ADVANCES IN PHILOSOPHY OF TECHNOLOGY? COMPARATIVE PERSPECTIVES

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Advances in philosophy of technology? Addressing the central theme of this volume, I first ask myself whether there have been any advances in North American philosophy of technology in the last fifteen or twenty years. Attempting to answer this question, I discover—and report on—quite a few recent books and a few journal articles. In spite of this seemingly-significant flood of publications, however, critics have questioned whether any *significant advances* are being made in these admittedly numerous books and articles.

Joseph Pitt, past president of the Society for Philosophy and Technology, quotes friends of his in the Society for History of Technology as reacting with horror to a proposal for a joint meeting: "Oh, no! Those SPT people hate technology. Further, they know nothing about technology" (Pitt, 1995). Philosophers of technology, in this view, are certainly not making any advances—at least, not any advances that would mean anything to people outside this would-be field.

This raises the obvious question: What counts as a genuine *advance* in technology studies? And the view or thesis that I want to defend here is this:

In all respects except one, *advances in the philosophy of technology are approximately equal, in their progressiveness, to progress in the fields with which those advances have been negatively contrasted*—namely, the philosophy of science and social studies of science and technology. (The exception is important, since I consider it the most important area of advance.) In my conclusion, I make some comments about *all* of these fields, including philosophy of technology, contrasting academic with real-world social progress.

ADVANCES IN NORTH AMERICAN PHILOSOPHY OF TECHNOLOGY

I begin with the best evidence there is to support a claim that there have been advances in the philosophy of technology in the USA and Canada. To support such a claim, I point to the work of the North American philosophers who traveled to the first international conference of the Society for Philosophy and Technology in Bad Homburg in 1981 and whose papers were printed in the proceedings volumes, *Technikphilosophie in der Diskussion* (1982), and *Philosophy and Technology* (1983)—both edited by Friedrich Rapp and myself. At least six of the North Americans invited to Bad Homburg can be cited in support of the claim that there are continuing advances, right up to the present. I have in mind Stanley Carpenter, Don Ihde, Alex Michalos, Carl Mitcham, Kristin Shrader-Frechette, and Langdon Winner. (I set aside my own case for now, not out of modesty but because I want to make a separate point at the end.) To these six can be added one other philosopher at Bad Homburg, Bernard Gendron—not in terms of his own later work but viewing his as a springboard to the later development of that part of the environmental ethics movement that has a close relationship to technological issues—and also Albert Borgmann, who was not at Bad Homburg but whose thought has undergone development in ways that have led people to say that his work represents the first real tradition in North American philosophy of technology.

Stanley Carpenter came to Bad Homburg at least partly on the basis of a book that he had co-edited (with Alan Porter, Alan Roper, and Fred Rossini),*A Guidebook for Technology Assessment and Impact Analysis* (1980). At the conference, Carpenter's contribution was listed under the technology assessment heading, but his interests were already oriented toward environmental concerns, and focused particularly on ways in which an "alternative" or "appropriate" technology is necessary if the ecosystem is to be preserved. Carpenter has not so far produced another book after Bad Homburg, but he has been a regular participant in the series of Society for Philosophy and Technology international meetings that continues today. For instance, at the 1993 SPT conference near Valencia, Spain, Carpenter presented a paper, "When Are Technologies Sustainable?" Again, at the 1996 conference in Puebla, Mexico, his topic was similar: "Toward Refined Indicators of Sustainable Development."

Don Ihde had also written a book on philosophy of technology before Bad Homburg, *Technics and Praxis: A Philosophy of Technology* (1979), but his case differs from that of Carpenter in two respects: he has written several more

books, and he is the editor of a philosophy of technology book series published by Indiana University Press. The first book published in that series, Larry Hickman's *John Dewey's Pragmatic Technology* (1990), shows that Ihde was not interested, in the series, in pushing his own phenomenological approach to philosophy of technology, but is open to a variety of approaches. Ihde's own approach does show up in his later books, *Existential Technics* (1983), *Consequences of Phenomenology* (1986), and *Technology and the Lifeworld: From Garden to Earth* (1990)—even in his *Philosophy of Technology: An Introduction* (1993), though that textbook does present other views. In general, one can say that Ihde's development is a matter of greater depth and clarity in his phenomenological analysis, though *Technology and the Lifeworld* gives more than a passing nod to the centrality of environmental concerns.

Alex Michalos talked about technology assessment at Bad Homburg, but he had been invited at least in part because of his editing of the journal, *Social Indicators Research*, which is devoted in large part to quality-of-life measurements in our technological culture. Michalos has continued these efforts in a massive way, with his five-volume *North American Social Report* (1980-1982) and his four-volume *Global Report on Student Well-Being* (1991-1993), and with regular contributions to all sorts of conferences devoted to various aspects of measuring the quality of life today.

Carl Mitcham's contribution to the Bad Homburg proceedings focused on what he called "the properly philosophical origins" of modern technology, as opposed to the more commonly-discussed social or economic or scientific origins. And this metaphysical/religious approach to the understanding of technology both reflected Mitcham's earlier work—in the two volumes he compiled with Robert Mackey, *Bibliography of the Philosophy of Technology* (1973, which cites other approaches but gives heavy emphasis to the metaphysical/religious), and *Philosophy and Technology: Readings in the Philosophical Problems of Technology* (1972; reprinted with revised bibliography, 1983)—and presaged his later work, *Thinking through Technology: The Path between Engineering and Philosophy* (1994). Many reviewers have applauded this as Mitcham's masterpiece and as the first true summary of the development of the field.

Kristin Shrader-Frechette's first major work, *Nuclear Power and Public Policy*, appeared in 1980. In later books, she has addressed *Risk Analysis and*

Scientific Method (1985) and *Risk and Rationality* (1991). These and others of her publications are always masterpieces of clarity and precision--no matter whether the risk analysts she attacks appreciate her criticisms or not. In my opinion, Shrader-Frechette's most interesting book to date is *Burying Uncertainty: Risk and the Case against Geological Disposal of Nuclear Waste* (1993). There all her skills as an analyst and arguer are on display as much as ever; and the comprehensiveness of her survey of arguments on all sides is admirable. But what makes me admire the book more than anything else—and more than her earlier contributions—is her new-found awareness of how enormous the pressure is in technical communities to ignore, and resist, the force of her arguments, no matter how clear and convincing.

Langdon Winner's contribution to the Bad Homburg conference, "Techne and Politeia: The Technical Constitution of Society," follows up on his themes in *Autonomous Technology: Technics-out-of-Control as a Theme in Political Thought* (1977). A typically Winnerian gem of an essay, "Techne and Politeia" was used many times in many arenas, and shows up in Winner's later collection of essays, *The Whale and the Reactor: A Search for Limits in an Age of High Technology* (1986). It is probably Winner more than any other single author whom historians and sociologists of technology love to hate, and he has returned the favor in, "Upon Opening the Black Box and Finding It Empty: Social Constructivism and the Philosophy of Technology" (1991), his presidential address at the 1991 SPT conference in Puerto Rico.

Bernard Gendron's Bad Homburg paper, "The Viability of Environmental Ethics," suggests another progressive path in the history of the philosophy of technology in the last fifteen years. In 1989 and 1992, Eric Katz published two excellent annotated bibliographies of environmental ethics in *Research in Philosophy and Technology* (volumes 9 and 12), and the theme of volume 12 is technology and the environment. Many younger philosophers associated with SPT have taken up this theme, notably David Rothenberg, in *Hand's End: Technology and the Limits of Nature* (1993)—where Rothenberg argues against setting up any opposition between human, including technological, civilization and nature; David Strong, in *Crazy Mountains: Learning from Wilderness to Weigh Technology* (1995; here Strong tries to heed Rothenberg's message but ends up seeing many more positive features in natural wilderness than in today's consumer-oriented technological society); and Eric Katz (again), in *Nature as*

Subject: Human Obligation and Natural Community (1997). There Katz argues against applications of traditional ethical theories to environmental problems, as the right approach, and in favor of a more radical "moral justification for the central policies of environmentalism" in terms of "the direct moral consideration and respect for the evolutionary processes of nature" (p. xvi). Katz has also teamed up with Andrew Light in the editing of *Environmental Pragmatism* (1996)—a collection dear to my heart because the essays collected generally argue that we should go beyond theoretical debates to a discussion of real environmental issues and even more toward attempts to work out (with others) solutions for real environmental problems.

Albert Borgmann was not at Bad Homburg, but his thought has been viewed by some as the only contribution to philosophy of technology that has given rise to its own tradition or school of thought. Borgmann published *Technology and the Character of Contemporary Life*, his neo-Heideggerian manifesto, in 1984. This was followed by *Crossing the Postmodern Divide* in 1992. David Strong's *Crazy Mountains*, mentioned earlier, is an explicit attempt to apply Borgmann's theses in an effort to arrive at a philosophy of wilderness in the midst of—and as confronting—technological culture. In 1995, a group of Borgmann disciples convened a conference, "Workshop on Technology and the Character of Contemporary Life," in Jasper National Park in Canada. Approximately twenty philosophers attended—some disciples, some critics—and Borgmann concluded the meeting with a thoughtful reply to his critics and some reflections on the future of philosophy of technology. The organizers expect to publish a volume based on the proceedings.

COMPARATIVE PERSPECTIVES

Everything I have summarized so far in support of a claim that there have been advances in North American philosophy of technology since Bad Homburg is, actually, preparatory to the question I want to address in this paper. It should be obvious that there has been progress in the field of philosophy of technology in *some* sense. But exactly what do we mean when we speak of "advances," whether in the philosophy of technology or in any other similar field today? Is it just a matter of a continuing stream of new books and new journal articles

published? I want to address this issue comparatively, by way of a comparison and contrast with developments in the philosophy of science and the sociology of science and technology.

First, however, we need some definitions of what it may mean to speak of advancing or making progress in any academic field.

Discussing the rise of analytical philosophy in the early twentieth century, Bertrand Russell (1945, p. 834) once claimed that, using logical techniques, analytical philosophy is "able, in regard to certain problems, to achieve definite answers" (in contrast with older philosophical approaches); in this respect, Russell claimed, analytical philosophy's methods "resemble those of science." Like scientific advance, Russell was assuming, there can be similar philosophical progress, with one contribution building on others, and so on. In the United States at least, this has become the ideal of academic progress, with one article in a "leading" journal in a "cutting-edge" field worth more, in terms of merit and reward, than any other kind of publication—except possibly a "major" book reviewed (favorably) in all those leading journals.

However, once this academic standard of progress was extended, by departmental committees and deans, to almost every field of higher learning, it began to come under attack. An early and vituperous version can be seen in Jacques Barzun's *Science: The Glorious Entertainment* (1964). These critics maintain that, when the standard is applied in humanities fields such as literature, history, and the arts—and many of the critics lump philosophy together with other humanistic disciplines—it is totally inappropriate. The only measuring rod we can use in these fields (and, as we will see below, later postmodern critics now say this is true even in the sciences) is greater and greater originality, especially in terms of persuading whatever are perceived to be the relevant audiences.

A few transcendentalist metaphysicians and theologians object to both the strict (progressive) academic standard and the much broader "originality" (postmodern?) standard as retrogressive chasing after increasingly trivial minutiae. The only *real* progress moves in the opposite direction, toward more and more comprehensive syntheses—ever closer approaches to truth or beauty or goodness (sometimes capitalized as Truth, Beauty, and Goodness). Such Hegel-like synthesizers are, I admit, rare today; but there are "right-side-up" dialectical

materialist neo-Hegelians and others who insist on *real social progress* as the only appropriate standard. (I will return to this at the end of the paper.)

Finally, still others insist on what I would call an Aristotelian model, recognizing that academic fields are divided along disciplinary lines, each with its own standards. At least some of the sciences may meet the standard criterion of progress within limited domains, but most intellectual endeavors can make only "intensive" or "qualitative" progress, providing no more than a deeper appreciation of, or new insights into, old truths, traditional arts and crafts, and so on.

We can now ask whether, in the past twenty years or so, there has been progress, in any of these senses, in philosophy of technology or in such allegedly more progressive fields as the philosophy of science and the sociology of science and technology.

PHILOSOPHY OF SCIENCE

I take as my starting point for comparison here the (U.S.) Philosophy of Science Association's collaborative volume, *Current Research in Philosophy of Science* (1979), edited by Peter Asquith and Henry Kyburg. Two articles in the book are illustrative: Noretta Koertge's "The Problem of Appraising Scientific Theories" (pp. 228-251) and Ronald Giere's "Foundations of Probability and Statistical Inference" (pp. 503-533).

Koertge says, "Philosophers of science [especially Popperians] have made considerable progress in providing clear accounts of how to appraise the content and the test record of a theory"—and the series of citations she lists may seem impressive to at least sympathetic readers (though Koertge also adds immediately, "They have had much less success in explicating complicated mixed appraisals"—p. 246).

Giere says, "The development and consolidation of the 'subjective' Bayesian account of statistical inference during the past twenty-five years has been a remarkable intellectual achievement" (p. 508). This, however, must be balanced against Giere's claim less than a decade later, in what can only be called a philosophical "conversion" to "naturalized epistemology":

> My skepticism [has] progressed to the point that I now believe there are no special philosophical foundations to any science [or, in the example above, statistical inferences in science]. There is only deep theory, which, however, is part of science itself. And there are no special philosophical methods for plumbing the theoretical depths of any science (*Explaining Science: A Cognitive Approach*, 1988, p. xvi).

As evidence of the current state of philosophy of science in the USA, I can cite two recent books: Robert Klee's *Introduction to the Philosophy of Science: Cutting Nature at Its Seams* (1997), and Joseph Rouse's *Engaging Science: How to Understand Its Practices Philosophically* (1996).

Klee's exciting and challenging introductory survey of everything that has happened in the philosophy of science since the 1930s ends with a chapter on the realism-antirealism debate. At the end, Klee says, "I have never tried to hide from the reader my realist leanings" (p. 239), and the main sources he appeals to are articles by Ian Hacking (1983), Richard Boyd (1984), and Richard Schlagel (1991). Antirealists referred to are Bas van Fraassen, in his *The Scientific Image* (1980), and Larry Laudan and Arthur Fine in articles included in Jarrett Leplin's *Scientific Realism* (1984). Though Klee seems to be up-to-date in his sources, an attentive reader will note that the articles cited are not much more recent than *Current Research* (1979); and the mere fact that Klee ends with a debate as old as that on realism versus antirealism should give one pause. Even when (in another chapter) Klee cites a clearly progressive claim—in Wesley Salmon's "Four Decades of Scientific Explanation" (1989)—the reader can quickly check Joseph Hanna's "An Interpretive Survey of Recent Research on Scientific Explanation" in *Current Research* and see that Salmon has added little new in the intervening decade. And Hanna admits that there has only been limited ("intra-paradigmatic") progress within several different and competing approaches.

Rouse's book is, if anything, even more troublesome for anyone claiming that recent philosophy of science has been progressive. Rouse mounts a detailed attack not only on realism but also on its opponents—he discusses in detail Larry Laudan (1984), Dudley Shapere (1984), Richard Miller (1987), and Peter Galison (1987), not to mention Arthur Fine (1986), who is analyzed and critiqued in chapter after chapter, and a whole raft of social constructionists, but particularly

Harry Collins (1992)—all in the name of "cultural studies of science," with a heavy dependence on such feminist critics of science as Donna Haraway. Though Rouse is extremely careful about uses and misuses of the "postmodernist" label, his book is intended to be a contribution to the right kind of postmodernist critique of scientific progress claims.

Deans and promotion committees are likely to continue to accept publication in *Philosophy of Science* and similar journals as unquestionable evidence of contributions to the advancement of philosophy of science. But as soon as anyone actually reads the articles published there, he or she will see that their authors have no illusions that the field is any longer even cohesive, much less progressive in the narrow sense.

FROM SOCIOLOGY OF SCIENCE TO SOCIOLOGY OF SCIENTIFIC KNOWLEDGE (SSK)

According to one source (Gaston, 1980), sociology of science as a subspecialty within sociology only dates back to the 1950s. From the mid-fifties until 1980, the field was dominated by one giant figure, Robert K. Merton—though his *On the Shoulders of Giants* (1965) is an eloquent defense of the claim that intellectual originators, no matter how creative they may seem, always owe enormous debts to those who have gone before them. Between the 1950s and the late 1970s, almost all sociologists of science felt that they owed a major debt to Merton. His model of objective science as requiring the sharing of information, mutual criticism, disinterestedness, and universalism (disregarding social characteristics in the recognition of the importance of contributions to science) became the basis of other sociologists' research. As Gaston summarizes the situation: "The model of a social system of science in which scientists pursue knowledge in a social environment, hoping and expecting to receive recognition for their original contributions, provides a multitude of research questions—what has come to be called 'Mertonian' sociology of science" (Gaston, 1980, p. 475). This approach continues to have its followers—most notably in the various forms of the *Science Citation Index* and cognate series—but hardly anyone today thinks of this tradition when referring to advances in social approaches to the study of science.

In 1979, Bruno Latour and Steve Woolgar published *Laboratory Life:*

The Construction of Scientific Facts, and a new tradition was launched. One of its principal aims was to undercut the Mertonian model and the positivist philosophy that was perceived to lie at its core. Since then, the "sociology of scientific knowledge"—as the field was renamed to emphasize its focus on the actual doing of scientific work rather than on allegedly authoritative products of successful scientific work—has been perceived by almost everyone in science and technology studies as one of the most prolific, rapidly advancing fields in all of academia. Joseph Rouse dates the revolution from the so-called "Edinburgh Strong Programme," associated especially with the names of Barry Barnes (1974) and David Bloor (1976), and he goes on to list the fragments of later social constructivism as including "Bath relativism, ethnographic studies, discourse analysis, actor/network theory, and constitutive reflexivity" (Rouse, 1996, p. 1). But he and nearly every other commentator treats constructivism as an advancing—if not monolithic—field. Indeed, nearly everyone who is not unalterably opposed to it (see Gross and Levitt, 1994) thinks of the constructivist school(s) as advancing at an amazing pace.

What I want to do here is contrast later with earlier stages of one of these strands, laboratory studies. If we date this subspecialty in constructivist studies from Latour and Woolgar's *Laboratory Life* (1979), it is fairly easy to demonstrate that there have been a large number of later developments building on earlier ones. In Karin Knorr Cetina's summary in the *Handbook of STS* (1995), the developments extend Latour and Woolgar's examples, from Eisenstein (1979) on the printing press as a social agent of change, to Amann and Knorr Cetina (1990) on image interpretations in molecular biology, to Henderson (1991) on computer graphics, to Hirschauer (1991) on sex-change surgery—to broader sets of examples in Lynch's *Art and Artifact in Laboratory Science* (1985) and Latour's *Science in Action* (1987). (See Knorr Cetina, 1995, p. 155.) Indeed, it sometimes seems that any adequate list would be too long to summarize. (Knorr Cetina tries, in her 1995.)

It would take a churlish critic to deny that there has been progress here—and I have not even referred to advances in actor/network theory and similar approaches.

Nonetheless, even Knorr Cetina as the loyal chronicler of these advances admits that her favored approach, laboratory studies, has its limits. The most

important ones she lists have to do with their microscopic focus on individual laboratories rather than on consensus building among larger groups of scientists; and with their failure to account for larger societal contexts that influence laboratory life (Knorr Cetina, 1995, pp. 161-162).

And of course this does not even mention criticisms by jealous defenders of science's progressivism (Gross and Levitt, 1994), who view what is alleged to be progress here as no more than an ever-broadening smear campaign against more and more hardworking scientists.

In concluding this section, it seems fair to say that advances in laboratory studies continue right down to the present; but it is also fair to say that such studies have their limits and their critics.

SOCIAL CONSTRUCTIVIST STUDIES OF TECHNOLOGY

Moving closer to a direct parallel to philosophy of technology, several sociologists (and sociologically-oriented historians) in the mid-1980s extended their constructivist studies, in an explicit way, to the study of technology—usually, of particular technologies.

It was this group of scholars whom Winner was attacking in his paper, "Upon Opening the Black Box and Finding It Empty" (1991). And representatives of this school have fought back. (See Bijker, 1993, and Aibar, 1996.)

Wiebe Bijker, in his summary of developments in the field in the *Handbook of STS* (1995), traces its roots to Thomas Hughes, the historian, in his masterly study, *Networks of Power: Electrification in Western Society, 1880-1930* (1983). Hughes then combined with Bijker and Trevor Pinch to edit the book that others often list as the beginning of the new tradition, *The Social Construction of Technological Systems* (1987). That does not leave much time for a great deal of development between 1987 (or even 1983) and Bijker's summary (1995). Nonetheless, people do perceive the constructivist study of technological systems as a rapidly advancing field.

But what kind of advance has there been? Bijker and John Law, in

Shaping Technology/Building Society (1992), offer an answer. According to them, technology studies had earlier been "fragmented":

> There are internalist historical studies; there are economists who are concerned with technology as an exogenous variable; more productively, there are economists who wrestle with evolutionary models of technical change; there are sociologists who are concerned with the "social shaping" of technology; and there are social historians who follow the heterogeneous fate of system builders (p. 11).

By the end of the book—which summarizes the evidence in a somewhat heterogenous collection of essays, though written by leading figures in the field—Bijker and Law conclude that a "first step" has been taken in understanding "that technical questions are never narrowly technical, just as social problems are not narrowly social" (p. 306).

Back in the introduction, Bijker and Law had summarized the progress made so far:

> The last five years has seen the growth of an exciting new body of work by historians, sociologists, and anthropologists, which starts from the position that social and technical change come together, as a package, and that if we want to understand *either*, then we really have to try to understand *both* (p. 11).

In short, all that Bijker and Law are claiming as advances in the new field so far is that there has been a "development of an empirically sensitive theoretical understanding of the processes through which sociotechnologies are shaped and stabilized" (p. 13). But everyone knows that theoretical arguments are never-ending, and if there is to be any progress in this new field it will show up in detailed studies that confront theory with evidence. And Hughes had already displayed that process admirably, in *Networks of Power*, in 1983.

So where do we stand at this point in our comparative survey?

The new sociology of scientific knowledge, especially laboratory studies, comes closest to the ideal of science-like progress, with one article building on others in continuous advance. Paradoxically, however, these studies are narrow and limited, and defenders of science maintain that, cumulatively, they serve to undermine scientific progress and give comfort to the enemies of science.

Studies in the new social contructionist approach to technology have so far seen only theoretical advances—and every new theoretical formulation is met with challenges, even within the field.

Philosophy of science today is a battleground, fragmented and splintered not only into subspecialties, but also setting modernists against postmodernists in seemingly endless variations. So what started out as the most progressive of science studies fields, in the narrow sense, now shows advances only in specialty areas and within particular paradigms.

Citation indices document all of these advances, along with advances in the sciences themselves, but nearly everyone treats them as raw data awaiting a theoretical interpretation.

And what about philosophy of technology? I think the evidence I displayed at the outset supports the claim that our field is just about as progressive (or lacking in progress in the narrow sense) as any of the comparator fields discussed here.

CONCLUSION

Are there, then, *no* advances in science and technology studies—or at least none that go beyond qualitative change? I will conclude that real though limited progress has been made during the years surveyed here, but it is not in the academic sense implicit in our title, "Advances in the Philosophy of Technology." To make this point, I will quote Bijker and Law at the end of *Shaping Technology/Building Society* (1992):

> When things go wrong, it may not make much sense to blame technologies. Neither does it necessarily make sense to blame people, nor even . . . economic systems. . . . If we want to make sense of [technological] horrors—and more important, do

> something about them—. . . what we urgently need is a tool kit . . . for going beyond the immediate scapegoats and starting to grapple with and understand the characteristics of heterogeneous systems (p. 306).

To which I would say amen, but especially to the phrase, "more important, do something about them." Surely we do need theoretical advances, but even more surely we need to make more progress in *solving the real-world problems of our technological society*.

In the very first volume of *Research in Philosophy and Technology* (1978), I argued for a social action approach to philosophy of technology (following the lead of the American Pragmatist philosophers, George Herbert Mead and John Dewey). I repeated that call to action at the Bad Homburg meeting. And I made my most extensive appeal in *Social Responsibility in Science, Technology, and Medicine* (1992). I believe that progressive activists have been making progress in solving technosocial problems (see McCann, 1986), and there is no reason why philosophers and other academics cannot join with them. At Bad Homburg, I quoted our German colleagues, Hans Lenk and Günter Ropohl:

> The multidisciplinary and systems-like interlocking of techn(ologi)cal problems requires . . . the interdisciplinary cooperation of social science experts and generalists, . . . systems analysts and systems planners. Philosophy has to accept the challenge of interdisciplinary effort. . . . It has to step out of the ivory tower of restricted and strictly academic philosophy (Durbin, 1983, p. 2).

But we must take this plea quite literally, and cooperate not merely with other experts; we must also cooperate with all sorts of citizens of good will who are seeking progressive solutions for serious contemporary social problems.

And we must hope that philosophers of science and academic philosophers of technology and sociologists of science and students of the social construction of technology will do likewise. It is important to understand sociotechnologies, but it is more important to *do something* about the social

problems associated with them.

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