interaction.

1:30 - 3:30 pm

Session 27 Political Dimensions of Science and Technology

Audiences and Problem Choice in Toxicological Research. **Peter Groenewegen**, University of Groningen, Netherlands. In fields like toxicology scientific researchers can choose from a number of contexts when proposing their research. Within these various contexts different audiences are addressed. Audiences fulfill a role in scientific research by enabling scientists either directly or indirectly to secure resources for their research. The necessity in fields like toxicology to involve nonscientific audiences directly is addressed. Subsequently the effect of this attention on the development of research is assessed.

This is done by taking the research from one institutionally defined group of scientists. The manner in which they assessed the needs of the outside groups is addressed in my study by following strings of problems on which research has been done. Either frequent shifts or stable research interests are explained by asking the following question:

—how do scientists deal with constraints arising in outside context?

These constraints can be either cognitive elements, institutional task settings or audience interests.

Political Creativity in Science: The Antebellum State Geological Surveys. **Stephen Turner**, University of South Florida. Forms of patronage, the primary medium of the influence of interests on the growth of knowledge, are not only mutable, they may be readily created. In this paper I discuss a period of intense political creativity within U.S. science. By the Civil War, virtually all of the states had legislated some kind of geological survey: several of these surveys were significant intellectual successes; several were economic and political successes; several were fiascos of the first order.

The careers and political methods of James Hall and David Dale Owen, the two great figures in this period, are described and analyzed. The process of geologists borrowing political appeals and survey designs from one another and of legislators acting on precedents established in other states and of geologists tailoring surveys to the political needs of particular states is examined. In some cases the work included agricultural and soil surveys, in others detailed mapping, while in others the work was aimed primarily at international scientific prestige—very successfully, in the case of one of the major beneficiaries of the State survey system, Hall, a political manipulator of great talent and few scruples. In the course of this political development, certain characteristic features of the American mode of political patronage of science emerged.

Modeling the Grasslands. **Chunglin Kwa**, University of Amsterdam

Within the framework of the U.S. contribution to the International Biological Program (1968-1974) a large program was launched under the name of 'Analysis of Ecosystems.' The U.S. was divided up into five large ecosystems or biomes, and models were to be developed that would accurately describe or mimic the behaviour of the biomes. This program has been extremely important for American ecology, because it doubled the amount of funding of ecological research through the National Science Foundation.

The paper focusses on the development of one of the biome programs, the Grassland Biome Project under the leadership of the late George Van Dyne, then Professor at Colorado State University. This project was the biggest, and in several aspects the most ambitious of the five biome projects. The production of simulation models of the total ecosystem has been a far more explicit goal in this project than anywhere else, and it was successful in producing those.

Being by far the most centralized project, the Grassland Biome featured a work organization typical of the 'big science' undertaking, more prominent than the other IBP projects. The total ecosystem models that were to be developed formed the justification for the strong role for central management in the Grassland Biome. In a sense, the work organization repeated the structure of the grasslands model that it aimed to develop.

The introduction of a 'big science' management approach in a field otherwise highly fragmented proved not without difficulties. The Grassland Biome suffered a major managerial crisis, resulting in the resignation of George Van Dyne as principal investigator of the Grasslands study, a few months prior to the official termination of the U.S. INP (30 June 1974). While it is obvious to all participants of the Grasslands study that Van Dyne's personality has been the single most important factor leading to his eventual resignation, the crisis in the Grassland Biome study also brought into light major differences in opinion on the Biome work organization, and also on what was to be accomplished by ecological modeling and indeed, by the IBP studies as a whole.

The paper will examine the development of the Grassland Biome work organization, as well as its major ecosystem model, and its underlying assumptions.

The Politics of Genetic Diversity. **Lawrence Busch, William B. Lacy, and Jubel Moraga-Rojel**, University of Kentucky

Plant breeding may be divided into four historical periods based upon the techniques employed by breeders: trial and error, experimental approaches, Mendelian genetics, and molecular biology. A central ingredient in each of the four approaches is the availability of a diverse genetic pool from which to select improved materials. Yet, at the same time, breeders consider themselves successful when a cultivar (cultivated variety) that they have developed, is widely adopted. The result of this is the reduction of diversity available in the field. The new molecular techniques are likely to exacerbate this problem, leading to both genetic uniformity and vulnerability.

Genetic uniformity refers to the tendency for monocultural production to be not only of the same crop but of the same variety. Essentially, breeders use the current best cultivars or lines as parent stocks for the next generation of cultivars and then repeat the process. The net effect is a continuing destruction of the genetic base. Genetic vulnerability refers to the threat of the extinction of landraces (traditionally cultivated varieties) and wild relatives of crop plants. Since breeders must have variation with which to breed, no matter what the technique, genetic vulnerability threatens the entire breeding enterprise.

Over the last several decades, a system of germplasm banks

in which seed is stored have been developed around the world. Their creation gives rise to a number of issues including 1) *in-situ* vs. *ex-situ* maintenance, 2) management, 3) maintenance, 4) identifiability, and 5) useability. In general, scientists see these issues as essentially technical in nature. In contrast, politicians and even some administrators see the issues as largely normative. Moreover, the problem of germplasm maintenance forces scientists with very different backgrounds to associate with each other. In particular, it forces close association between "discursive" and "instrumental" sciences, between taxonomists and molecular biologists. The relations between the two groups, however, is a marriage of convenience. Disputes over classification schemes are frequent as is the (illusory?) search for the perfect, universal classification system. The implications of these disputes for our understanding of science and science policy are examined.

Session 28 Perspectives on Science, Technology, R&D, and Policy

How Much Can Science Contribute to Technology? David Collingridge, Aston University

Discussion of this question is difficult because even if a theory was used to develop a technology, there is no way of assessing whether its use was marginal or central. Such a measure is proposed here. Where a scientific conjecture is central to the technology, the technology would be expected to fail before the conjecture was developed, and the conjecture would make its success very likely. This resembles Popper's conditions for severity of test. It is therefore proposed to use Popper's measure for severity of test, as a measure for the closeness of a scientific conjecture to a technology. General arguments are then given for expecting the closeness to be always low, a claim which is corroborated from a number of case studies, including early antiseptic surgery, the transistor and the first atomic bomb.

The paper therefore extends Collingridge and Reeve's sceptical account of the role of science in public policymaking, by removing the objection that since science contributes strongly to technology, then it must be possible to employ science in policymaking. Science can, on the contrary, have only the kind of marginal influence over technology which it can have over public policy. Further, the paper contributes to the more general discussion of science and technology, by showing that the fact that technology works may not, after all, show that the scientific conjectures it involves are successful.

New Materials: Governmental Policy for Stimulation of R&D and Monitoring the Possible Consequences. Philip Vergragt and Peter Groenewegen, University of Groningen

In Europe, the USA and Japan a worldwide race appears to be going on in the field of new materials: polymers with new combinations of properties, technical ceramics, semi-conductor materials, optical materials and composites. These materials are claimed to be "tailor-made" for specific purposes, mainly in space, aviation and the automobile industry, but also in other consumer products. The consequences of these developments could be quite large: large-scale substitution of existing materials by new ones, affecting the quantity and quality of labor, the environment and energy consumption. Many of these consequences have been hardly investigated in terms of their positive or negative effects.

In this paper, we present some results of our investigations

in Britain and the Netherlands into three related themes: the stimulation of research and development in this area by governments; the effects of this stimulation on the actual R&D processes and the effectiveness of the governmental policy; the possible social effects of development of these new materials. A related theme is the question of how much social groups, which may be confronted with the consequences of these new developments—trade unions, consumer organizations, environmental organizations—know about the new developments, and how much their influence on the developments could possibly be.

The results of these investigations will be interpreted in terms of problem definitions, research lines and audiences. 'Problem definition' is a notion borrowed from M. Callon: it implies that social reality and scientific developments are shaped by negotiations between actors each with their problemization of reality. 'Research lines' is a concept which has been introduced in our earlier work on social construction of innovations; in essence, it means that at the intermediate level of the research group there is an inherent continuity of the research process, which is shaped by problem definitions and critical (research) events. 'Audiences' is a notion introduced by one of us denoting the groups external but highly relevant to the research process which serve as reference points for strategies of scientists competing for external resources.

The aim of the paper is to show the socially constructed, negotiated character of new technologies, with new materials as an example, and to open up possibilities for feeding-back foreseeable consequences of present developments into the research process. Links will be made with theories of innovation and of social construction of technology.

The Social Construction of Military Space Technology in West Germany. Johannes Weyer, University of Bielefeld

In Europe and especially in West Germany plans for future involvement in manned space flight as well as military applications of that new big technology are intensively discussed. The paper will analyze the political concept of a European space-power and will assess the financial and social costs of that program. It asks for the civilian need for manned space flight and tries to identify military applications and interests, which lead to the assertion that manned space flight can only be regarded as a 'quasi'-civilian technology, building a shelter-belt around the hard core of military space flight. This thesis may help to explain the rapid shift from 'civilian' to military technologies as in the cases of the shuttle or the space station. The main focus of the argumentation will, however, be the question, how this new technology could emerge through the interaction of different actors and groups with well-defined own interests. The processes that brought the groups involved together and formed a self-stabilizing social structure with own dynamics and own interests will be analyzed, using the concept of self-organization. Transformation of arguments, mutual reinforcement of actor's positions in the respective fields, and adjustment of interests can be regarded as elements of the mechanism that constructs a social network necessary to bring about innovation processes of that order. The growing resistance of that network to public criticism and control and the steady extension of its borders prove the coherence and dynamics this structure has gained. Nevertheless, different legitimation strategies indicate that the public, the peace movement, scientific organizations, and others are still important points of reference for the space-lobby, which may shape the future of manned space flight or even could help to construct other (more peaceful) networks.

Logan Airport (Boston):

Logan Airport is the nearest full service airport to Worcester. Most major airlines provide service there. Traveling by automobile, it is about 1 to 1½ hours from Worcester, depending on stops, traffic and time of day.

Public ground transportation between Worcester and Logan is quite strong. The "Quickway" Airport Limousine Service is essentially a busline which operates between Logan Airport and the Worcester Marriott Hotel (with two other stops on the route) according to the following schedule:

Logan to Worcester Marriott:

Weekday: 7:30 AM— 8:30 PM every hour on the half hour

8:30 PM—11:00 PM every half hour

Weekends: 7:00 AM—11:00 PM every hour on the hour

Worcester Marriott to Logan:

Weekdays: 4:30 AM—7:00 AM every half hour

7:00 AM—9:00 PM every hour on the hour
(no 7:00 PM trip)

Weekends: 5:00 AM—9:00 PM every hour on the hour

Round trip tickets on "Quickway" are \$20
(Schedule as of 7/1/87)

Bus service is also available between Boston and Worcester. While this service may be somewhat faster and cheaper than the Quickway Limousine, it does require that the user take the subway or other ground transportation from Logan Airport to South Station in order to pick up the bus. On the other hand, the bus station in Worcester is located adjacent to the Quality Inn, and would be very convenient for those who elect to stay at this hotel.

Public Ground Transportation:

Worcester is serviced by Greyhound and Peter Pan buslines. There is also limited Amtrak rail service. Taxicab service within the city is generally adequate.

Travel Agency Arrangements:

Young's Travel Service (which is the American Express representative for the Central Massachusetts area) has been briefed on the 4S meeting and the likely needs of its participants. They are currently making arrangements with American Airlines to offer 4S meeting attendees a 40% discount on American's regular coach fare, or a 5% discount on American's lowest available fare. Once the details of this agreement are finalized, Young's will be establishing a special reference number to be used by those attending the 4S meeting who wish to make flight reservations through them. Further information on these arrangements committee shortly, and may also be obtained by contacting Mr. Alden Anderson at Young's Travel Service.

Worcester Center, Worcester, MA 01608 (617) 755-4375.

In addition, Young's will be arranging for rental cars or airport-hotel transfers for those booking flights through them. They expect to be able to offer substantial discounts on the rental cars as well as the "Quickway" limousine service, and will probably set up a table at the Marriott Hotel to serve you.

Young's is also eager to arrange sidetrips for those interested in seeing the area. Plymouth Plantation and Old Sturbridge Village are both interesting places to visit around Thanksgiving time. Trips to the historic sites of Boston are also popular. Those interested in exploring any of these possibilities should contact Alden Anderson at Young's Travel Service.

For the more daring and independent "frequent travelers" (who like to make their own arrangements and yet know they are getting the lowest rate), membership in the "Ultimate Travel Service" is being arranged by Kathy Wilkes, 25 Minuteman Way, Shrewsbury, MA 01545 (617) 842-4416. The way this service works is that one pays between \$60.00 and \$80.00 per year for membership (depending on the level of service), and is then given access to an 800 telephone number through which one can book a *Guaranteed Lowest Air Fare* between cities, hotel rates at 25%—50% off regular (not conference) rates, and 25% car rental discounts. In addition, one typically gets rebates upon returning home and reporting expenses. Message service, emergency cash, insurance, short notice trips and change of plans services are also provided.

Most people recover their initial investment on the first trip and then, if they travel often, do well indeed. There is the "dilemma" of figuring out what to do with rebates (when the university has paid the cost of the trip), but most institutions are very satisfied with the discounts their frugal "Ultimate Travelers" report at the outset of the trip when using this service. Some air travel costs to and from Worcester as indicated by Kathy's sources are listed below. Some of these fares are subject to restriction or require advance bookings, but they illustrate the domestic travel costs an "ultimate traveler" should be able to approach. International flight costs are also available on request.

Washington D.C.—Worcester	\$116.00
Toronto—Boston	 106.00
Chicago—Worcester	 168.00
Chicago—Newark	 128.00
San Francisco—Worcester	 292.00
San Francisco—LaGuardia	 238.00
Atlanta—Worcester	 174.00

For application forms or more information contact Kathy.

TRAVEL INFORMATION FOR THE 1987 ANNUAL 4S MEETING (WORCESTER MARRIOTT HOTEL)

Directions for Highway Travel

From the north:

Take I-495 southbound to Rte. I-290. Take I-290 west to Worcester, getting off at exit 18. Turn right at the end of the exit ramp and continue straight through Lincoln Square (about ¼ mile), crossing Route 9. The Worcester Marriott will be on your right just past Route 9.

From the east:

Take the Massachusetts Turnpike (Interstate 90) to exit 11A. Follow I-495 north to I-290. Take I-290 west to Worcester, getting off at exit 18. Turn right at the end of the exit ramp and continue straight through Lincoln Square (about ¼ mile), crossing Route 9. The Worcester Marriott will be on your right just past Route 9.

From the west:

Take exit 10 off the Massachusetts Turnpike (Interstate 90) at Auburn. Take Rte. I-290 eastbound into Worcester. Get off at exit 17 (Lincoln Square, Belmont Street, Rte. 9) and turn left off the exit ramp. Follow signs to the Worcester Marriott Hotel.

From the southwest (New York City):

Follow I-84 eastbound into Hartford, or take I-95 northbound to I-91 northbound in New Haven CT, and take I-91 to I-84 eastbound in Hartford. Follow I-84 eastbound until it terminates at the Massachusetts Turnpike (Interstate 90). Take I-90 eastbound and follow directions above "from the west."

From Logan Airport (Boston):

Sumner tunnel to Route I-93 south (Fitzgerald Expressway) to the Massachusetts Turnpike west (Route I-90). Follow directions above "from the east."

From Bradley Airport (Hartford/Springfield):

Route I-91 north to Route I-291 (Springfield) to the Massachusetts Turnpike east (Interstate 90). Follow directions above "from the west."

From T.F. Green Airport (Providence, RI):

Route 1 north to route I-95 north into Providence. Take Route 146 north to Route I-290 in Worcester. Get off at exit 17 (Lincoln Square, Belmont Street, Rte. 9) and turn left off the exit ramp. Follow signs to the Worcester Marriott Hotel.

Air Travel Information

Worcester Municipal Airport:

The Worcester Municipal Airport is serviced by Eastern Express, Piedmont and Continental airlines. The best current direct access is on Piedmont Airlines, which flies directly into Worcester from the Baltimore/Washington Airport. The flights depart Baltimore at 12:21 PM, 3:03 PM, 5:40 PM, and 8:48 PM, and are scheduled to arrive in Worcester 70 minutes after departure. Return flights to Baltimore depart Worcester at 7:05 AM, 2:20 PM, 5:00 PM and 7:50 PM. (Schedule as of 7/1/87)

The Eastern Express Shuttle (Bar Harbor) makes connections between Worcester and Boston (Logan) or New York (LaGuardia) according to the following schedule:

Boston to Worcester:

11:40 AM (Monday through Saturday)
3:50 PM (Daily)

Worcester to Boston:

9:45 AM (Monday through Saturday)
11:20 AM (Monday through Saturday)
3:00 PM (Daily)
7:10 PM (Daily)

Worcester to LaGuardia:

7:00 AM (Monday through Saturday)
12:35 PM (Daily)
4:30 PM (Daily)

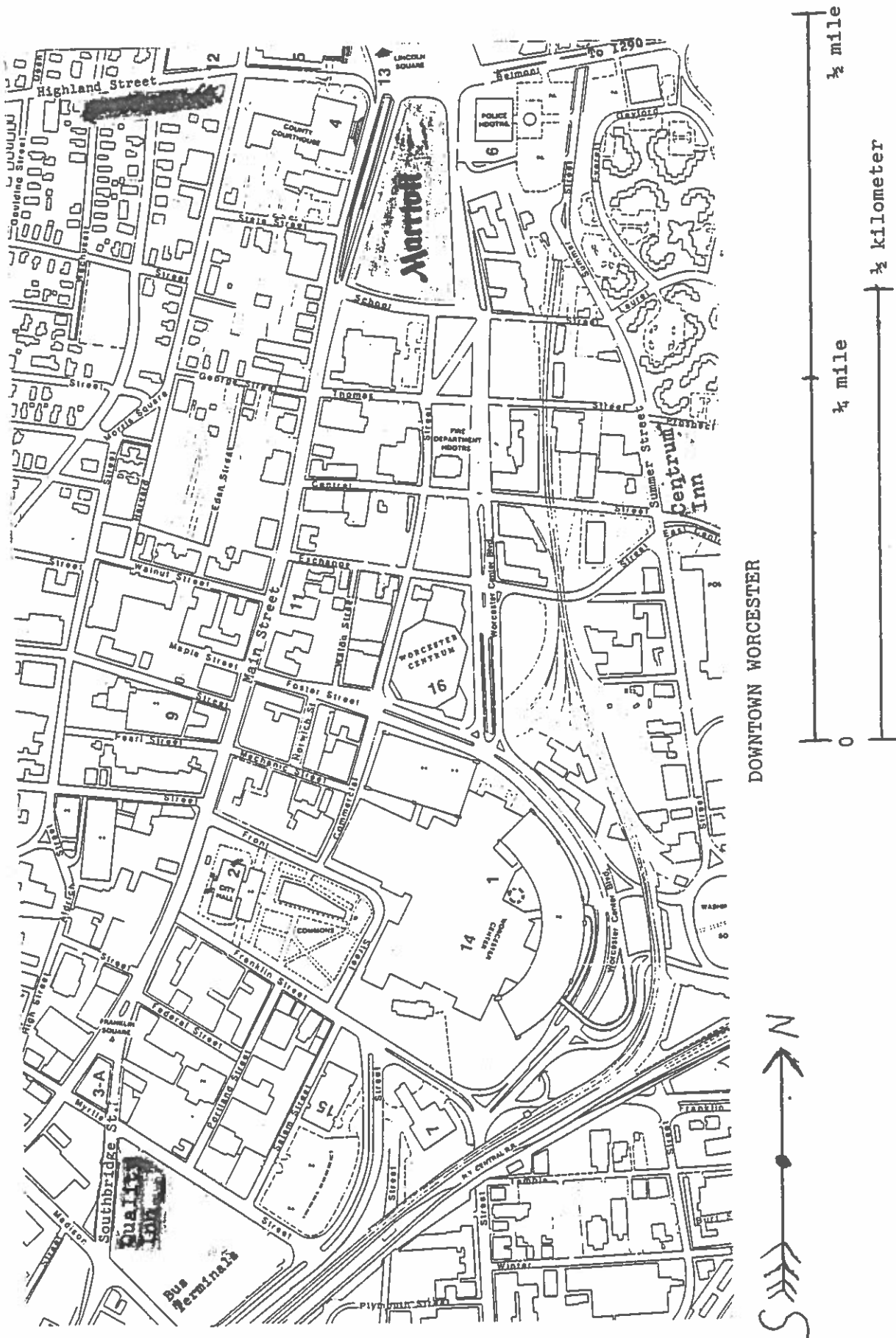
LaGuardia to Worcester:

8:30 AM (Monday through Saturday)
1:55 PM (Daily)
5:55 PM (Daily)
7:25 PM (Sunday through Friday)

Flights between Boston and Worcester take about one half hour, while the flights between LaGuardia and Worcester take an hour. (Schedule as of 7/1/87)

There is no direct connection between JFK (New York) and Worcester. Service is via either Boston or Washington (Dulles).

Continental Airways does not currently have a great reputation, as there are stories of lost luggage and missed connections in the wind. However, as of August 1, Continental will no longer serve New York at LaGuardia, but will provide service into Worcester from Newark, NJ. Their current two trips daily are expected to increase in number, providing improved domestic connections.



LOCAL ARRANGEMENTS AND REGISTRATION INFORMATION FOR THE 12th
ANNUAL MEETING OF THE SOCIETY FOR SOCIAL STUDIES OF SCIENCE

PERSONS PLANNING TO ATTEND THE 1987 ANNUAL 4S MEETING SHOULD CAREFULLY READ THIS SECTION AND RETURN THE ATTACHED REGISTRATION FORM BEFORE NOVEMBER 1, 1987. THIS FORM MAY ALSO BE USED FOR JOINT REGISTRATION OF BOTH THE 4S MEETING AND STS CONFERENCE.

HOTEL ACCOMODATIONS

Those wishing to reserve space in one of the hotels housing the 4S meeting/STS conference attendees should indicate their hotel preference on the registration form. Allocations of hotel space will be made prior to November 1 by the 4S meeting local arrangements committee. After November 1, attendees requesting hotel accomodations should contact the hotels directly. To the extent possible, attendees with professional status will be given preference at the Headquarters Hotel, while those on student status will be given preference at the lower priced hotels. In either case, early requests will have priority over later ones. Efficient utilization of available hotel space will be a consideration, so requests for doubles will have priority over requests for singles which arrive simultaneously. It is expected that the Marriott Hotel (Headquarters Hotel) will fill up first, so those wishing to stay there during the conference should plan to make reservations early.

Worcester Marriott Hotel	: Quality Inn	: Centrum Inn (Best Western)
10 Lincoln Square	: 70 Southbridge St.	: 110 Summer Street
Worcester, MA 01608	: Worcester, MA 01608	: Worcester, MA 01608
(617) 791-1600	: (617) 791-2291	: (617) 757-0400

EAT, DRINK AND BE MERRY:
A GASTRONOMIC GUIDE TO THE 12TH ANNUAL 4S MEETING

An effort was made this year to reduce the number of times people must face the choice of making that time consuming trip out of the meeting site for meals, or eating in an overcrowded hotel restaurant. Hence, tickets will be available for many meals that have been organized in advance.

The festivities begin with a wine and cheese reception Thursday Evening from 6:00 PM - 7:00 PM, prior to the First Plenary Session. This session and reception are being held at the Worcester Marriott Hotel, but are sponsored by Worcester Polytechnic Institute (WPI), a host institution.

Friday meals will also be at the Marriott Hotel.

Friday Noon:

Cahoots," a Marriott Hotel Restaurant which is normally only open at night, will offer an optional Buffet style Chicken & Ribs Barbecue with a Western Theme. There are 120 tickets available at \$13.50 each (including all taxes and gratuities) for those interested. (Those registered for the S.T.S. conference get a half price discount on this meal.)

Friday Evening:

The Joint 4S/STS Banquet will be at the Marriott Hotel Friday Evening from 7:00 PM - 9:00 PM, following a reception at Clark University. (Transportation between the Marriott and Clark University will be available.) Tickets for the banquet are \$20.00. The menu is French Onion Soup, Caesar Salad, Sirloin Steak and Bernaise Sauce, Potato, Vegetable, and White Chocolate Mousse. Those interested in a Fish or Vegetarian alternative should indicate that on their registration forms.

On Saturday, the activities and meals move to WPI.

Saturday Breakfast:

Special groups wishing to have breakfast meetings together will be offered private meal areas for 10 - 30 people. Breakfast will be served at \$4.50/person for those in such scheduled meetings. (There is still room for 3 more groups interested in taking advantage of this opportunity.)

Saturday Lunch:

A special Plenary Meeting Luncheon will be addressed by Evelyn Fox Keller. The luncheon tickets are \$7.50 each. It will be a light meal featuring chicken breast, to be served at 12:45 PM sharp.

Saturday Dinner:

Following the presidential address from 6:15 - 7:15 there will be an optional special event dinner for those interested. A Medieval Manor Style Feast will be held at the Higgins Armory, a Museum of Medieval Armor in Worcester. The "Great Hall" of the armory can accommodate about 100 people for this dinner, which should be a memorable event. The tickets are \$30.00 each, which includes bus fare, taxes, gratuities, entrance fees, hall rental and catering.

This buffet style meal will be served on long banquet tables. The Fare will include Peasant Soup, Manor House Sallat, Henry the VIII Ribs, First Joint of Dragon Wings, Blank Mangere, Medieval Vegetables, Lemonwhyte, Soppes Dorres, Black Perys, Love Apples and Fromage, Aphrodisiac, Monastery Waffles, Ypocras, and Saracren Brew - all in spectacular surroundings.

Malcolm Parkinson, Professor of History (of Science) at WPI and Current President of the Higgins Armory bids all his colleagues in S.T.S. welcome and urges you to come to see his newly renovated "Great Hall." Entertainers are being sought, but Troubadours are hard to come by these days. Any 4S Wandering Minstrels, Jugglers or Jesters who care to volunteer will dine as guests of the Lord of the Manor. Revelers may arrive in costume if they wish. Those wishing to rent one locally should let us know by Oct. 1.

The Sunday activities return to the Marriott Hotel.

Sunday:

A Buffet Style Brunch has been arranged to coincide with the 4S business meeting at the Marriott Hotel. Save your appetite until 10:30 and we will meet for a sumptuous meal of French Toast, Sausage, Quiche or Eggs, and Fruit or Juice. This is an official function and hence a subsidized meal. The cost to the membership will be \$7.00 each.

WPI Student Project President's Awards Banquet:

Those STS Conference attendees interested in attending the WPI President's Student Project Awards Banquet on Wednesday evening November 18th should note their interest on the registration form. The President's award banquet is not normally an open event, but if there is sufficient interest I will ask President Strauss to open it and allow me to sell tickets at the conference.

Meal Refunds:

Requests for meal refunds must be made by November 1 to be guaranteed. After that date, every effort will be made to accommodate those affected by selling their unwanted tickets to others at the door.

Gastronomic note to 4S members coming early for the S.T.S. conference: Joint 4S/STS attendees should note that the STS Conference registration fee includes a "Getting To Know You" Luncheon on Wednesday November 18, continental breakfast Thursday morning November 19, a reduced price for the Thursday STS luncheon, a half price ticket to the Friday "Cahoots" luncheon, and the printed proceedings of the conference. The only additional food cost for a Joint 4S/STS registrant is the optional Thursday STS luncheon.

Information for Contributors

Science & Technology Studies is a multidisciplinary journal which publishes research, commentary, and reviews. As the official journal of the Society for Social Studies of Science, it seeks to foster exchange and communication among a variety of individuals and groups concerned with the development and dynamics of science and technology, including their relationships with politics and society.

Contributors are urged not to be constrained by the format of traditional scholarly research articles in their submissions. Contributions may also take one of the following forms: letter to the editors, commentary on a published article, opinion column, or synthetic review essay.

Contributors should submit three copies of their manuscripts, double spaced throughout including footnotes. Two review processes are available: blind and open. If a blind review is desired, all indications of the author(s) should be removed on three of the copies. If open review is desired, the author's identity will be disclosed to the reviewers.

Contributions will be reviewed for interest to a multidisciplinary audience concerned with science and technology, for communication with that audience, and for quality of data and dependability of information. Verbatim comments of all reviewers will be forwarded to authors. If publication is approved, the names of the endorsing referees may be appended to the article when it appears.

In deference to the many disciplinary writing styles represented in the Society, our refereeing policy is pluralistic.

References alone, references plus end notes, or end notes alone may be used. In format, the journal follows the *Chicago Manual of Style*. In end note style, examples are:

Jarlath Ronayne, *Science in Government* (London: Edward Arnold, 1984).

Richard S. Westfall, "Science and Patronage: Galileo and the Telescope," *Isis* 76 (1985): 11-30.

Paul Forman, "Weimar Culture, Causality and Quantum Theory, 1918-1927," in *Historical Studies in the Physical Sciences*, ed. R. McCormach, Vol. 3 (Philadelphia: University of Pennsylvania Press, 1971), 1-115.

In reference style, examples are:

Latour, Bruno and Steve Woolgar. *Laboratory Life: The Social Construction of Scientific Facts*. Beverly Hills: Sage, 1979.

Roll-Hansen, Nils. "The Death of Spontaneous Generation and the Birth of the Gene: Two Case Studies of Relativism." *Social Studies of Science* 13 (1983): 481-519.

MacKenzie, D. and B. Barnes. "Scientific Judgement: The Biometry-Mendelism Controversy." In *Natural Order*, edited by B. Barnes and S. Shapin, 191-210. Beverly Hills: Sage, 1979.

Submission of accepted articles on diskette is encouraged, but not required. If you plan to use a word-processor to produce your manuscript, please contact the editor about the prospect of providing your copy on diskette.

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