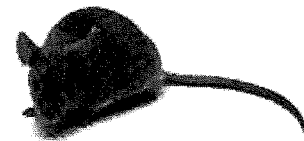


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front and backpage illustrations: logo of Taconic, supplier of Oncomouse, and some of the mice from their transgenic mice catalog



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When Technoscience Rewrites Biology

by Patricia Radin

School of Communications, University of Washington, Seattle

Review of: Donna J. Haraway,
*Modest_Witness@Second_Millennium:
FemaleMan[®] Meets OncoMouse[™],*
Routledge, 1997.

What's the difference between man and
mouse, when both are cyborgs? For that
matter, what's the difference between man and
woman, in a world where technoscience is
rewriting biology?

The fact that these kinds of questions arise is
an indicator of where science and technology
have taken us. "We're talking about whole
new forms of subjectivity here," science
historian Donna Haraway says in a Wired
magazine interview.¹ "We're talking seriously
mutated worlds that never existed on this
planet before." She says we are already
cyborgs -- living creatures melded to machines
-- in the sense that we are dependent on
machines in our daily lives, although not in
the sense that American space scientists meant
when they coined the word 37 years ago to
refer to space travel-enabled bionic creatures.

While Les Levidow and Kevin Robins, in
their readings of military images of the Gulf
War,² and others argue that we as cyborgs
have begun to identify with soulless machines,
Haraway does not necessarily agree. She
declares that women, marginalized in science
as in society, are grateful for new, even
shocking ways to extend our powers. "I would
rather be a cyborg than a goddess," Haraway
declared in "A Manifesto for Cyborgs" (1985),
referring to the propensity of California
feminists to turn into sandaled "earth mothers"
who toil in their gardens and hold primitive
ceremonies. The same theme of female
potency returns, elaborated, in her latest book.
No, Haraway says, women don't need to
remain harmlessly mystical. We need to use
our own powers from whatever sources we
can gather them to create new meanings and

better worlds.

To explicate the book's title: The trademark
and copyright signs are ironic reminders of the
commodification of life in contemporary
society, especially in the United States. The
"modest witness" refers to the widely accepted
convention of scientist as invisible observer,
reporting with supposed objectivity on socially
constructed scientific results. The e-mail
address is reflective of today's
informatics-driven Western society.
FemaleMan is the protagonist of a witty 1975
science-fiction novel by feminist Joanna Russ,
The Female Man, about four women
descended from each other, and brought
together in the same time and place by a time
warp. Three of the women live in the future,
in all-female societies, a twist of plot that
allows exploration of oppressive gender roles
which is, of course, the book's appeal to
Haraway.

A Yale-trained biologist, Haraway focuses in
this book on genetic engineering and
especially on OncoMouse, the mouse
developed at Harvard with funding from
DuPont. Engineered to get breast cancer, for
use as a live research tool, OncoMouse was
patented in the U.S. in 1988 in a
precedent-setting action. Today scores of
different bioengineered mice are for sale, for
prices as high as \$175 apiece. A disturbing
painting of a caged, womanly OncoMouse
crowned with thorns, by feminist painter Lynn
M. Randolph, appears on the back cover of
Modest_Witness. The original painting hangs
over Haraway's desk at the History of
Consciousness Board at the University of
California, Santa Cruz, where she has taught
history of science since 1980.

Haraway looks at gene narratives as
indicators of "gene fetishization," the
clinically abnormal belief that genes have
magical powers to protect us from evil. What

sort of evil? Death? Castration? Indeed, Haraway argues, modern society has castrated itself through racism, sexism and profitability through commodification as the sole arbiter of worth. In fact, modern society craves this fetish, no matter how harmful the obsession, no matter how pathetic it appears to cultural outsiders, no matter how horrifying when embodied as a disposable mouse whose sole value to science is its ability to die painfully of cancer.

Haraway argues, "Belief in the self-sufficiency of genes as 'master molecules', or as the material basis of life itself, or as the code of codes, not only persists but dominates in libidinal, instrumental-experimental, explanatory, literary, economic, and political behavior in the face of knowledge that genes are never alone, are always part of an interactional system. There is no such thing as disarticulated information in organisms, computers, phone lines, equations, or anywhere else."

Although she insists that she is not anti-science, Haraway demands a re-reading of science history and a re-casting of our beliefs about research. She writes: "To produce belief that the boundary between the technical and the political, and so between nature and society, is a real one, grounded in matters of fact, is a central function of narratives of the Scientific Revolution and progress. My goal is to help put the boundary into permanent question." This is not an unusual ambition in science and technology studies; but seldom is the dissection performed with such sharp feminist analytical instruments, or with such stylistic rambunctiousness. The centerpiece of *Modest Witness* is an 11-page table labeled "Universal Donors in a Vampire Culture: Twentieth-Century U.S. Biological Kinship Categories." This offers not a hierarchical tree, but a "bush" of associations to show how meanings have evolved since 1900. For example, the table says that race as a key object of knowledge gave way in the 1940s to population and in recent years to the genome. The table says the "rhetorics of unity and diversity" used to deal with family trees; later, with the "universal family of man;" today, with the "Human Genome Project (ManTM)." The taxonomy is intentionally aphoristic; the

reader is not always sure what it is referring to. Haraway acknowledges that it probably would benefit from hypertext treatment, but she prefers that readers populate the spaces with their own meanings.

One is tempted to label the book, both in style and content, "postmodern," but Haraway says she is inclined towards Bruno Latour's declaration (1993) that *We Have Never Been Modern* - that is, we are "amodern," she says, because the pure rationality that supposedly was a keystone of modernism never really existed. (She is no uncritical Latour fan, though, judging him too immersed in the hierarchical, male-dominated world of science and its methodologies to provide a truly revolutionary analysis.)

Haraway expresses frustration with the lack of interest in mainstream science studies with issues raised by antiracist feminist cultural studies: "Either critical scholars... have not been clear enough about racial formation, gender-in-the-making, the forging of class, and the discursive production of sexuality *through the constitutive practices of technoscience production themselves*, or the science studies scholars aren't reading or listening or both." The failure to engage, she says, has not been the feminists' fault.

Although provocatively pointing out new issues in science studies, *Modest Witness* leaves the reader hungry for suggestions on how to make science and technology development more inclusive. With the 1995 closure of the Americans' own, once-progressive Office of Technology Assessment, the U.S. government seems to be satisfied with profitability as the lone measure of an innovation's rightful place in society. While Haraway correctly senses this, she does not look far beyond the national borders for other models, a common failing of American academics. Only three pages are devoted to "public actors." A brief description of the Danish consensus-conference model of technology assessment, for example, fails to address the probable difficulty in transplanting this process from the relatively small, homogeneous European countries where it has been successful to the sprawling, multicultural United States. Haraway also does not appear to be aware of the Dutch "constructive technology assessment" model, nor the work

of feminists such as Janine Morgall, author of *Technology Assessment: A Feminist Perspective*, (Temple, Philadelphia, 1993).

Because of its scope, *Modest Witness* demands a lot of the reader. It's useful to know something about, for example, spiral dancing, the history of the atomic bomb, vampires, and California popular culture, to say nothing of major arguments in genetic engineering, ethnic studies, feminism, and Marxism.

Stylistically, the book can be jarring. As Haraway circles an idea, her writing voice shifts, sometimes disconcertingly. In the quotation above about her intention to blur the boundary between science and politics, we heard Haraway the traditional science historian. In contrast, we hear the distinctive voice of Haraway the prophetess in this generative re-statement of the same theme: "FemaleMan[®] and OncoMouseTM are both creatures of genetic technologies and, along with the modest witness, of writing technologies... They do not rest in the semantic coffins of finished categories but rise in the ambiguous hours to trouble the virginal, coherent, and natural sleepers."

In his 1992 review of Haraway's *Simians, Cyborgs, and Women* in *American Anthropologist* vol. 94, Latour diagnosed this characteristic writing style as the cacophony of the modernist, postmodernist, and "what I would call a *nonmodernist*," all trying to speak at once. He advised her to "write crisply and never leave the empirical field." Although his technique works superbly for Latour, surely it is shallow advice for Haraway, who is using this multidimensional presentation as a way to enforce her message. She calls her style "diffraction" like the breaking up of a lightstream into colors. It aims to skew ideas so one can examine their components. "Interference patterns can make a difference in how meanings are made and lived," she explains. To bend her writing to the task, she says, "I try to force the words -- like all meaning-making tools to stumble, make a bit of a racket, and generally resist naturalization." Like any racket, this one both calls attention to itself and interferes with the kind of concentration a reader trained in linear communication is accustomed to.

Confronting this dizzyingly broad book, the

reader might well be tempted not to grapple with the analysis, but to enjoy the narrative for its off-beat entertainment value. This would be a grave mistake. Haraway's material is new, her message urgent: It is time to shape the onrushing age of informatics in ways that are more humane, more just, more sustainable than the world we know today. In order to do that, it is vital to understand that there are many ways to write science and technology. Feminists, and others in this world who have not yet had their say, are beginning to take up the task.

Notes

1. Hari Kunrzu, "You Are Cyborg," *Wired* 5:2, February 1997, online at www.wired.com/wired/5.02/features/ffharaway.html
2. Kevin Robins and Les Levidow, "Soldier, Cyborg, Citizen," *Resisting the Virtual Life*, San Francisco: City Lights, 1995

How Can We Educate Green Engineers?

Reflections on Technology, Society and Ecological Modernization

by Andrew Jamison

Aalborg University, Denmark

In Sweden, nature was, from early on, a rather forbidding place - both in theory and practice - harsh and vast and somewhat mysterious, and the task, for science and engineering, was to control it, exploit it, use it effectively, colonize it, bring it under human mastery. Not for nothing has Linnaeus in the 18th century been called the initiator of an imperialist attitude to nature and the instigator of a managerial approach to environmental science, conceiving natural relationships in a mechanical, systemic way. And it was striking when I looked at the historical literature that many other Swedish scientists and engineers had shared this imperial, or mechanical attitude that was so apparent in Linnaeus. As the German writer Hans Magnus Enzensberger has put it in the book *Mausoleum*, his epic poem on the history of modernity, Linnaeus had a different folly from ours: the folly of a classic. 'Any accidental feature must be rejected. Gathering, determining, naming. All obscure similarities were devised to the shame of science. Terminological knives, for the flesh of a blind and writhing world, to peel out the constancy. Inventories, nomenclatures, repertoires. Nature a timeless rectangle, a motionless grid.'

Throughout Swedish history, we find systematizers, taxonomists, system-builders, modellers, both among scientists, philosophers and engineers - and even among politicians. And, of course, in many ways, this mechanical, systemic bent has served Sweden well.

It's quite different in Denmark. There we find - or, at least, I found, when reviewing the historical literature - that the image of the workshop is a recurrent theme in the attitude to nature. The natural environment was to be worked with in a pragmatic way, not through theory or systemic distancing, but by a kind of

organic interaction, or experimentation. We find, already in the middle ages, a practical bent among Danish philosophers, and an organic, experimental relation to nature. My own favorite example is Tycho Brahe from Skone, who almost alone among the great men in the history of science, has his reputation for practical work, for instrument-building and observing rather than for theorizing. Tycho was, of course, also one of the first organizers of science; on Ven he constructed one of the world's first scientific communities, which lasted for twenty years, the practical utopia that provided inspiration for Francis Bacon and all the other theoretical utopians of the 17th century. Brahe was a classmate in Copenhagen with Peder Sorensen, or Petrus Severinus, a follower of Paracelsus, who was one of the first to challenge the idols of the past and urge experimental, practical methods over book learning. In 1571, Severinus captured well the Danish attitude to science: 'Go my sons, sell your lands, your houses, your garments and your jewelry; burn up your books, buy yourselves stout shoes, get away to the mountains and the valleys, through the deserts, investigate the shores of the sea and the deepest recesses of the earth. Be not ashamed to learn by heart the astronomy and the terrestrial philosophy of the peasants. In this way will you arrive at a knowledge of things and their properties.'

In the 19th century, there was Hans Christian Oersted, whose fame again rested on a practical discovery of electromagnetism rather than on a theory: he never could explain how magnetism and electricity were connected. Oersted was an impassioned believer in the practical value of understanding nature's secrets; almost uniquely in the Europe of his time he combined a romantic nature philosophy with a

technically-oriented utilitarianism. He wrote about the spirit in nature and gave lectures to industrialists about the importance of science. It is said that the Carlsberg laboratories were created on his inspiration: JC Jacobsen regularly attended Oersted's lectures as a young man and went on to establish one of the world's first industrial research laboratories, which Danes are still benefitting from.

But even more importantly, perhaps, there was Grundtvig, whose ideas about practical knowledge and the practical importance of myth and vision were so crucial for the modernization of Denmark in the 19th century. Not just the folk high schools, but also, we might argue, the system of technical consultancy that was so important in the development of the dairy and food processing industries, can be said to be derived from the rural populism that Grundtvig articulated. It is at the third of my conditioning levels - the institutional - where Denmark's decentralized, rural based organizational structure linked together the artisanal attitude to technology with an agricultural economic orientation. In Sweden, there developed a number of large engineering firms in the 1870s and industrialization was largely based on the handful of companies that grew up at that time - Ericsson, Asea, Alfa-Laval, Nobel, Bofors - big, export oriented firms that drew on the Swedish mechanical and chemical heritage, and which derived their strength from a basic engineering competence. In Denmark, as I first learned from Esben Sloth Andersen, now at institute 3, there was instead a decentralized structure that developed in the 19th century, with technical consultants spread across the countryside and with the folk high schools and the various technical schools and institutes, like the one that developed here in Aalborg, providing the necessary training for the typically small-scale modernizers. The Danish national style of science and technology was thus quite different from the Swedish; and identifying some of its component parts helped me understand why Denmark could not only survive as an industrial country in the 20th century, but make some significant contributions to global science and technology, in spite of the country's small size and rather limited natural

resources.

These national components of science and technology are evident, not just in the energy movements of the 1970s, but also in the different approaches to technology policy and, indeed, to engineering education that are pursued in our two countries. In Sweden, the emphasis has long been on supporting basic technological research, and on using technology policy as a way to improve the international competitiveness of the big Swedish engineering firms. There has been little attempt by the state authorities to steer or redirect technology, or, for that matter, to assess the social and environmental consequences of technological projects. Mechanization, rationalization - and now information technology - are seen as the main determinants of social change, and the emphasis in Sweden is to follow and support the systemic logic that further technological development requires. As such engineers are instructed in the basic sciences, and in the more abstract and theoretical aspects of engineering. There is little, if any, teaching in technology and society at the Swedish technological universities; the task is to stimulate further technification of the life-world, further technical gadgetry, not to raise questions about which techniques are humanly beneficial or to analyze the social, environmental, and cultural implications of technological change.

The interesting thing is that today we have a situation in Sweden where the industrial structure and its mechanical orientation serve as barriers or constraints for the much discussed ecological restructuring of industrial development - what some of our colleagues in Germany and Holland have started to refer to as ecological modernization. A program of ecological sustainability was one of the promises that Goran Persson made when he took office as prime minister last year, and, in what we might call typical Swedish fashion, he has seen the task in systemic terms. As with the so-called Swedish model in the 1930s and 40s, ecological modernization is to come from above, in the form of a comprehensive plan that is now being prepared by a committee of ministers, advised by a retired master architect, who was also involved in the massive building programs of the 1950s and

1960s.

In Denmark, the readjustment process has been much more gradual, much more piecemeal, much more decentralized. Cleaner technologies, environmental management, pollution prevention have all been tested and evaluated and assessed at what might be termed the grass-roots level. We have an ecological modernization from below, and, as such, it has been somewhat limited and skewed in its impact on the broader society. There is little systematic investigation of the somewhat contradictory social processes of ecological modernization in Denmark, with deep ecologists and green consumers pulling in one direction, environmental managers and clean technologists in another. I hope, in the years to come, that I can bring a little Swedish systematizing to bear on research on ecological modernization and education of green engineering.

For there is, it seems to me, a certain amount of cross national learning that might be useful. I have always felt that the Swedes have much to learn from the Danes about the value of local experiments and technology assessment, while the Danes can learn from the Swedes something of systematization and more all encompassing planning and modelling. My own thinking about technology and society has benefited a great deal from such cross-national learning, more specifically from my interaction with Erik Baark, a Dane who has spent many years in Sweden. Erik and I found ourselves involved many years ago in a rather grandiose Swedish project - an ambitious attempt to compare technology in Europe and Asia - and we tried to find ways to make sense of what struck as a vast array of difference. What we came up with was a schema that has proved helpful in understanding processes of technological appropriation or assimilation.

The idea is that there is a cyclical interaction in all societies between technology and culture, and that, if technological development is to be successful, it is important to take account of the cultural responses, both the cultural critics and critical social movements, but also to traditional aesthetic principles and ways of life, in the formulation of technological policies. The relative success of Japan in the 20th century, for example, can be

explained, at least in part, by the manner in which technological development was adapted to cultural patterns and traditions, to the distinctive combination of the chrysanthemum and the sword, as the anthropologist Ruth Benedict so colorfully put it in the 1940s. The particular nature-inspired fascination with design, and the peculiar aesthetic of the miniature, have combined with the order and discipline of the Samurai tradition to lead the world in consumer electronics. Technology, we might say, was appropriated by Japanese society by using certain central elements of the cultural heritage, not imposed upon the country as an alien force. In India, by contrast, a dualism developed between western and Soviet oriented modernizers, on the one hand, who saw the Indian past as a barrier to development, and traditionalists who rejected modern technology as being inimical to the inward-oriented, spiritual character of Indian culture. It was the modernizing traditionalists who followed Gandhi's lead - those who could mobilize traditional knowledges and technical skills in a selective fashion - who pointed out a possible middle way. In the concepts of appropriate or intermediate technology and in the various environmental movements that have emerged during the past 20 years, the Gandhian approach has lived on, but it has been subordinated to, and often marginalized by, the hegemonic conceptions of the dominant, western trained, increasingly neo-liberal political and economic elite. It was this general perspective that has led me to devote much of my research effort to trying to understand, in a systematic way, the role of the environmental movement in technological change. The environmental movement, I have come to argue, has been a kind of social laboratory for the articulation of new ideas, as well as for experimentation with new technical artefacts. What Ron Eyerman and I have called the cognitive praxis of the environmental movement has played a central role in subsequent processes of scientific and technological innovation. Indeed, I would now claim that the new political discourse of sustainable development and ecological modernization is, to a large extent, a translation of the collective identity of environmentalism into the idioms and life-worlds of engineering, management and

economics.

Our conceptualization can perhaps be seen to have some basis in the Swedish systemic style, even though both of us are transplanted Americans. Our approach has, in any case, been inspired by Jürgen Habermas' early theorizing about knowledge-constituting interests and the different forms of rationality that are to be found in modern societies. What we have tried to do is to identify and describe the knowledge interests that are articulated in environmental and other social movements. In the 1970s, environmental movements integrated an ecological world view or philosophy with an anti-elitist organizational form, and, in some countries, like Denmark, actually developed alternative technologies as part of a movement cognitive praxis. These knowledge interests were an important part of the collective identity of the environmental movement. It was not just political campaigns or demonstrations that made up the life of the movement. Our argument was that the movements provided a new public space for knowledge production, for the working out of new technological projects and criteria, and also for the social innovation of new forms of knowledge production. In Denmark, there was OVE, the organization for renewable energy, and in the Netherlands, there were science shops that developed at the universities in the interface between student activists and environmental organizations. In other countries, this movement knowledge production and diffusion took other forms - in the United States, it encouraged the creation of a congressional office of technology assessment, and in Norway, it led to the deep ecological theorizing of Arne Naess and Hartvig Saetra. The point is that the movements provided a temporary space for experimentation with new modes of knowledge production, that had both cosmological, technological and organizational dimensions.

When we began to explore these issues in the mid 1980s, the environmental movements had begun to change character. New kinds of professional organizations had emerged, such as Greenpeace, and the activism that had been so widespread in the 1970s had begun to fade into the collective memory. There were also Green political parties that had begun to take

part in the more formalized political arenas, and, most importantly, there were new kinds of activities in national and international politics, that were starting to be grouped together under the paradigmatic slogan of sustainable development.

Sociologically, one of the key processes which served to decompose, or break apart, the integrative cognitive praxis of the environmental movement into a disparate cluster of organizations and individuals, was professionalization. The knowledge interests of the environmental movement were transformed into various kinds of professional expertise, which made it possible to incorporate parts of the movement into the established political order, and shift at least some of the members of the movement from outsider to insider status. Some of the alternative technical projects proved commercially viable - biological agriculture, wind energy plants, waste recycling. Some of the alternative visions were taken up by professional philosophers and politicians, and the alternative contexts for knowledge production and dissemination either cleaned up their act and developed more sophisticated communication and information strategies or they eventually ran out of steam.

There were both internal and external reasons for this professionalization process. In the course of the energy debates of the 1970s, the environmental movement had generated within its own ranks a new range of expert competences in energy planning, energy policy, alternative energy production, environmental policy, and so forth. As the intensity of the public debate over energy futures waned in most of the industrialized countries during the early 1980s, either through over-exposure or some kind of definitive parliamentary decision, these so-called counter-experts thus found themselves in need of new sponsors to support their work and new institutional locations. Some became professional consultants, working either in private consulting firms or in relation to the government, and some found jobs at non-governmental organizations, like Greenpeace, or the older, more established conservation societies. Others carved out niches in the media and the universities, creating new professional identities as

environmental journalists, environmental and energy researchers. Still others moved into governmental and intergovernmental agencies, like the World Bank and the European Commission, to develop programs in energy efficiency and sustainable technology development.

What began to be noticeable in the mid-1980s, to a significant degree as a result of these professional outgrowths, or spin-offs, from the environmental movement, was a new kind of environmental policy agenda, the so-called global environmental agenda that focused on problems of biodiversity, climate change, and transborder pollution. These problems were, of course, identified by scientists and engineers as serious and urgent, particularly after the hole in the ozone layer was disclosed over Antarctica. It is, however, worth noting that most of these international environmental problems had been discussed at least since the 1940s by concerned scientists and nature-lovers, and, at the 1972 UN Conference on the Human Environment in Stockholm, the global nature of environmental problems had been stressed by many scientific participants.

What had changed in the meantime was the character of the international political economy. By the mid 1980s, production, in many branches, had become increasingly globalized, with research carried out in one part of the world, development in another, and manufacture in still another. Individual firms were increasingly nodes in transnational corporate networks. Economic life had more and more come to be governed by international patterns of production and diffusion, and this globalization trend was further accentuated by developments in telecommunications and information technology. It became possible, and, in a few short years, common practice, to plan industrial operations on a global basis, and to shift operations from country to country depending on changes in market and financial conditions. There are, of course, many elements to this globalization that are open to dispute, and there is, to say the least, a lively discussion of what all this means. For environmentalism, and environmental science and technology policy, globalization has meant a shift in substantive focus - from the

local and national to the global, when it comes to the issues to be dealt with - as well as a shift in location - from national policy-making bodies to intergovernmental and international organs, when it comes to agenda-setting, and, increasingly implementation of research programs, as well. In actual research practice, the new information technologies have meant a great deal, in terms of the kinds of observations that can be simulated, the kinds of models that can be constructed, and the kinds of calculations that can be made. The social construction of scientific facts has been shifted from a more or less direct interaction with the environment and its component parts, to an ever more abstract and aggregate meta-environment of atmospheric, hydrological and geological processes that cannot be directly observed or, for that matter, studied.

It can be suggested that what has made these new issues particularly interesting for the new cadres of environmental professionals that had, as it were, grown out of the environmental movement, is that their solution requires something more than old-fashioned science and technology. They require rather a new kind, or mode of knowledge production that combines various disciplinary perspectives. Most importantly, these new global environmental problems require a new kind of socio-economic expertise to complement the traditional kinds of scientific-technical expertise that had previously dominated environmental science and technology policy.

In particular, there is need for an intermediary expertise between the global and the national, an expertise in the social, or, as it is often called, the human dimensions of global change. What this expertise often involves is a knowledge of particular methods of accounting, assessment, scenario building, forecasting, foresighting, prediction, and the like that seem to be called for in dealing with these extremely abstract and uncertain global problems. But it is also, at various levels and in various ways, an expertise in societal adjustment, environmental management, life-cycle analysis, risk assessment, the methodological and analytical toolbox of ecological modernization. The German social theorist Ulrich Beck calls it reflexive

knowledge, a kind of knowledge that Beck sees as characteristic for the emerging risk society that, he has argued, has largely supplanted the modern industrial society with its production of goods and services. Now our societies are primarily managing the consequences of industrial production, dealing with the risks and uncertainties of large technical systems, rather than producing useful products.

I would contend that it was the environmental movement that first identified the coming of the risk society in the 1970s. Instead of calling it risk society, however, which, in many respects, implies an acceptance of continuous and ever more serious risks and dangers in our complex, technological societies, the environmental movement saw the social construction of risks as the problem to be overcome. It was industrial or capitalist development itself that was the problem, the project of modernity with its cornucopian vision of limitless progress; an ecological society would be one that lived within nature's limits, however difficult it was to define those limits in practicable terms. The ecological society - ecotopia - proved, however, to be a vision that could not be realized in practice, at least not on a general, global scale. Risk society, on the other hand, can be lived with, but it requires new kinds of expertise in order to become sustainable.

I have called the new kind of movement that has emerged around the global environmental agenda for transnational environmentalism, in order to emphasize that the environmental non-governmental organizations that are contributing to science and technology policy increasingly transcend national borders and operate much like transnational corporations. They have sophisticated media and communications strategies, and they often contain experts in the new kinds of knowledge that are becoming ever more important in environmental research. We can contrast the cognitive praxis of this new transnational environmental movement with the cognitive praxis of the movement of the 1970s.

Ecological modernization can be seen as a result of this transformation of environmentalism - from a loosely organized, activist movement in the 1970s to an ever

more integrated program of industrial and technological policy in the 1990s. The positive aspect - the good news, as they say in America - is that sustainable development is now something of a fashion in scientific and technological circles, and there is a real change in many industrial, administrative and institutional practices. The problem, however - the bad news - is that there is a great deal of opportunism and rhetoric masquerading as substance. And it has become a real question what kind of competence, indeed what kind of knowledge, the new industrial practices actually represent. What is the cognitive core of cleaner production and environmental management, what kind of knowledge do ecological modernizers really need?

My own thoughts on the matter are still quite preliminary, but I do think there is a great deal that our own interdisciplinary subject area of technology and society can contribute. On the one hand, we can put some of these new activities into a historical perspective, as I have tried to do this afternoon in my highly schematic and personal way. We can explore the intellectual and cultural roots of the new ideas and technological principles. We can also try to distinguish, by means of our theories of technological change, between those cleaner technologies that are radical and those that are incremental, that is, between those improvements that based on new kinds of production principles and a different set of world view assumptions and those that are ecological in name only, what are really just good old-fashioned business practices cast in a different terminology. It is all of the variants in between the radical and the incremental, and the social and organizational implications that are associated with them that are interesting to study and assess. In terms of education, it is important to develop new categorizations, new ways of grouping the emerging cleaner technologies into clusters or systems, or ideal-types and classes, and seek to identify their common characteristics, so that we might be better able to suggest ways to reorganize the technical subject matter, even technological disciplines along sustainable lines. These are, of course, major undertakings, which will require collaboration among historians, economists and sociologists

together with engineers and environmental experts.

I think there is also a need for considering new kinds of courses and educational programs. As I see it, green engineering is not just a matter of eliminating waste products at their source, or of using natural resources and energy more efficiently. Nor can it be confined to so-called lifecycle analysis, which is undoubtedly an important new conceptual and analytical method in that it brings holism back to engineering, from where it has been away on a long modernist vacation. Green engineering requires also a change in attitude, or in what might be called a change in engineering ethos. So much of the environmental movement questions the underlying meanings of technological development. The entire mythology of progress, as articulated by Bacon and Descartes in the 17th century, and further refined by Comte and Marx in the 19th century, is seen as the root of the problems of environmental degradation. The linear model of innovation, and with it, the promethean, or masculine, project of artificiality, of making, manufacturing, industrializing, mechanizing has been brought into question by the environmental movement. And it is important to expose future green engineers to that questioning.

Already in 1948, Fairfield Osborn described 'man's war with nature' as he put it in one of the first environmentalist books, *Our Plundered Planet*, which came out in Danish the following year. Fifteen years later, Rachel Carson brought the war with nature to even broader public attention in her book, *Silent Spring*. With a mixture of scientific precision and poetic passion, Carson awakened the world to the environmental crisis and the need to reorient socio-economic development into ecological directions. As she put it, 'The road we have long been traveling is deceptively easy, a smooth superhighway on which we progress with great speed, but at its end lies disaster. The other fork of the road - the one less traveled by - offers our last, our only chance to reach a destination that assures the preservation of the earth.' It is important for engineers to remember, and read, the works of these pioneers of environmentalism, so that they may better understand what it is that they

are trying to achieve.

Ecological modernization, we have been told by one of those who has studied it most closely - Maarten Hajer in the Netherlands - can be seen to have three conflicting sorts of social implications. On the one hand, it can be seen as a new technocratic project, a way of renewing business as usual, a new stage in the rationalization of nature and in the exploitation of nature for the benefit of humanity. On the other hand, ecological modernization can be seen as a social process of institutional learning, a pragmatic process of change much as it has been seen in Denmark, by which environmental concern seeps into social institutions in a piecemeal and largely unplanned manner. This is an instrumental strategy, and requires management competence, and an integration of ecology and economics. A third variant is ecological modernization as cultural politics, as a way of reorganizing humanity's interactions with nature, readjusting the place of production in social life, and, most crucially, involving as many people as possible, as many so-called stakeholders in the actual process of technological development and use.

In our educational activity, we should prepare engineers not just for the hegemonic interpretation of ecological modernization; and most especially, I would argue, with Hajer, that the cultural elements need to be given much more emphasis than they have been given up till now. My own research focuses on the role of public participation in the new programs of sustainable science and technology. I am currently coordinating a European project, where we are investigating the ways in which environmental organizations and other direct and indirect representatives of the people are involved in the emerging innovation networks in environmental science and technology. In Beck's risk society, there is a new risk that ecological modernization will put in place new power elites, new authoritarian forms of knowledge production and business management that are neither accessible or accountable to democratic forms of decision-making. And as environmentalists, particularly in developing countries, have been pointing out for some time now, the new

discursive practices of sustainable development and ecological modernization can be seen as a reinvention of imperialist relations between the North and the South. As Vandana Shiva told a conference on science, technology and society in Goteburg in 1992:

The way 'global environmental problems' have been constructed hides the role and responsibility of the globalising local - (i.e., we in the west) - in the destruction of the environment which supports the subjugated locals.... Through a shift from the present to the future, the North gains a new political space to control the South.

The 'global' thus creates the moral base for green imperialism.

And so, as we train the green engineers of the future, we need to educate them in the social and cultural implications of what they are doing. We need to devise new courses and create new competences. And I look forward in the years to come to work together with you in making some contributions to achieving these tasks.

This text is the greater part of Jamison's inaugural lecture as professor in technology and society at Aalborg University, February 7, 1997.

Letter from London

by Janet Rachel Low

Such is the life of a research fellow, that once again I find myself faced with the imminent closure of my contract, and the possibility of penury until I find a place to prostitute my knowledge once more. In anticipation of this I found myself diversifying over Christmas to find out whether I could at least support myself financially if no academic post materialised in the unlikely season of spring. I took a job cabbying.

That is, me and my car (which I couldn't bear to sell even though that seemed the most obvious solution to the cash crisis) gave ourselves over to the business of moving people around London for a small fee (a very small fee in fact, I have no idea how anyone makes a living out of driving a mini-cab). My friend Madeleine, motivated by a concern for my safety, recommended me a 'good' cab company to apply to: Q-cars. That's Q for queer. And so I commenced.

A couple of transsexuals, a handful of transvestites, a bunch of dykes, a clutch of gay guys, and me the quaint curiosity, an old fashioned 'straight'. Never before had I found myself so continuously having to account for my sexuality to a constantly renewing queue

of people tumbling into the intimacy of my car. Blimey. Sex became the single most important thing in the world over Christmas. Sex, that is, in talk. And you know what Foucault said about that. The more it's spoken the less it's done. Actually, he said the more it's a secret the more it is known. And this seems at least one worthwhile thing to remember: making sex the single most important defining feature of a community is likely to rob sex of its sexiness. It felt more like a threat. Which serves to remind us all just goes to show how easy it is for a radical to turn into fascist without even noticing (especially a drunk radical).

Which seems a good point to remember the social studies of sociology of anthropology of science of knowledge of scientists of technology of things of humans and of actants - a bunch of radicals with the full potential of sliding into complacency and complicity, and turning into a menace on the edge.

One of the transsexuals (a very sophisticated being with implants and explants to bring the shape of the body into line with a certain understanding of what a woman is) was also interested in deconstructing technological

artifacts. She was saying that she liked to take things to bits, to find how they worked. A sister de-constructivist, I thought. And she said that once she'd figured them out, she could never be bothered to put them back together again: I know exactly what she means. Once she knows how they work, she says, the interest in them goes. Well. I couldn't help wondering whether she thought she now knew how she worked, now she had taken herself (in some sense) to bits (literally).

OK, we have a similarity, but there is also a difference. It was always my brother who took the physical objects to pieces and never put them back together again, never me. And my deconstructive activity is wholly intangible in the concrete sense. But we do seem to be engaged in something of the same. Also, I have a very different relationship with my breasts than she does to her penis. I wouldn't let anyone take mine away without a fight, whereas she is saving up money, and going through all kinds of institutional hoops in order to persuade someone to remove hers.. Nevertheless I'm with her all the way in saying that the bits of the body matter quite a lot in the business of living and not just in the small moments of actually making love. But we don't just live in a physical plane.

Gay, lesbian, transvestites, transsexuals, feminists have all done their best to make the point that bodies and sexuality are active actors in the social network that we all move about in. Transvestites and transsexuals make a lot of sense as a breaching experiment, they force everyone into at least a small hesitation in which to realise that they routinely change their behaviour according to whether they think they are speaking to a man or a woman. By the way, this doesn't mean that it is necessarily wrong to treat people differently. Issues of morality easily muddle the thinking. Let's just take this step by step.

First is the point that there are two categories of people that everyone has to fit into. But who made that rule? That's another question, leave it alone for a moment. First is the point that there are two categories of people that everyone has to fit into. If you still rebel about this, then all you have to do is ask yourself do you have children? do you have parents? are you married or in love? Can any of these mundane things be thought about

without recourse to these two basic categories of people. And can you see how you are implicated in making the rule work as you practice being that son or daughter, that mother or father, that husband or wife, that lover. None of us has the power to say they won't take part. As soon as you're born, you're categorised, before you even know how to say Hey! You can't choose to not take part in this. Even if you choose suicide, you'll be marked as a dead man or woman. It's the price you pay to take part in life. And, as any transsexual will testify, it is the presence or absence of the penis that carries the guarantee of this difference.

When Saussure realised that signifiers weren't the same thing as signifieds he opened the way to a cluster of interesting avenues of thought about the relationship of the word to the world. But for some reason, whereas we are all happy with the idea that the word Tree has an arbitrary relationship with the thing it stands for we don't seem to be able to carry the idea across to the thing that gets called the phallus. Is a phallic object like an erect penis because a penis is a phallus? or is a penis called a phallus because it looks like a phallic object?

That we all continue to assume (even on the odd occasion, if not always) that people with penises are people with power - and don't forget, it is people with penises who have the power over naming in our society [a not in-sign-ificant thing] - should alert us to the fact that there is an issue lying precisely there that needs to be addressed. This revelation raises 3 vital questions about theoretical directions. 1. Do we try to break the word away from the bit of the world it's trying to speak for (ie take it from the penis and attach it to the vagina or the breasts)? 2. Do we break the thing itself and try to drain away its power altogether? or 3. Do we wonder about the nature of the power of the word and how it comes to be so attached to certain kinds of things?

All three strategies are deployed in our 'EASST' community: first, science studies, etc., can be found trying to take the power of the scientist and attaching it to sociology (for example) in an outright contest of who is right (Collins and Wolpert spring to mind, and this can be found in some feminist theories who

try to give it to for example Donna Haraway and take it from, well, any man actually). This sets up a counter attack where other people try to keep a grip on the phallus and hold it firmly in its traditional place (those people who consistently look for great men to quote and quote and quote ad nauseum rather than spend any time reading something written by a woman let alone any [other] object). Second, the deconstructive reflexive stuff (of, lets say, Woolgar and Ashmore) are very good at taking away everyone's power, and draining it off via an intricate and infinite network of other words and sentences. And the third: the Actor Network Thing which leaves power intact and spreads it between many kinds of thing via a semiotic network. But how do the actors relate to this semiotic? and from where does the sign draw its power? This is the same place I arrive at having come through the Woolgar/Ashmore discursive route. How do words, or signs, conjure up and carry power around from object to object? And how do they fix certain objects in more or less powerful positions? The mystery remains firmly locked in the big black box.

Dissertation Abstract

Sabine Frühstück, *Die Politik der Sexualwissenschaft. Zur Produktion und Popularisierung sexologischer Wissens in Japan 1900-1941*. (The Politics of Sexual Science. On the Production and Popularization of Sexological Knowledge in Japan 1900-1941). Ph.D., Institute for Japanese Studies, University of Vienna.

What happened to the western science of sex -- often considered to play a major part in the liberation of a repressed sex -- when it was received in Japan, a country where allegedly sex had never been repressed and in any case the traditional use of sex had been entirely different? How and why was western science transferred to Japan in the special instance of sexology? What was the role of the wider public in the attempts to establish sexology as a scientific discipline and an instrument of social policies.

What to do, what to do. Well, I don't really know, and I certainly don't know what you will do. But in the meantime, I'll tell you what I'm going to do. I'm off to lurk around Lacan, that dead French bloke who listened to anthropology, looked again at Freud, and learned a thing or two about the power of the phallus and the place of the penis. The human subject as a three dimensional thing which has mind body and unconscious to contend with. There's a lot going on, don't you know, in psychoanalysis these days. It has something to say about science, signifiers, subjectivity, and sexuality. It comes straight in on these sociological questions. It says something substantial (if a little tricky) about knowledge and truth and all that. It won't help solve my financial crisis, ok, but it might help to understand why that's something else that we never address in our studies.

Cheerio for now, then, and remember, if you're ever lost in London, look me up, I know all the quick routes from Battersea to the City, or at the very least, I can let you have the number of a very good cab company (0171 622 0011).

I treat these questions as both historical and methodological questions, the former in the frame of late nineteenth and early twentieth century Japan, and the latter in the history and social study of science, where the issues revolve around conflicts between scientific and popular knowledge, political uses of science, and the interaction of different national cultures of knowledge in the case of the confrontation between 'inferior' Japanese and 'superior' European, or mainly German sexology.

These problems are set in a distinct chronology which begins with the public appearance of certain so-called 'sexual questions', continues with attempts to establish the discipline of sexology, and ends, finally, with the failure of these attempts in the political context of the 1930s and 1940s.

Chapter one of the dissertation begins with an account of the first public dispute in one of

the largest Japanese national newspapers in 1908 on the supposedly dangerous effects of masturbation and the necessity of sex education. Using this dispute as a starting point, I discuss the interaction and collaboration between Japanese and mainly European physicians, biologists, and pedagogues. Chapter two takes up crucial questions of the professionalization of sexology in Japan. Chapter three deals with the problems faced when self-appointed sexologists attempted to forcefully disseminate their knowledge to the general public. Chapter four describes the decline of their endeavour in the face of the newly emerging racial hygiene movement and the later policies of the 1930s and 1940s.

The empirical basis of the work consists of an analysis of 440 articles and more than 100 advertisements and advice columns. While the articles are taken mainly from sexological journals, the greater part of the advertisements and advice columns stems from popular scientific print media and from women's journals and those of social reform movements. I also drew on additional sources which included data on the structure of and changes in secondary education and print media and its readership, as well as governmental statistics on the condition of the Japanese population in general, and prostitutes and soldiers in particular.

With respect to the question of how the western liberating aspect of sexology can be transferred to the non-repressed Japanese, the first and perhaps most drastic step was the translation and adaptation of European problems to Japanese society which did not consciously have these problems previously. The discourse on the so-called 'sexual question' between the turn-of-the-century and the early 1940s was manifold. It developed around the necessity of sex education in the broadest sense and reached towards concrete birth control, prevention of venereal diseases, opinions on masturbation and its consequences, the aims of the women's movement, the fight against prostitution, and, before the Second World War, the emergence of not only 'positive' but also 'negative' eugenics. Finally I treat the execution of racial hygienics at the cost of sex education.

As for the second question (how and why

was western sexology transferred to Japan?) I come to the conclusion that one central idea was shared by all participants in this endeavor and played a key role in the fight for (and against) sexual and social reform and sex education: Alliances and opponents were convinced that correct knowledge would lead to correct behavior. And the 'correctness' of the latter was measured by its social consequences. Thus it becomes comprehensible why representatives of politically divergent aims, as the liberation and the improvement of the living standards of underprivileged groups or the enforcement of racial hygienic programs by the government, could both successfully refer to knowledge, science and the necessity to disseminate issue-specific knowledge.

There are two main conclusions to be drawn with respect to the third question (the role of the wider public in the attempts to establish sexology as a scientific discipline and an instrument of social policy). First, none of the scientists, journalists, politicians and representatives of social movements involved appeared in any one single role. Instead, they had to apply various and continuously changing strategies, the rhetoric of 'scientific authority', as well as intimacy and distance towards governmental institutions and the power apparatus, according to the changing configurations which all of these participants formed as a whole. Second, from the viewpoint of the historiography of science, the differentiation between 'pure scientific knowledge' and 'popularized knowledge' cannot be kept up in any phase of the development and dissemination of sexological knowledge. What occurs instead is a co-production of knowledge in a public and semi-public hybrid space. In the case of sexological knowledge, the production, stabilization, and establishment of scientific knowledge as well as popular scientific knowledge was the result of interactions among all the aforementioned participants. These complex and continuously changing configurations of participants and discourses were to a great extent influenced by political, social, scientific, as well as the international conditions.

Some Notes From the Past A personal history of EASST

by Aant Elzinga

This summer it will be six years since Stuart Blume, former EASST president, asked me to consider running as chairman of a new board to be put in place. I think it was after he had scouted around in the European STS network and sent out ballots with seven names - all men - in April 1991. I was already a member of the council elected Dec. 1986/Jan.87 and that for a four year period, but had not been very active. It was with considerable reluctance that I even considered the idea. As an organization EASST had been around for roughly a decade and had now become rather dormant. Most of the more regular and visible action seemed to be in the U.S. with 4S, or with SHOT, HSS and PSA, all four of which were fairly weak on the science and technology policy studies dimension, an area that EASST also tried to cultivate (in the very first number of the *EASST Newsletter* Arie Rip already pointed to the unfortunate gap that existed between social and historical studies of science, and science policy, an observation that came up and evoked commentary from time to time also in subsequent issues). Furthermore, I was already overloaded by other commitments. One of these, the organizing of a joint 4S/EASST conference in Goteborg the next year overlapped, so after talking with Andrew Jamison and Paul Hoch, who were also to be on the new board, we decided, OK let's give it a go. After all we had some strong names with us: Steve Woolgar (Brunel) and Ben Martin (Sussex).

I

The first council meeting over which I presided was held in Cambridge Mass. (MIT), where several of us went to attend the 4S conference. Here we discussed a strategy for revitalizing EASST and revamping its

newsletter. Chunglin Kwa was also coopted as editor, taking over from Arie Rip who generously appears to have shouldered this task with great stamina for the whole previous decade of EASST's existence as an organisation. The date was 16 Nov. 1991, exactly to the day three years after the opening of the joint 4S/EASST conference in Amsterdam so deftly managed by Loet Leydesdorff. Being (after Ghent 1984 - "the Sarton Centennial Conference") the second such a joint conference with our North American (and Australian) cousins, Amsterdam could be called the tradition-setter. Now it was our turn to go on from there; directly from Amsterdam we had Rob Hagendijk with us - and from the far north there was Hans Skoie (Oslo) who was more than most of us directly concerned with European science policy.

Actually we didn't have much of an idea what we were taking on, which was probably fortunate, for otherwise the whole project might have been dropped. Anyway, Stuart Blume had come up with the genial idea that EASST should have a new category of members alongside individual ones (*EASST Newsletter* vol. 9, no. 2 May 1990). These were to be institutional members, a consortium of centres in the field that would sign on to pay a substantial sum for a three year period. In addition Stuart had an agreement with the Science Policy Support Group (SPSG) in London to handle the functions of a secretariat and distribution of the newsletter on a year by year contractual basis. Peter Healey's role in this and other respects became pivotal for our success.

Counting from that first fateful day in November 1991 I have now presided over eleven council meetings in various locations - Cambridge Ma., Amsterdam (thrice), Goteborg, Brunel/N. London, Budapest, Paris, London, Bielefeld, and most recently Vienna.

The old council was replaced by a new one in Budapest, where a glaring gender skew was partly corrected (cf. the inside front page of this issue). The most recent round of elections earlier this year led to a partial correction of a northern European bias. So now the new council that constituted itself at its first meeting in Vienna (hosted by Ulrike Felt, the Organisational Secretary) also includes members from southern Europe and one with a strong third world connection.

At midnight the 30th of June Rob Hagendijk and I will get together over the phone so I can wish him luck as new president of EASST for the period to come.

Rob's steady hand as our "finance minister" during what will have been 2040 days (five years, seven months and fifteen days since the Cambridge Mass. meeting) has been a mainstay, keeping us on a lean, yet dynamic track, critically appraising the expanding infrastructure needed to push the original newsletter (redubbed "Review" in 1994) ever further in the direction of Chunglin's ideal, revamping into the (S)T(S) Literary Supplement of Europe.

On top of this we now have a Homepage, travel stipends, funding of joint workshops with NECSTS, and other initiatives, as our strategy document put it, "to foster within Europe the scholarly study of science and technology, including their historical development and role in society. More specific aims include improving scholarly communication and exchange in this field, increasing the visibility of the subject to policy-makers and the general public, and stimulating and supporting teaching on the subject at all levels" (Revitalization strategy statement of 29th Oct. 1991).

Looking back, I think it is fair to say that we achieved some of the goals that we set ourselves five years ago, yes. EASST has expanded its membership, even if there is still an ebb of numbers between and an influx during major events, like conferences. Here Bielefeld marked a new tidal high point.

Also, we have actively reached out to all parts of Europe in connection with workshops on topical issues, often in collaboration with NECSTS. The system of travel stipends for young scholars has helped highlight new curricular programmes and attendance to

summer or graduate school initiatives in STS. More recently EASST began to raise its voice in Brussels, as one of many actors to provide input into policy discussions in as far as these have a bearing on intellectual agendas in STS. Apart from workshops the major events remain our conferences, the one held jointly with 4S every four years, as well as the biannual EASST conferences in between, the last one in Budapest 1994 (the next one due next year). Out of Budapest, thanks to David Edge's unflagging enthusiasm, also came a handsome solid thematic issue of Social Studies of Science, on the situation of and prospects for science in Eastern and Central Europe.

Putting on a certain species of spectacles one might say what I have just described is an episode in the construction of a network-cum-institutional arrangement with a five letter acronym that someone dreamed up some sixteen years ago, and how serendipity had us run with it. Rob Hagendijk in his recent book "Wetenschap, constructivisme en Cultuur", shows that constructivisms now are many. Not even the non-relativist brand of "cultural constructivism" comes with ready made wooden shoes - if anything, it is a question of appropriate sunglasses and ballet dancers' slippers.

I want to express my gratitude here and now to all members of the previous two councils; without your support and involvement we could not have gone ahead, nor had as much fun on the way. To the new council, all the best with your continuing venture.

But is that all I have to say? No, there is more. Of course as reader you may very well stop here, because now I am going to get more longwinded.

II

At the Bielefeld meeting last year 4S celebrated its 20th anniversary. This got me thinking - isn't EASST actually older than 4S? From where do we trace our roots as an organisation? Is it really only from the foundational year 1981, or isn't it more like 1973, or even slightly before that? In a certain sense it depends on one's perspective - (Rob,

don't forget those old sunglasses that you use when you go sailing on the Frisian lakes).

Doing some quick historical spadework revealed to me that a case might be made that we are actually older than 4S. This is if we negotiate the "fact" that EASST sprung out of and continues the spirit of project PAREX which was created in 1970 and continued until 1986. The first EASST council of five persons consisted of several leading figures from PAREX (a second - six man - council elected in Dec. 1982 showed less overlap, with John Ziman as president).

PAREX is a contraction of "Paris-Sussex", and the project was a vehicle for promoting collaboration on Anglo-French basis between scholars working on different aspects on the social studies of science. The two "co-animateurs" behind the effort were Gerard Lemaine and Roy MacLeod, the one in Paris and the other then at Sussex. Others involved were Elisabeth Crawford, David Edge, Michael Mulkay, Günther Küppers, Peter Weingart and, I think, also Helga Nowotny (Helga tends to have been involved in most things European).

PAREX's secretariat was located at the Maison des Sciences de l'Homme (MSH) on Boulevard Raspail in Paris, a nice location with good library facilities and the home of the journal *Social Science Information* which picked up on the finalization thesis (1976) and even now continues to publish in our field. MSH, together with the CNRS Programme STS also supported publication of the journal "Pandore" in which Bruno Latour took a leading role.

In November 1994 I took an initiative to invite persons from various French STS groups and had a meeting at MSH with representatives from many of them in hopes of bringing EASST back to its former (PAR-EX) home. This, and a second meeting later, was set up by Iskender Gokalp from CNRS-Orleans who is on the board of MSH, where the director, Maurice Aymard was interested. The idea, was simply to try and site an EASST conference in Paris, and perhaps also a workshop at MSH. Unfortunately nothing came of it.

For the sake of the record I can say that our invitations went out to persons at GERS, BETA, GERSULP (Strasbourg), ECP,

GEMAS, Orstom, CRHST, INSERM, REHSEIS, Ecole des Mines, and CNAM. Those of you who know the Paris scene won't need a glossary of acronyms at this point - anyone else is here and now challenged to make an effort to get acquainted with what's behind all those letters.

I still have not given up the idea of a Paris conference and/or workshop(s), and hope the new council may pick it up again and try to make it happen in the future. We will have history on our side, in that case.

In 1973 PAREX, in order to continue to receive support through MSH was asked to "Europeanize". This led to a leap from two to eight countries being represented. Apart from becoming more solidly European in scope PAREX also became more broadly interdisciplinary in character (*EASST Newsletter* vol. 5, No. 1 Feb. 1986 contains some of this story). The purpose of this European network became to organize one general meeting each year and several working sessions on particular themes. Benchmarks are a meeting the "Naissance des nouvelles disciplines: conditions cognitives et sociales" (Paris Dec. 1973), the meeting "Methodology in the Sociology of Science" (York, UK June 1974), and "Finalization in science" (Starnberg, Germany 1974); "The Role of Research Organizations in Orienting Scientific Activities" (Inst. of Advanced Studies, Vienna, org. Karin Knorr). Similar questions and the new approach had already been ventilated at the International Sociological Association's Research Committee on Sociology of Science (RC 23) held in London in September, 1972. The volume from this latter meeting characterized the papers it included as follows: "They represent a radical change in the dominant concerns of the sociology of science: from exclusive attention to the social behaviour of scientists to a systematic understanding of how and why particular sciences have developed and of the relations between scientific and dominant cultures and institutions." The meeting of ISA's Research Committee 23 followed upon an earlier discussion of new developments at the World Congress of Sociology in Varna, two years earlier -- (RC 23 was there with us in Bielefeld last year and some of the folks from

EASST will probably see them again at the coming ISA world congress in Montreal).

Project PAREX for its part also sponsored a series of interdisciplinary, comparative and international studies in the history and sociology of scientific development. Concern was the need to study "both internal, scientific and technological processes involved in the generation of new knowledge, and the social factors which accompany and influence those processes", and to get historians and sociologists to join forces in such an endeavour. This was also a leitmotif of the group at the Max-Planck Institute in Starnberg who brought in the problem of the bounds and conditions of "steerability" of science from the side of policy, a theme continued in Bielefeld. In his review of the new problematique Peter Weingart in "Wissenschaftsproduktion und soziale Struktur" (1976) spoke of the transformation of the "wissensoziologischen Grundfrage auf die Analyse der Wissenschaft", particularly in the light of the work of Thomas Kuhn. The old externalism-internalism distinction was blurring as it received new form and content.

The Starnberg and Bielefeld approaches were also influential at first in the initial stages of the design of a programme for science studies at the University of Amsterdam which came out of a national competition in the Netherlands for government funds for a priority programme in STS. This took place around the same time as EASST was being conceived.

At the same time the MSH had been particularly active in stimulating science studies, with a couple of symposia 1979; in March 1980 a PAREX French-British workshop at CNAM in Paris focused on controversies in science. Later in the same year, 25-29 September 1980, at the ISA/RC 23-PAREX meeting in Deutschlandsberg near Graz in Austria (a meeting in which I also participated) the idea of a new membership organization took shape and the name "EASST" was coined: Peter Weingart was one of the organizers. Two years later the first conference of the newly formed EASST was held in Deutschlandsberg. Trevor Pinch in a report from that meeting observed how a "brief head-count of the participants showed that over half came from the Netherlands",

and now with "the establishment of a new Chair in 'science dynamics' at Amsterdam it would seem that the locus of science studies in Europe has shifted a few hundred kilometres north of the Paris-Sussex axis". EASST, he also noted, "has largely replaced PAREX as the main European forum for Science Study activities" (*4S Newsletter* 7, no. 4, Winter 1982, p. 27).

Through the PAREX-network the proposal to establish such a Europe-wide membership organization was distributed, and the response was sufficient for the PAREX Steering Committee (which had already been expanded in 1979) to take the step and start acting as the council of the new organization. Thus out of PAREX came EASST, officially working as such 1981. In 1979 the PAREX newsletter (PAREX Informations, first appearing in 1976) had gotten a new base in Bielefeld, with Georg Kamphausen as editor; with the reconfiguration of part of PAREX into EASST 1981 Kamphausen became responsible for the new membership secretariat, while Arie Rip (since 1979 a member of PAREX's enlarged council) took charge of the EASST secretariat and became the editor of the new organization's newsletter.

By this time another new entity had also already appeared on the European arena, the annual publication of a Sociology of the Sciences Yearbook, starting with its first volume in 1977, the same year as the Ina Spiegel-Rösing and Derek de Solla Price edited *Science, Technology and Society, Cross-Disciplinary Perspectives*, published under the aegis of the International Council for Science Policy Studies (ICSPPS); rapidly on the heels of this came Stuart Blume's book, *Perspectives in the Sociology of Science*, also repealing Mertonian analyses of the production of scientific knowledge.

The 1980 Yearbook on *Social Processes of Investigation* (Knorr et. al. eds.) actually came out of a joint conference with PAREX at the University of Bielefeld (June 1979).

In the Yearbook collective, too, emphasis was on the comparative cross-disciplinary understanding of the sciences. The term "sociology" was defined broadly to include historical and philosophical dimensions, in contrast to a "narrow professionalised conception of the field." The basic standpoint

was to view the sciences "as a plurality of socially constructed ways of comprehending natural and social phenomena. It therefore rejects any attempt at imposing a unitary and monolithic schematisation of scientific knowledge and aims to situate developments of the sciences in broader systems of cognitive production." The institutionalisation of scientific knowledge as distinct cognitive structures and their relations with other forms of understanding institutionalised in different societies was also seen as important, opening up to anthropology of science and knowledge in a broad sense.

1981 was also the year that Karin Knorr-Cetina's *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science* appeared. So did the special issue of *Social Studies of Science*, "Knowledge and Controversy" edited by Harry Collins. This was just two years after *Laboratory Life: the Social Construction of Scientific Facts* by Bruno Latour and Steve Woolgar; *On the Margins of Science* edited by Roy Wallis; *Natural Order* edited by Barry Barnes and Steven Shapin; *Counter-movements in the Sciences* ed. by Helga Nowotny and Hilary Rose; and in 1980 came the anthology by G.S. Rousseau and Roy Porter, *The Ferment of Knowledge*.

It was a heady time where the existence of important macro-actors on our own transepistemic arenas could make a difference. In Paris the designation of the Délégation Générale à la Recherche Scientifique et Technique (DGRST) in France made it possible in 1980 for the Centre de Sociologie de l'Innovation at the Ecole des Mines to undertake an extensive programme of research and method involving co-word analysis. The actor-network theorists were sceptical about the nature and influence of pre-existing, large-scale social structures such as class and markets, and in particular the prior attribution of social interests. Instead they started their analyses from the level of interactions between individuals, groups and non-human entities, aiming to "scale up" from there to obtain broader explanations. Here constructivism took a different - Machiavellian - direction, leading to a more radical implosion of boundaries, between social and cognitive dimensions, external/internal, as well

as between nature and culture. This was an orientation antagonistic to the more historical sociological approach to constructivism cultivated in PAREX and even tended to irritate some people in the same town like Jean-Jacques Salomon at OECD who later set up an STS teaching programme with a strong policy/macro-politics slant at CNAM. In the somewhat broader and neo-institutionalist perspective that still informed PAREX, and in part the new more oecumenical organisation EASST, Everett Mendelsohn in his introduction to the Yearbook of 1981 on Sciences and Cultures, assessed the significance of the break with Mertonian sociology of science. The latter was seen to represent in its maturer post-war form a scrutiny of the role of the sciences in democratic societies, analyzing the social norms and social structures in which this obviously important activity might flourish. In consequence with this, even if not intended so, the "early historical and sociological studies were in large measure celebratory of the sciences and scientists", with a conscious pairing of the norms of science and those of the democratic state. The new approach was destined to deviate from that glossiness.

III

The breakdown of this positive and celebratory appraisal of the sciences did not just grow out of Kuhn's hat. In the perspective that informed EASST at the time of its inception 1981, and one that I have tried to promote on various occasions as president since 1991, there is another dimension that becomes equally important in drawing attention to our politico-cultural and intellectual roots. It is the emergence in society of various social movements in the 1960s and into the 70s, among them specific movements of criticism of science, its uses and its products on the one hand, but questioning also the very value of instrumental rationality and its very existence on the other. I am thinking of the anti-imperialist movement of those days, the radical student protest movements, the environmental movement, and particularly the women's movement which within academe spurred a radical feminist

critique of science. In my view we have a double heritage, intellectual and activist at once. From a moral point of view, and speaking of our own "ethos" as part of what I like to think of as a movement for the social responsibility of science, our roots go back even further than 1970, the year that PAREX, our institutional ancestor started up as a collaborative project/network in Europe.

When it comes to trying to get a handle on a history like this, I am struck by something paradoxical; STS-people are puzzling. They tend to become rather narrow-minded in constructing their own past, positioning themselves programmatically in mostly cognitive terms, while ignoring their own broader historical and socio-political contextuality. I guess this is part of the (very much "modern") game of having to legitimate oneself in purely professional terms in academe in the fight for respectability and funding. As Loren Graham has pointed it out once in an article, this has funny consequences; Hilary Rose in her book on *Love, power and knowledge* points to the same thing -- (Here, by the way, it was heartening to hear a corrective to this at the Bielefeld conference; referring to Bernal in his informal introductory remark, David Bloor countered the usual idealist account and made some important points of retrospect in his talk at the banquet on the occasion of his receiving the Bernal award).

Loren Graham remarked how scholars in our field have repealed internalism but when they write their own history in the long term perspective they sometimes point to J.D. Bernal and even Joseph Needham, and so they play up the event of the History of Science Congress in London 1931 as very significant. Then then they go on to trace an intellectual genealogy from the ideas in Boris Hessen's benchmark paper to Bernal 1939, Needham, Levy Hyman, and others to the externalism-internalism debate in the 1960s (with Merton coming in as a target alongside historians like Butterfield) - leaving out all the activism in and around the key figures from the thirties. Thereafter Kuhn's *The Structure* is usually introduced as a new benchmark. In this reconstruction of the past something vital is rendered invisible. It is the existence and importance of the Science and its Social

Relations movement of the 1930s and the many science activists in various countries who took part as committed and reflective intellectuals and scientists in a broader anti-facist and anti-racist movement of that time. Their concern, shared with Merton's, was "keeping science straight".

Hilary Rose for her part has noted in the same way how the "social turn" in our later history from the late 60s onward too has been reconstructed as an internalist cognitive genealogy that tends to fix upon Kuhn.

There is a paradox. While today's social studies of science take for granted the social context of science, their practitioners none the less typically tell their own origins story so as to emphasize the internal development of their history, and to neglect any version of externalism, whether the historical materialist question of 'What conflict outside us was within us the reflex of thought?' or any social constructionist account. There has been a tendency to focus upon Kuhn as founding father, single-handedly opening the doors to the possibility of a fully social account of science. To question this account is not to diminish Kuhn's contribution, nor to neglect the importance of intellectual development, but rather to insist that attention is paid both to theories and their historical location - not least our feminism's own theorizing and our own contexts of production".

It is in such a broader perspective that I also like to locate one of the dimensions of EASST, an amalgam of impulses coming from the various "turns" in research agendas marked by various programmes within STS together with responses to socially, culturally and politically shaping forces in society at large - not as something internal/external, but as a part of a coevolution of, broadly speaking, intellectual and social orders. Here, for example, in Scandinavia the welfare state that has now been dismantled in various respects has also been a significant patron. Where, moreover, would be the various programmes without the sorts of government priority programme funding, and even commercially inspired projects, that have come our way, permitting consolidation of research units with exiting agendas of research under some variant of the rubric STS? This has created new opportunities, but it has also contributed to many of the inbuilt tensions we are facing today. If these tensions are articulated and properly managed at an

organisational level I think it should even be possible to do some further bridging with colleagues working in the neighbouring domain of Science, Technology and Innovation (STI).

I also think that in this broader perspective that takes us back to the 1930s, we can make the case that we are also carrying forward the spirit of critique and analysis of a generation ago, pertinent to the social responsibility of science in our own. It was this quest that scientists in 1937 too formulated, under for them specific historical circumstances, when they set up the Committee on Science and its Social Relations (CSSR) under the International Council of Scientific Unions (ICSU). At that time it was physicists, biologists, astronomers, geoscientists, and some chemists, and they complained of the lack of social scientists to participate in their critique of science and/in society. Granted they only focused on the one way relationship of the impact of science on society, and not the reverse relationship of societal shapings of science, even in its cognitive character. Granted too, therefore, that in retrospect their generation to us appears as rather scientific. This assessment also goes for both Bernal and Needham, and it is confirmed by a look at the approach of the parallel body with overlapping membership with the CSSR. I am thinking of the international organisation set up in 1947, the Commission on the Social Relations of Science (CSRS), within the International Union of History of Science which was also created in 1947 (under ICSU - later IUHS became IUHPS, International Union for the History and Philosophy of Science (IUHPS)).

At the outset CSSR too was fired by an 'externalist' radicality in its view on the science question in society. With the advent of Cold War politics this appears to have ebbed out - some of this can be seen by reading through the annals of the offshoot journal "Impact (of Science on Society)" published by Unesco. However, and this is the point here in the present context, the scientism cultivated by the amateurs of CSSR was not narrowly celebratory of science as is the case with our present day natural scientist instigators of the so-called Science Wars. These latter day luminaries, as David Edge has convincingly

shown, do not even have the courtesy to apply the same rules of argument and evidencing they use in their own domain when for some strange reason they feel compelled to declare battle with people in STS. I am sure the scientists in CSSR and CSRS in the 1930s and 40s respectively, fired by an acute sense of social responsibility, would have no difficulty understanding what our mission in STS today is about, and be sympathetic.

Fellowships and Positions Available

The **Oxford Centre for the Environment, Ethics and Society** (OCEES) proposes to elect a Visiting Research Fellow for the academic year 1997-8 or part thereof. The Fellowship is intended for a scholar who is usually based at another university in the UK or overseas, who is already provided with financial support, and who wishes to pursue academic study and research relevant to the concerns of this multi-disciplinary research centre. The Fellow will be eligible for a housing allowance of up to £4000 p.a. pro rata, and up to £1000 for research expenses p.a. pro rata. The Fellow will also be a member of the Senior Common Room with dining rights. Application forms are available from the Administrator at OCEES, Mansfield College, Oxford, OX1 3TF, UK. Final applications must be received no later than 30 June 1997 including the names and addresses of three referees. Email enquiries: ocees@mansfield.oxford.ac.uk Website: <http://users.ox.ac.uk/~ocees>

Leo P. Chall Dissertation Fellowship in the History of Sociology, administered through the Research Committee on the History of Sociology, is open to PhD candidates writing a dissertation intended as a contribution to the history of sociology. It is worth \$5000. Applications for this year's fellowship should be submitted by 15 August 1997. Applicants should provide the following information, in English: A precis of the dissertation proposal, not more than 3 pages single spaced, including a description of the project, an explanation of how the research will contribute to the field, and a description of the work completed so far; plans for completing the dissertation; two letters of recommendation; a complete academic curriculum vitae, including exam results and grade transcripts where available, and details of any publications; and a list of current or previous awards or fellowships. Submit application to Prof. Jennifer Platt, Leo P. Chall Fellowship Committee Graduate Research Centre in the Social Sciences, Arts E, University of Sussex,

Brighton BN1 9QN, England

Science, Technology & Human Values, the journal of the Society for Social Studies of Science (4S), seeks a *new editor*. The term would begin with the 1999 volume; the editorship has an official term of five years (renewable). The Publications Committee is planning to meet and consider applications at the 4S annual meeting this coming October. Indications of interest can be given to any member of the Committee by June 30, 1997; the Chair (Rachelle Hollander, Room 995, NSF, 4201 Wilson Blvd. Room 995, Arlington, VA 22230) must receive all materials in support of an application by September 5, 1997. ST&HV is the flagship journal of the society; it is published by Sage Periodicals Press; the current editor is Olga Amsterdamska, University of Amsterdam. The editor of the Journal solicits manuscripts, arranges for their review, and makes final determination as to suitability for publication. Around 80 submissions are expected each year, and the success ratio is around 25%-30%. The editor also works with a group of contributing editors and an editorial advisory board of set terms, and is responsible for nominating replacements to the 4S Publications Committee. The editor reports on the status of the journal to the Publications Committee at the annual meetings each year. The ideal candidate is a person of stature in the field, with breadth and sensitivity to the alternate points of view within it, and with appropriate institutional support. For further information, contact the current editor, Olga Amsterdamska, at the University of Amsterdam <amsterdamska@chem.uva.nl>, the president of 4S, Karin Knorr-Cetina, at Bielefeld University <knorr@post.uni-bielefeld.de>, the secretary of the society, Wesley Shrum <sowesl@unix1.sncc.lsu.edu>, or any member of the publications committee: Michel Callon <callon@csi.enscm.fr>, Rachelle Hollander <rholland@nsf.gov>, Linda Layne <linda_layne@mts.rpi.edu>.

Michael Lynch
<michael.lynych@brunel.ac.uk>, Nelly Oudshoorn
<n.e.j.oudshoorn@wmw.utwente.nl>, Sal Restivo <restis@rpi.edu>, or Judy Wajcman
<judyw@coombs.anu.edu.au>

The **University of Groningen, the Netherlands**, section Theory and History of Psychology, offers a four-year research position to a student of non-Dutch nationality for writing a thesis on the history of psychology. More specifically, the thesis should deal with a topic related to the increasing emphasis on precision and/or standardization in the history of (some part of) psychology. After taxes and health insurance, the fellow will receive an income of about Dfl.2100,- (about 1100 dollars) a month. The university will help to look for an apartment for a reasonable rent (about Dfl 500 a month). The fellowship is open to applicants with a Master's degree in psychology or in a related field with demonstrable knowledge of psychology. The fellow will be working in the section Theory and History of Psychology at

Awards

Paul Bunge Prize 1998

The German Chemical Society invites applications for the 1998 Paul Bunge Prize, awarded the Hans R. Jenemann foundation, and administered by the Gesellschaft Deutscher Chemiker (German Chemical Society) and the Deutsche Bunsen-Gesellschaft für Physikalische Chemie (German Bunsen Society for Physical Chemistry). The award amounts to DM 10.000 and honors outstanding publications in all fields of the history of scientific instruments in German, English or French. Besides of the scientific paper applications should include a curriculum vitae and - if available - a list of publications of the applicant. The deadline for applications is September 30, 1997.

Nominations and self-nominations are invited. The Paul Bunge Prize will be awarded

the Department of Psychology. He or she will be supervised by Trudy Dehue or Douwe Draaisma and possibly a second supervisor in the Netherlands or abroad, who will be chosen after the dissertation topic has been specified. The fellow will be expected to finish the four-year educational program of the international school for science and technology studies 'Science and Technology in Contemporary Culture' which organizes summer and winter schools (of 2 weeks per year, max) in the Netherlands with internationally renowned teachers (in the past Bruno Latour, Karin Knorr, Donna Haraway have taught at the school). Considerations of applications will begin from June 1 and finish at November 1 1997. Applicants are requested to send a letter of interest, a c.v. and 10-15 pages of writing samples in English, preferably in the domain of the dissertation's topic to: Dr. T. Dehue, section Theory and History of Psychology, University of Groningen, Grote Kruisstraat 2/1, 9712 TS Groningen, the Netherlands. For questions contact T. Dehue, tel 31 50 363 6354 (secr. 6366), email G.C.G.Dehue@ppsw.rug.nl.

at ANALYTICA 1998, on April 21-24, 1998 in Munich.

The prize is named after the most important constructor of analytical, assay and high performance precision balances in the second half of the 19th century, Paul Bunge.

Information for applicants is available at the German Chemical Society, Public Relations Department, P.O. Box 900440, D-60444 Frankfurt/Main, tel 49 69 7917 325, fax 49 69 7917 322, email pr@gdch.de.

US Scientist receives Paul Bunge Prize 1997

(press release of the GDC)
On the occasion of the Bunsen conference 1997 in Darmstadt (Germany) the Paul Bunge Prize for the history of scientific instruments consisting of DM 10.000 was awarded on

May 9, 1997 to Dr. Silvio A. Bedini of Washington DC.

Bedini, born in 1917 in Ridgefield, Connecticut, studied at Columbia University. Afterwards, until 1987, he worked in several high positions at the Smithsonian Institution of the National Museum of History and Technology, Washington DC. Among his numerous scientific publications one of the latest, and a most remarkable book, published in 1994, is titled *Science and Instruments in Seventeenth-Century Italy*. Bedini, member of several American and European scientific societies, is still writing comprehensive publications on the history of science and scientific instruments, e.g. on the subject 'The Vatican and Science'. Dr. Bedini was honoured for his impressive scientific life's work on the modern history of instruments.

In addition to the 1997 Paul Bunge Prize, Claudia Schuster-Spiekenheier (Berlin) was honoured for her exemplary studies about the cooperation of craft and science within Assman's Aspiration-Psychrometer, and Jan Frercks (Oldenburg) for his methodically unprecedented investigations into the replication of Fizeau's toothed wheel for measuring the speed of light.

An Award by the AAAS

The Scientific Freedom and Responsibility Award is presented annually by the American Association for the Advancement of Science to honor scientists and engineers whose exemplary actions have served to foster scientific freedom and responsibility. The Award recognizes scientists and engineers who have: acted to protect the public's health, safety, or welfare; or focused public attention on important potential impacts of science and technology on society by their responsible participation in public policy debates; or established important new precedents in carrying out the social responsibilities or in defending the professional freedom of scientists and engineers.

This annual award was established in 1980 and consists of a plaque and \$2500. The 1998 Scientific Freedom and Responsibility Award will be presented at the AAAS Annual Meeting in Philadelphia, Pennsylvania, 12-17

February 1998. The deadline for nominations is 1 August 1997. To submit a nomination: send your name, address, and phone number; the name(s) and address(es) of the nominee(s), a summary of the action(s) that form the basis for the nomination (about 250 words); a longer statement (no more than three pages) providing additional details of the action(s) for which the candidate is nominated; at least two letters of support, with addresses and phone numbers; the candidate's vita (no more than three pages); any documentation (books, articles, or other materials) that illuminates the significance of the nominee's achievement may also be submitted. All materials become property of AAAS. Please submit all information to Office of Development, American Association for the Advancement of Science, 1200 New York Avenue, NW, Washington, DC 20005, USA, tel 1 202 326-6636, fax 1 202 789-2009, <http://www.aaas.org>

Conference Announcements and Calls for Papers

Collective Memory & Heroic Science in Nordic Arctic Experience, a conference marking the close of the Fridtjof Nansen Exhibition, will be held 29-30 August 1997 at the **NorskFolkemuseum in Oslo**. Organized by 'The Northern Space: Polar Research and the Nordic Nations', a research program sponsored by the Joint Committee of the Nordic Research Councils for the Humanities [NOS-H], the conference is devoted to humanistic polar studies. Among the themes to be discussed are Ritual & Ceremony: Celebrating Homecoming & Mourning Tragedy; Constructing Polar Heroes in Literature & Popular Media; Polar Experience and Official Memory: Exhibitions, National Histories, & School Books; Polar Lobbies: Science, Commerce, & Politics; Polar Nationalist Iconography; Gendering Arctic Nature & Polar Exploration; Depicting the North: Aesthetic Convention & Cultural Bias; and European vs. Indigenous Sense of Space and Landscape in the Arctic. Information may be obtained from Prof. Robert Marc Friedman, Voksenkollveit 128A, N-0394 Oslo, Norway, e-mail: robert.friedman@step.no

A *Science Peace workshop* will be held at the **University of Southampton**, UK, on Monday, 28 July 1997. Short Presentations will be made by David Bloor, Philosopher-sociologist of scientific knowledge, University of Edinburgh; Harry Collins, Sociologist of scientific knowledge, KES, Southampton University; Kurt Gottfried, Physicist, Cornell University (recent contributor to *Nature* on topic of Sociology of Scientific Knowledge (SSK)); Jay Labinger, Chemist, California Institute of Technology (Contributor to *Social Studies of Science* on topic of SSK); David Mermin, Physicist, Cornell University (extensive contributor to *Physics Today* on topic of SSK); Trevor Pinch, Sociologist of scientific knowledge, Cornell University; Arkady Plotnitsky, Analyst of scientific history and literature, Duke University; Simon Schaffer, Historian of science, Cambridge University. The meeting

will start at 11am and continue into the middle-evening, allowing time for members of the audience to travel to and from Southampton on the day of the workshop. The workshop has already received discussion in *Nature* (22 May, p 331). All communication and publicity for the meeting will be by email only. Registration: Send one pound sterling by cheque or cash to Professor H. M. Collins, KES, Department of Sociology and Social Policy, University of Southampton, Southampton SO17 1BJ, UK. Cheques should be made out to University of Southampton. Coins should be sellotaped securely into a sheet of folded cardboard. The equivalent of one pound sterling may also be sent in foreign currency - cash only. Registration after 14 July ten pounds sterling or equivalent. Post coinage to Professor H. M. Collins, KES (Centre for the Study of Knowledge, Expertise and Science), Department of Sociology and Social Policy, University of Southampton Southampton SO17 1BJ, UK; correspondence to h.m.collins@soton.ac.uk.

The Association for Research in Telecommunications History announces the *5th International Symposium on Telecommunications History* is to be held at the **Military Communications & Electronics Museum, Kingston, Ontario**, September 26-27, 1997. Persons interested in presenting a paper dealing with telecommunications history should write or fax Russell A. Pizer, 305 Cooper Road, North Babylon, NY 11703-4430, USA, fax 1 516 422 2324.

The History of Science as public culture?, the British Society for History of Science's 50th Anniversary Conference, will be held at the British Association Festival of Science, **University of Leeds**, 9-11 September 1997. It will explore the ways in which the enterprise of history of science has functioned in public culture and within academia, particularly in Britain during the past fifty years. The primary focus for the conference will be on

the role played by the history of science in public culture - in public debate, in public celebrations, in museums, and in visual and printed media. In addition, the conference will consider the extent to which scholars in other disciplines have responded to the history of science as an academic discipline, and will examine the British tradition of academic history of science from an international perspective. Register with BSHS Executive Secretary, Tel & Fax: +44 (0) 1367 718963, 31 High Street, Stanford in the Vale, Faringdon, Oxon, SN7 8LH, UK. Enquiries should be directed to Wing-Commander Geoffrey Bennett at bshs@hidex.demon.co.uk

Time, Heat and Order, Interpretations of thermodynamics, a conference on metaphysics and history of science and nature, will be held September 8th - 11th, 1997 at the **University of Aarhus, Denmark**. The aim is to bring together resources of metaphysics, history of science and social studies of science, and seek a coherent understanding of the double historicity of thermodynamics: historicity of nature, and historicity of science and technology. Contributors include Isabelle Stengers, Matthew Norton, Wise, Uffe Juul Jensen, Niels Viggo Hansen, Ole Knudsen, Simon Schaffer, Andrew Pickering, John B. Cobb, Jesper Hoffmeyer, Mary Midgley and Bruno Latour. For further information email Thermo-hist-97@hum.aau.dk, or contact "Time, Heat and Order", Department of Philosophy, University of Aarhus, Ndr. Ringgade, DK-8000 Aarhus C, Denmark, fax 45 8942 2223.

The Meanings of Practice: historical and sociological perspectives on the practices of science, technology and medicine, a conference co-sponsored by the Society for the Social History of Medicine and the British Society for the History of Science will take place in **Manchester** on Friday, November 14, 1997. Speakers include: Marc Berg, Harry Collins, Nick Hopwood, Michael Lynch, Paolo Palladino, John Pickstone, Steven Turner, Andrew Warwick. The aim of the meeting is to highlight this diversity, without necessarily seeking, probably impossibly, to

establish a single meaning of 'practice'. It might be more fruitful, instead, to consider the relationships between the diverse meanings by bringing together historians of sociologists and asking them to reflect and comment upon this diversity and the reasons for divergence. For further information, contact: Paolo Palladino, Department of History, Lancaster University, Lancaster LA1 4YG, UK, P.Palladino@Lancaster.ac.uk, 44 1524 592 793.

HOPOS '98, the second international history of philosophy of science conference, will be held at the Reilly Center for Science, Technology, and Values, **University of Notre Dame, Notre Dame, Indiana** on March 12-15, 1998. The conference will be open to work from all approaches in science studies that focus upon the history of philosophy of science. Abstracts of individual paper submissions should be between 250 and 500 words in length. Panel proposals should include one panel abstract, names and contact addresses of all participants, and abstracts of 250 words for each of three to four papers. All submissions should arrive by 1 September 1997. Preferred format for all submissions is plain ASCII text submitted by email to maffiej@spot.colorado.edu with "HOPOS Submission" in the subject line of the email. Other submissions should include three paper copies and one copy in plain ASCII format on a 3.5" DOS diskette and be sent to James Maffie, 3280 Sentinel Drive, Boulder, CO 80301-5498, USA, email maffiej@spot.colorado.edu. Registration is handled by HOPOS '98 Conference, Mrs. Harriet Baldwin, Center for Continuing Education, University of Notre Dame, Notre Dame, IN 46556, USA, email Harriet.E.Baldwin.1@nd.edu. More information may be found at the HOPOS website: <http://scistud.umkc.edu/hopos/index.html>

Philosophy of Science Association's Sixteenth-Biennial Meeting will be held on 21-25 October 1998 in **Kansas City**, October 21-25. Symposium proposals are invited.

Submissions should include (1) the title of the proposed symposium; (2) a description of the topic and a justification of its current importance to the discipline (about one or two pages); (3) titles and abstracts of papers; (4) a list of participants and either an abbreviated cv or a short biographical information file for each; (5) addresses of each participant, with the institutional affiliation, postal and email addresses, and telephone and fax numbers. Please indicate clearly the name of the organizer or contact person for the purposes of communication with the Program Committee. In addition to inviting proposals in traditional, core areas of the philosophy of science, the Program Committee wants also to encourage the submission of proposals in areas where the philosophy of science engages social issues in science or issues in science policy. Deadline: July 31, 1997. Direct submissions to Don Howard, Chair, PSA 1998 Program Committee, History and Philosophy of Science, 346 O'Shaughnessy, University of Notre Dame, Notre Dame, Indiana 46556 Tel. 1 219 631 5015, Fax 1 219 631 8209, email Don.A.Howard.43@nd.edu

The 1998 International Conference on STS in Japan, subtitled Science and Society, the Technological Turn, will be held in **Tokyo, Kyoto & Hiroshima, Japan**, on March 16-22, 1998. Contact the new conference office, STS International Conference Organizing Committee, c/o Bilingual Group Ltd., 2-7-22-2F Kudan-minami, Chiyoda-ku, Tokyo 102, Japan, tel 81-3-3263-1261 fax 81-3-3263-1264, e-mail jdn00050@niftyserve.or.jp. It's on the web at <http://hostcinf.shinshu-u.ac.jp/stsconfjp.html>

Intelligentsia and Power, the second international conference on the intelligentsia, organised by the Interregional Centre for Advanced Studies, will be held at the **State University of Moscow**, 5-9 September, 1997. The topics include the attitude of intelligentsia towards state power in different countries; the role of intellectuals in the preparation and implementation of political and economical reforms; Intelligentsia and the power of public opinion; and Intelligentsia in retrospective and

prospective. For more information, contact Conference Organizer: Prof. Alexander I. Studenikin at studenik@srldan.npi.msu.su, Interregional Centre for Advanced Studies, Skobeltsyn Institute of Nuclear Physics, Moscow State University, 119899 Moscow, Russia, tel 7 095 939 50 47, fax 7 095 939 08-96.

La Société Française de Physique will hold an international conference on *Radioactivity: History and Culture*, 1896-1930s on Monday, 7-9 July, 1997 at **Institut Curie**, 12 rue Lhomond, Paris. The conference program includes a visit to the Museum of the Radium Institute (Curie Museum), and a scientific tour of 'Pioneers of Radioactivity'. Contact the society at 33 rue Croulebarbe, 75013 Paris, tel/fax 33 3 44 27 44 48 or 33 1 44 08 67 10. One of the organisers, Pierre Radvanyi, can be reached at radva@frcpn11.in2p3.fr. Awards

Web news

The Journal of Electronic Publishing, a quarterly electronic-only publication, is at <http://www.press.umich.edu/jep>, ISSN 1080-2711. Published by the University of Michigan Press, it covers both scholarly and experiential aspects of the growing field of online publishing. JEP is offering free access to the site through the end of 1997. For the September issue, JEP is seeking essays by those who read e-journals and by those who do not, explaining their biases in a thoughtful, provocative, and readable way. Only opinionated writers will be considered. Contact Judith Axler Turner, Editor, judith@turner.net, or 1 202 986-3463

The former *Bulletin* of the German Working Group "Philosophy and Chemistry" has been extended to an online journal published at the University of Karlsruhe, Germany: *HYLE. An International Journal for the Philosophy of Chemistry*
<http://www.uni-karlsruhe.de/~philosophie/hyle.html>

HYLE is dedicated to all philosophical aspects of chemistry. Detailed informations concerning scientific concept, subscribing, and contributing to *HYLE* are available on the homepage. The former issues (1.1995, 2.1996) as well as a preview of 3.1997 are available. There is also a Collected Bibliography "Philosophy of Chemistry" (1700 titles) including articles on the history of theories, ideas, and concepts of chemistry. Web sites containing related links, new publications, and current activities in the philosophy of chemistry are in progress. Contact Dr. Joachim Schummer, Institute of Philosophy, University of Karlsruhe, Postfach 69 80, D-76128 Karlsruhe, Germany,

On-line History of Psychology Library Guide. James H Korn and Miriam E. Joseph of Saint Louis University have compiled a quite extensive list of library resources relevant to the history of psychology, and is available at: <http://www.slu.edu/colleges/AS/PSY/510Guide>

.html

George Herbert Mead Archive is at <http://paradigm.soci.brocku.ca/~lward/George2/frame2.html>.

The world wide web site for the International Society for the History of the Neurosciences (ISHN) is at <http://www.mednet.ucla.edu/som/ddo/bri/nha/is/hnhome.htm>. Features include the *Journal of the History of the Neurosciences* and the society's net forum.

ScienceNOW Online Magazine is available at <http://www.sciencenow.org>. It's an online news service of Science magazine, published by the American Association for the Advancement of Science. Contact Richard Stone, Editor, rstone@aaas.org.

HAYEK-L@MAELSTROM.STJOHNS.EDU is an international network for researchers doing work connected to the contributions of Friedrich Hayek. Hayek is the co-originator of the Hebb-Hayek synaptic learning model and author of the well known *The Sensory Order*. The basic purpose of the Hayek-L list is to serve as a forum for scholarly discussions and as a clearing house the distribution of information on academic conferences, publication opportunities, fellowship information, academic grants, and job openings of interest to Hayek scholars. Subscribers are encouraged to post questions, comments, or announcements of interest to individuals working on topics related to Hayek's writings. To subscribe to Hayek-L, send mail to: listserv@maelstrom.stjohns.edu with the message (body): SUBSCRIBE HAYEK-L yourfirstname yourlastname Further information may be obtained from Greg Ransom at gregransom@aol.com.

The Secular Web <http://www.infidels.org/> has classic, historical <http://www.infidels.org/library/historical/> and modern texts by freethinkers and other writers who pondered science, religion and related matters <http://www.infidels.org/library/modern/>

A new discussion list on Science and Technology Studies in Germany has been established by the Gesellschaft fuer Wissenschafts und Technikforschung. The GWTF aims to offer an arena for trans-disciplinary discussions across the borders between humanities and technical and natural sciences. 'GWTF-talk' is meant to be the primary media to this purpose. Its usual discussion language is German. To sign up send the following message: subscribe GWTF-talk YOUR EMAIL.ADDRESS to majordomo@majordomo.zedat.fu-berlin.de. Further information on the society may be obtained from Gerald Wagner at gerald@chem.uva.nl or Dr. Joerg Struebing,

Free University of Berlin, Institute for Sociology, Babelsberger Str. 14-16, 10715 Berlin, tel 49 30 85002 140; email jstrueb@zedat.fu-berlin.de.

CHOMSKY is an unmoderated discussion list intended as a resource for both intellectuals and followers of his political ideas and scholars doing research on Noam Chomsky's contribution to linguistics. The basic purpose of the list is to serve both as a forum for free political ideas and scholarly discussions on linguistics and as a clearinghouse for the distribution of information on academic and political conferences, publication, opportunities, fellowship information, academic grants, and job openings of interest to both Chomsky political intellectuals and scholars. To subscribe to Chomsky, send the following command to Listserv@maelstrom.stjohns.edu in the BODY of e-mail: SUBSCRIBE CHOMSKY yourfirstname yourlastname. For further information, contact the owner J.C. Garelli at <lagare@attach.edu.ar>