



## “So ... who is your audience?”

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### Abstract

To whom, if anyone, are the writings of philosophers of science relevant? There are three potential groups of people: Philosophers, Scientists, and Interested Citizens, within and beyond the academy. I argue that our discipline is potentially relevant to all three, but I particularly press the claims of the Interested Citizens. My essay is in dialogue with a characteristically insightful lecture given thirty years ago by Arthur Fine. Addressing the Philosophy of Science Association as its president, Fine argued that general philosophy of science was dead, and that all the action lay in the philosophy of the special sciences. I try to identify what was correct about Fine’s diagnosis, while supplementing his message by describing fruitful projects that have since emerged. I also hope to share his subversive spirit.

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I have a friend, a Shakespearean scholar, whose books are not only acclaimed by his fellow academics but avidly read by many other people. Sometimes, when we meet for a cup of tea or a drink, we talk about our current research topics. I will sometimes explain what I am thinking and writing about, and he will listen sympathetically. His own work inclines him to consider how professors might reach a wider public. And, often, he will lean back and ask ... the question I have quoted in my title. “So ... who is your audience?”

I hate the question. As I’ve sometimes told him, I conceive my writing differently. There are ideas and arguments I want to explore, debates I hope to resolve. My first job

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is to do justice to the ideas and the reasoning. Once I have that straight, I try to make them as clear and as accessible as I can. I would like as many people as possible to gain a better understanding of the issues with which I'm concerned. But the articulation of the ideas, the generation of the understanding comes first. The audience will be determined by my ability, first to figure things out, and second to be as clear as possible about what I've done.

My friend isn't convinced. He finds this a curious way to think and write. Much as I dislike his question, I've come to think that he has a point. So today, I want to explore it, both for myself and also for (and with) all of you.

The trouble is that there's a skeptical worry in the neighborhood of my title question. "Is there *any* audience for the issues that interest you, *any* group of people for whom the ideas and arguments you struggle to clarify are valuable or important?" It's easy to dismiss that form of skepticism, to see yourself as pursuing inquiries that are intrinsically significant, even "fundamental" and "timeless". But I think the skeptical voice deserves a hearing. A century ago, John Dewey worried that many of his fellow philosophers were "socially absent-minded men" pursuing "intellectual busywork". Like him, I don't want philosophy to become "a sentimental indulgence for the few" (Dewey 1916, 338).

A few years ago, I wrote an article about this danger (Kitcher 2011). In it, I compared some of our professional discussions and printed exchanges to a musical culture that had set up competitions to see who could perform etudes of maximal difficulty at maximal speed, and that disparaged those who continued to play the standard (much-loved) repertoire of sonatas and concertos. My aim today is a little different. Philosophy of science strikes me as a relatively healthy part of philosophy. So I am not in the business of delivering a jeremiad or a lamentation. Instead, I hope to offer a positive answer to the skeptic. Whether you will like my answer remains to be seen. But, even if you don't, I'd like to prod you to produce an answer of your own.

There are three obvious responses to the question "Who is the audience for work in the philosophy of science?" The first proposes that philosophers of science can deliver something of value to philosophy; the second suggests that the practices of the sciences can benefit from philosophical reflection on them; the third views philosophy of science as reaching out beyond the academy and helping the general public to think through issues that are important for individual lives and for the health of human societies; perhaps this is sometimes best achieved through a direct influence on journalists or on policy-makers. I shall want to consider all three of these possibilities, but, before I do so, it's worth reflecting on the history of our subject, and considering how the question would have been answered at various stages.

I'll start with a group of thinkers who have left an enduring imprint on the way in which philosophy of science is done. Many of the questions we continue to discuss were posed by the philosophers displaced in the 1930s from Vienna and Berlin – Carnap, Reichenbach, Neurath, Feigl, Hempel and others. Initially, under the aegis of logical positivism, they took all philosophy to be "the logic of the sciences". The goal of philosophical work, pursued in the various volumes of the *Encyclopedia of Unified Science*, was to clarify concepts and expose structures of justification across a wide range of fields of inquiry. The grand project presupposed a principle, the criterion of cognitive significance, and, when the difficulties of formulating that criterion precisely became evident, logical positivism metamorphosed into logical empiricism.

Before I consider that transition, let's ask how the positivists would have identified their audience. They'd have emphasized two out of the three possibilities I mentioned. Not the first, of course. Traditional philosophy is contentless, the pursuit of pseudo-questions (Carnap 1928). That leaves no independent discipline to be enlightened by the philosophy of science. The second, however, is especially important. Recall that the logical reconstructions of parts of science were offered at a historical stage when the principal foci of the philosophical reconstruction were relatively new. The theory of relativity and quantum mechanics were recent theories, each of them posing difficult scientific questions. Philosophical reconstructions were intended to help clear up some of the scientific puzzles. And they did. Axiomatization enabled practitioners to see the conventionality of choice for the one-way velocity of light (Winnie 1970) and to recognize the equivalence of different formulations of quantum theory.

But this was by no means the only way in which philosophy of science aided the sciences. I read the *Encyclopedia* as a missionary effort. Through showing the structures of the well-developed sciences, and reviewing the state of the less-developed ones, the hope was to foster fields of inquiry that were only beginning to find their way. A general picture of how successful science worked would help the psychologists and the anthropologists and the linguists; the ideas of some researchers were affected by interactions with philosophers – witness the articles of Paul Meehl and the long-enduring impact of the covering-law model of explanation within archeology. Moreover, beyond the scope of my second possible answer, there would be illumination for a broader public. Non-scientists would understand the special credentials of the sciences. Their admiration might even translate into efforts at emulation. And, perhaps, the logical reconstructions would prove useful in showing how best to educate the young.

The transition from logical positivism to logical empiricism (signaled by the abandonment of any formal criterion of cognitive significance) preserved many of these aims, but modified the priorities. Hempel's classic articles on the problems encountered in trying to formulate a precise criterion of cognitive significance conclude by setting a new agenda for the philosophy of science (Hempel 1950, 1951). The old conception of a sharp distinction between the cognitively meaningful and the meaningless gives way to a graded account of the differences among various putatively cognitive ventures. Areas of inquiry differ with respect to their capacities to develop genuine theories, their ability to provide explanations, and the extent to which the claims made by their practitioners are supported by evidence. Hence arise three of the four main enterprises Hempel projects for logical empiricist philosophy of science: to find general accounts of theory, explanation, and confirmation. (The fourth, the explication of simplicity mostly fell by the wayside.) From 1950 to the present, logical empiricist philosophy of science – much of *general* philosophy of science – has pursued these tasks, and others that have spun off from them (for example, an account of laws of nature, and, most recently, an account of models and their functions).

The priorities shifted in that the focus on theory, explanation, and confirmation became seen as central to the missionary role of aiding the relatively undeveloped areas of inquiry. During the 1950s and 1960s, less emphasis was placed on reconstructing prominent achievements in the special sciences. Philosophy of science seemed to speak mainly to scientists by offering a richer account of the distinctive qualities that had made the natural scientists so spectacularly successful (by explaining scientific

explanation, for example). By doing so, it also offered an image of the sciences to a broader public, one that was occasionally influential in debates and discussions.

Why ‘occasionally’? Because, for all its orthodoxy among philosophers of science, the logical empiricist picture of axiomatic theories, tied to observational claims through correspondence rules, offering explanations through subsumption under general laws, and confirmed in ways that would be articulated in some still-to-be-completely-developed inductive logic or confirmation theory, was never the most popular account with the general public, with journalists, or even with practicing scientists. That honor fell to the rival approach proposed by Karl Popper (Popper 1934). The concept of falsifiability captured the imagination of a broad audience – as witnessed by the frequency with which it has been wheeled out as the hallmark of genuine science, even in famous court decisions (most notably in the Arkansas creationism trial in 1981).

Moreover, the potential influence of the logical empiricist picture was further diminished by the developments of the 1960s and 1970s. Attempts to answer the general questions about the sciences posed by the logical empiricists led a group of philosophers – Hanson and Toulmin, as well as Feyerabend and Kuhn – to consider the historical details of the classic achievements. They replaced the comforting legends of standard presentations with accounts that were far more informed and nuanced. And far more disturbing. Kuhn’s monograph (Kuhn 1962), of course, was the final volume in the *Encyclopedia of Unified Science*, and it was well received by many scientists, who saw in the concept of normal science a far more accurate presentation of their activities than anything the previous orthodoxies had been able to offer. Much to Kuhn’s regret, ‘paradigm’ slid smoothly into public discussions. To this day, the most prominent general image of science, among outside commentators and scientific researchers, blends Popper with Kuhn.

The only people bothered by references to ‘faith’ and ‘conversion experiences’ were, apparently, philosophers. From the late 1960s on, a principal task of philosophy of science was to show how the historical details might be accommodated without abandoning the conception of the natural sciences as special – as “rational” and as “progressive” (Scheffler 1967; Lakatos 1970; Laudan 1977). The task seemed urgent. Perhaps general attitudes towards science would be corrupted by the awful bogey Kuhn-Feyerabend – a chimera constructed by people (including me) who failed to recognize the important differences between these two thinkers. And it got worse. Feyerabend was very clear that his ideas were different from Kuhn’s, and his presentations became ever more flamboyant – and more entertaining (Feyerabend 1978, 1987). Meanwhile, the historicists had inspired other radical developments, including a sociological parade begun in Edinburgh and extending through Bath and Paris (Barnes 1977; Collins 1985; Latour 1987). Apparently, our public discussions needed an image of scientific research that would preserve the proper respect for the special accomplishments of the natural sciences. Yet, even when a few professional scientists paid attention, when Gross and Levitt and Sokal declared the science wars, the trenches were dug and the shells fell within the walls of the academy. A dash of Popper and a soupçon of Kuhn satisfied the journalists who wrote about science, as well as their readers.

Of course, there were rumblings about the credentials of particular sciences. Particularly evolution. But those who debated largely adopted an image of science as

a special enterprise. Scientists trotted out their Popper-Kuhn, philosophers tried their favorite emendations – more complex, definitely arcane, but more likely consistent. The more general doubts about science – about having “too much of experts” – are a later phenomenon, for which the supposed heretics – Feyerabend or Latour, say – are utterly blameless. (To the best of my knowledge, nobody has traced a line of intellectual development that leads from the writings of these authors to the utterances of Michael Gove or of climate deniers.)

Yet, from the 1960s on, there have been other important developments in our field. First is a return to studies of special sciences, now on a far wider scale. David Hull and Michael Ruse founded a new philosophy of biology; Merrilee Salmon and Alison Wylie ventured into archeology; Dan Hausman, Mary Morgan and Nancy Cartwright turned philosophical attention on economics; a host of scholars reinvigorated earlier attempts to study psychology; Patricia Churchland campaigned for neurophilosophy; and, even within physics, *the* science from the classical logical empiricist perspective, attention turned to a far wider range of areas, often centering on the previously understudied uses of experiment. Thirty years ago, in an important Presidential address to the Philosophy of Science Association, Arthur Fine suggested that general philosophy of science was dead (Fine 1988). It had given rise to something more profitable, namely philosophical study of the special sciences.

Fine refined the logical empiricist answer to the audience question. Forget about providing a general image of the sciences, either for practicing scientists (who almost certainly don't need it) or for the general public. Philosophy of science earns its keep by fostering the development of the particular sciences. That can occur through helping to clarify particular disputes or to reconstruct parts of a scientific theory so as to aid research. It can also happen through the elaboration of new methods for testing or justifying specific types of hypotheses.

The refinement tacitly absorbs another important evolution in the philosophy of science. Thanks to the “Stanford school”,<sup>1</sup> most of us, however we may once have been captivated by ideas of unity, have abandoned the idea of unified science. The sciences are many. Each of them is a complex practice, sharing features in common with other complex practices, and typically elaborating the most general (and banal) methodological theses in ways suited to its particular questions and its particular subject matter. Philosophers can sometimes help in making methodological progress within some scientific domain. They do so not by offering some general advice about methods, but by attending to the specific problems that arise.

Up to this point, philosophy of science could be characterized as epistemology (maybe with a bit of metaphysics thrown in) about a particular kind of subject matter (perhaps knowledge at its most striking). There was no intersection with other fields of philosophy – ethics, political philosophy, aesthetics and the like. As Elliott Sober once reminded me, that should come as no surprise. For the founding fathers, before they were logical empiricists, were logical positivists – and the positivists denied that value

<sup>1</sup> From the 1970s to the present, philosophers of science associated with Stanford, have argued strongly for the disunity of the sciences. The movement originates with Patrick Suppes, and is continued in the writings of Nancy Cartwright, Ian Hacking, John Dupre, and Peter Galison. Although she was not at Stanford during the period when the main ideas were being developed, Helen Longino should also be counted among the “Stanfordians”.

judgments have cognitive content. Sober's insight faces one puzzle – the case of Otto Neurath. But I shall let that pass.

The move to a more inclusive philosophy of science began in the 1980s, particularly in the work of some feminist thinkers. I think, in particular, of Evelyn Fox Keller and Helen Longino (Keller 1985; Longino 1990). Despite the fact that they began with similar questions to those occupying previous philosophers of science – asking what science is and how it is properly done – values started to creep in to the answers they gave. For some of us, they have hung around. But for many philosophers of science – perhaps for most of you? – the old questions are the right ones. Philosophy of science is epistemology-and-maybe-metaphysics of science.<sup>2</sup>

A more inclusive philosophy of science finds room for science (in the singular) as well as the sciences (plural). For science (singular) is a social institution, one that plays a role in social and political life. There are, I suggest, serious value questions, questions about what its role ought to be.<sup>3</sup>

This, however, is to anticipate. My immediate task is to take stock of the story I have told you.

We are all heirs to the history I have – very crudely – rehearsed. And, of course, there are other developments I haven't mentioned, a few of which will occupy me later. Where does that leave us with respect to the audience question? What kinds of people make up the audience for philosophy of science?

If you are a refined person, a follower of Fine, you will think that the second possibility is the right one. Philosophy of science should speak to scientists, offering its luminous reconstructions and its methodological advances in specific domains. Perhaps incidentally it should address the general public, explaining (say) why some apparently contentious piece of science is well grounded (think of evolution or climate modeling). Or arguing that some supposed scientifically grounded claim that bears on human lives and human aspirations is actually unwarranted (think of debates about IQ, about the biology of race, about sociobiology, evolutionary psychology, and genetic modification of organisms).

What's the alternative? You might suppose that general philosophy of science isn't dead. Hempel and his successors posed the right questions, and, despite the failure to find fully general answers, philosophy of science should keep plugging away. Here I *am* skeptical. The diversity of the sciences – and of human inquiry in general – goes very deep. There are lots of different kinds of theories, many types of explanation-seeking questions, even many species of evidence. To be sure, some, rather thin, general theses can be advanced and defended (as with the picture of “scientific method” provided in textbooks) – and they have already been stated and defended quite enough.

<sup>2</sup> As an anonymous referee pointed out to me, many philosophers of science now accept the thesis that values play a role in the sciences. That acknowledgment changes the character of some epistemological debates. There are lively discussions of how values enter when theories are underdetermined by the evidence. Consideration of ethical or political philosophical questions around the sciences remains, however, relatively rare.

<sup>3</sup> Here, I want to distinguish the important questions, raised by Heather Douglas and Helen Longino, about the roles particular values play within scientific research, from the social-philosophical question of the proper role of the sciences within the broader society. What are the responsibilities of the institution of science within a democratic society? How should that institution interact with, and co-evolve with, other social institutions?

When philosophers try for greater precision, along with science-wide scope, accuracy has to give. Generality and accuracy we can manage – so long as we remain vague.

This is by no means to declare that the attempts to wrestle with Hempel's questions are worthless. The partial proposals generated over the past half century and more are not without value. Echoing the pragmatism behind Fine's suggestion, we can see them as useful tools. In particular contexts, thinking of explanation as subsumption under law or as the ascription of increased probability or as revealing possibilities of intervention – even as unification – can be helpful for clarifying a scientific dispute. Much of the fruitful work in the philosophy of the special sciences depends on applying and honing the tools supplied by the failed efforts to provide a general account.

Alternatively, you might think my third possible answer has something going for it. Public discussion would go better if there were a well-developed widely-accepted image of the sciences. Perhaps. But most of the time people seem to manage quite well with a vague idea, even with their own blend of Popper and Kuhn. When public distrust wrongly arises (evolution, climate science) or when unsound ideas are greeted with enthusiasm (IQ, evolutionary psychology), proceeding piecemeal and focusing on the special science does just fine.

We have an answer to my title question. It comes with advice. Go, find your special science. Write about it. Write for (and maybe with) the pertinent scientists. They are your audience.

I am not happy with this answer. The rest of my talk will attempt to tell you why.

You may well have noticed that I started with three possibilities. Only two of them have figured in my cartoon history and the morals drawn from it. When the other one drops out, only Fine's favorite remains.

My first possibility, that philosophy of science might contribute to philosophy, was excluded at the start of my potted history, because of the fact that, for the logical positivists, there was no independent contentful philosophy for philosophy of science to influence. Of course, along the way to today, that has changed. Indeed, if members of the Vienna and Berlin circles were resurrected among us, they would be appalled by the profusion of incomprehensible speculations. You can imagine them at philosophical meetings, desperately going from room to room, exclaiming "Metaphysics!" in ever more agitated tones. Indeed, a place for independent philosophy already became apparent after the transition to logical empiricism. The great grey volumes of *Minnesota Studies*, rich in classics of our field, contain from the beginning modest ventures in the philosophy of mind. Nothing, of course, to the multiplication of modalities, the embrace of essentialism, and the proliferation of a priori principles that have come to flood philosophical journals.

Despite its epistemological focus, the philosophy of science of the past decades has very little to say to what is often dubbed "mainstream epistemology". And a good thing too. For philosophy of science has concentrated on the old-fashioned task of assembling tools for sorting out the evidential merits of different claims. It has been, healthily and steadfastly, more interested in promoting inquiry than in tackling strained questions about whether, under particular artificial conditions, a subject can properly be said to know. Where it has wrestled with skepticism, the forms of skepticism addressed have

been live options, typically generated from historical or sociological studies of scientific practice.

The metaphysicians of today might, however, learn much from the achievements of philosophers who have studied the special sciences. For anyone trained in the philosophy of biology, many of the pronouncements self-styled metaphysicians make about natural kinds are embarrassing. Given the sophistication of half a century's explorations of causation, from Wesley Salmon, Patrick Suppes, and Brian Skyrms to Nancy Cartwright, Jim Woodward and Clark Glymour, listening to many presentations on "the metaphysics of causation" is akin to hearing fingers scraped over a chalkboard. And those steeped in the history of attempts to give a formal account of explanation will see the latest fashions in "the logic of ground" as making their painful way into the blind alleys of the past.

Yet, even if philosophy of science has value for general philosophy, you ought to wonder whether the skeptic has been addressed. Finding a scientific or a public audience would appear to settle doubts, although even here we should ask *why* satisfying the needs of these audiences suffices. Serving as a handmaiden to philosophy – to metaphysics, say – only seems to pass the academic buck. An audience of philosophers would be small consolation, if the philosophers served have themselves no significant audience.

Later, I'll suggest an answer to this worry, by proposing that philosophy of science should contribute to a much-needed successor discipline to traditional metaphysics. For the moment, let's consider another part of philosophy, one about which it's much harder to sustain charges of irrelevance. Whether or not you're a fan of contemporary meta-ethics or of the higher reaches of ethical theory, it would be hard to deny that, in the past half-century, philosophical discussions of particular ethical and political questions have made a positive impact on the conditions of human lives (and on the lives of sentient animals). Just read *Philosophy and Public Affairs*. As my historical sketch has emphasized, philosophy of science has mostly avoided any questions about values. That is now changing.<sup>4</sup>

Specifically, within the past quarter century a number of philosophers have taken up Darwin's suggestion (in the *Descent of Man*) that our ethical life could be understood as a product of our history. The philosophical work has taken two main directions. One, pursued in the pioneering work of Brian Skyrms, has elaborated formal models for the emergence of norms (Skyrms 1996, 2004). The other, now a dialogue between philosophers and scientists, has attempted to understand the genealogy of morality. The primatologist Frans De Waal, the anthropologist Christopher Boehm, and the psychologist Michael Tomasello have found philosophical fellow-travelers, including Patricia Churchland, Kim Sterelny, and me. Perhaps I express the fondness of a partisan when I hope for a sophisticated account of the evolution of ethical life, one that will improve ethical discussions across a range of significant questions.

The first option, then, isn't as unpromising as it might initially have appeared. Philosophy of science might illuminate ethics, and thereby contribute something that would ultimately improve the world in which we live. Let's turn now to the second possibility, taking up Fine's thesis that the philosophy of science is the philosophy of

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<sup>4</sup> Here, Helen Longino deserves great credit as an early pioneer, as does Heather Douglas for her critique of the value-free ideal.



the special sciences. Part of this seems importantly correct. Already in the 1980s, the impact of philosophical work on particular areas of science was apparent. Biology was an outstanding example. (As I sometimes used to tell students, philosophers have less effect on physics, since physicists typically don't think they have much to learn from anyone else – except maybe mathematicians; social scientists, however, are sometimes so desperate for any advice that a well-intentioned philosopher might start a trend that spent decades going nowhere; biology is the Goldilocks science, peopled by scientists who have some sense of what they are doing but who are still open to the suggestions of outsiders.) Here are some obvious examples of early effects on biology. Fine's colleague David Hull made large differences to systematics, Elliott Sober and Bill Wimsatt clarified controversies about units of selection, John Beatty addressed long-standing concerns about the concept of fitness. Those early contributions have been extended in a long list, and, as the work of people like Samir Okasha, Peter Godfrey-Smith, and Laura Franklin-Hall shows, the influence on biological practice shows no sign of abating. Moreover, thanks to some recent contributions – those of John Dupre, Ken Waters and Marco Nathan, for example – the initial emphasis on evolutionary biology has given way to a more inclusive treatment of biological fields.

In fact, my advice to students was wrong. As philosophers of physics began to liberate themselves from the framework of logical empiricism, making use of its tools where studies of scientific practice seemed to show their promise, exchanges of ideas with physicists became more consequential (I think here of the work of my colleague, David Albert, and our philosophical neighbor, Tim Maudlin). Similarly, as philosophers with an interest in the social sciences abandoned the thought that philosophy of science has some grand general perspective to offer, they have begun to work profitably with scientific colleagues on concrete problems I have already mentioned Alison Wylie's impact on archeology, and the contributions of Dan Hausman, Mary Morgan, and Nancy Cartwright to specific parts of economics. It is evident today that many younger scholars are following their example. (I don't claim that the names I've dropped are necessarily the best instances to make my case – they simply pick out the people who came first to mind. So I want to apologize explicitly to those of you whose work might seem equally – or even more – worthy of mention.)

A resounding cheer, then, for part of Fine's thesis. On the other hand, I think Fine underrated the possibilities of continuing the methodological tradition, begun by the philosopher-scientists (natural philosophers?) of the seventeenth century who revolutionized the physical sciences, continued in the eighteenth and nineteenth centuries (think of Whewell and Herschel, Mill, Darwin and Peirce), and pursued by the principals who figured in my potted history. As I have already asserted, this tradition strikes me as the healthy enterprise in epistemology (in sharp contrast to the sickly cousin whose spasms are recorded in "mainstream" philosophy journals). Fine perceptively recognized that the disunity of the sciences spelt trouble for the logical empiricist approach to continuing that tradition. What he overlooked, I suggest, was the possibility of different methodological questions, pertinent to particular aspects of scientific practice that were shared by a number of fields.

Two examples stand out. Consider first the revival of causation. Russell's famous obituary turned out to be premature. Many special sciences hunt for causes, and many applied sciences use them. Almost forty years ago, Clark Glymour and Judea Pearl independently began to explore the possibilities of using directed graphs to frame

causal hypotheses from statistical data (Spirtes et al. 2000; Pearl 2000). Today, the methodological proposals advanced by Pearl, and, perhaps, even more those articulated by Glymour and the brilliant team he has assembled at Carnegie-Mellon University, are being used across a swath of special sciences. Valuable exercises in formal methodology have themselves been revived with the resurrection of causality. Nevertheless, Fine is partly vindicated. Significant further work in methodology required a different focus (on the problem of finding causal hypotheses), one that was compatible with the disunities found in scientific practice. And, perhaps most importantly, it required a vast amount of dedicated effort on the part of highly talented people.

My second example addresses an even larger lacuna in the logical empiricist approach, one shared with the great tradition out of which it grew. With the conspicuous exception of Peirce, pre-twentieth-century methodologists sought canons of *individual* methodology. They considered how a single investigator should properly adjust belief in light of interactions with nature. Peirce not only recognized that inquiry is a communal affair. He also started to consider the ways in which the efforts of individual researchers might best be distributed if the community is to make epistemic progress – he talked of the “economy of research”. Twentieth century methodology ignored the hint, remaining relentlessly individualistic, until – interestingly – Kuhn’s celebrated monograph highlighted the scientific community and recognized the possibility of reasonable cognitive variation within it. Inspired by this idea, as well as by David Hull’s attention to social processes in scientific inquiry (and only belatedly discovering that Peirce had been there before me), I made some clunky attempts at formal models of community inquiry. Those efforts have been advanced by Michael Strevens, and more recently by younger scholars who have deployed far more sophisticated mathematical tools. This is surely the place to celebrate the work of Kevin Zollman and his group at Carnegie-Mellon, and to recognize the many places in Europe where equally sophisticated kinds of formal social methodology are being pursued: LSE, Cambridge, Tilburg, Copenhagen, and, most outstandingly, the brilliant group assembled at the Institute for Mathematical Philosophy in Munich. To cite just one example, unfamiliar to most philosophers, the senior scholar, Rainer Hegselmann, formerly of the University of Bayreuth, has developed extraordinarily sophisticated models of the flow of information in scientific networks; a quick visit to Google Scholar will reveal just how unusually influential Hegselmann’s principal article (Hegselmann and Krause 2002) has been.

As in the case of methods for identifying causes, a change of focus opens the way for methodological investigations that cut across scientific disciplines. I am surely partial, but, despite my great admiration for the achievements of Pearl, Glymour, and their associates, I view social methodology, formal and informal, as the great area for epistemology today. Detailed studies of scientific practice, carried out by philosophers, historians and sociologists, have revealed many aspects of communal inquiry. That invites philosophical reflection. Are the conventions and norms governing interactions within scientific communities conducive to the progress of inquiry? Do the conventions on publication and rewards for discovery interfere with the sharing of scientific information? How much disagreement should scientific communities tolerate? Questions of this sort are obviously difficult, but answers would be profoundly relevant

to the practice of the sciences. To repeat my modification of Marx: The philosophers have ignored the social structure of science. The point, however, is to change it.

Thus, I agree with Fine that scientists are an important audience for the philosopher of science. But I want to expand the range of topics beyond those emphasized in his prophetic address.

And I also want to make a case for the third possible audience. Indeed, at this particular moment in human history, the need to present scientific issues clearly to people who are not scientists seems to me the most crucial task of all (and perhaps it is especially important to reach journalists and policymakers). The rest of this talk will be devoted to explaining and defending that view, idiosyncratic as it may initially strike you.

We live at a time when many decisions profoundly affecting human lives turn on the details of the sciences, sometimes with respect to matters on which a community of scientists agree, sometimes on topics about which there is lively debate. When citizens of affluent democracies have misguided views about what policies (and which candidates) will advance their goals, their choices at the ballot box can run directly contrary to their interests. Ironically, the act through which democracy is often taken to express its commitment to individual freedom then undermines that freedom. Where misinformation is rife, democracy lapses.

Hence, advancing the knowledge of citizens is an important task. If philosophers writing for the general public, were able to make their readers more likely to understand and accept the consensus views of an expert community – think of climate change – that would be a valuable achievement. By the same token, if they were able to unmask inadequately supported claims about genetic determinism – think of controversies about the inevitable limitations of women and racial minorities – they would have contributed to the general good. To write about socially consequential parts of science for a non-academic audience is to perform a public service. Succeeding with that audience delivers the most obvious answer to the skeptical question.

But why, you might ask, would this be a project for philosophers of science? Wouldn't scientific practitioners be much better at doing it? The short answer is that recent history shows how a combination of efforts from writers with different abilities has often proved profitable. In the original controversies about the fixity of IQ, Dick Lewontin played an important role – but so did Ned Block and Gerry Dworkin. In debates about the credentials of evolution, Niles Eldredge, Stephen Jay Gould, Ken Miller and Doug Futuyma had an important impact – as did Michael Ruse, Barbara Forrest and Robert Pennock. Lewontin, Leon Kamin and Stephen Rose offered a powerful case for the speculative nature of human sociobiology – but I like to believe that John Dupre and I have added something. With respect to climate change, despite the best efforts of James Hansen, Michael Mann, Naomi Oreskes and others, even the most basic thesis – the claim that anthropogenic climate change is real – remains disputed. Here, input from philosophers has been largely absent – a matter Evelyn Keller and I have tried to put right.

I don't think there's any great mystery about why philosophers have been able to make contributions in the areas I've cited. To be sure, recent decades have witnessed the emergence of an important scientific genre. Eminent scientists have written superb books and articles for a wide audience. Happily, they have been applauded for doing so. It is no longer true, as it was forty years ago, that scientific colleagues would utter

‘popularizer’ with a sneer. Yet, despite the excellence of many presentations of major scientific ideas by experts in the pertinent fields, philosophers of science have brought a special set of skills to their own – typically complementary – expositions. Trained to think hard about evidential relations, they have sometimes found ways of showing more clearly than their expert scientific colleagues how justification works or where exactly it is lacking. My judgment translates to a new context an old idea. Those who once believed in the “logic of the sciences” were held by the idea of a very particular form in which parts of science should be reconstructed. Eighty years on, philosophers of science take a far more diverse set of approaches to laying bare the evidential relations, although they continue to deploy tools forged by predecessors who sought general answers to Hempel’s three big questions.

Since I have spent significant periods of my life on projects of the kind just described, it is hardly surprising that I should see this kind of writing as a major part of the answer to my Shakespearean friend’s question. But I want to close by suggesting that, important though it may be, addressing the large public scientific controversies of the age is by no means the sole way in which philosophy of science should address the general public. In recent years I’ve come to view a different, more general, philosophical task as more important still.

As I remarked earlier, feminist scholars who attended to the practices of science – and I see Evelyn Keller and Helen Longino as pioneers – taught us to see science, the institution, in relation to human lives and to our socio-political condition. I was slow to take the lesson. But my involvement with the Human Genome Project brought home to me what Keller and Longino had so clearly seen – the importance of raising ethical and political questions not only about the practices of the different sciences but about science as an institution.

In retrospect the sheer weirdness of the fact that, for decades, philosophy of science has been “all epistemology and metaphysics, all the time” seems blindingly obvious. Even when scholars, influenced by Kuhn, started to think about scientific communities as heterogeneous, and not as large individuals with a single mind, social studies of science were dominated by issues about truth and knowledge. I wonder how much of the disagreement between philosophers of science and sociologists of science can be explained by seeing the socially oriented scholars as forced to pursue questions about values by other means. Were the flirtations with relativism simply a hangover from the positivist assumption that reason breaks down when value judgments enter the discussion?

Thanks to Heather Douglas, Torsten Wilholt, and others, the limitations of the value-free ideal are now recognized (Douglas 2009; Wilholt 2009). Yet, in many parts of our profession, the old questions – and often the old, inadequate, answers – remain popular. To be sure, new directions are being pursued in centers<sup>5</sup> that explore the connections between scientific research, public policy, and the quality of human lives. Important work is being done at Western University in Canada, and at many places in Europe: Bielefeld and Hannover, Munich, Rotterdam, Durham, and – definitely not least – here at Exeter. Under John Dupre’s leadership, Egenis has flowered into a model for socially relevant thinking about science.

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<sup>5</sup> And also through collaborations that link scholars at different institutions. A major example is the SRPoise network.

To my mind, we need much more ethically and politically informed philosophy of science. Many general questions about the proper role of science in democratic societies and with respect to human lives have yet to be adequately addressed. There are specific ethical issues about many kinds of research, especially in the biological and the human sciences. As Nancy Cartwright and her collaborators have demonstrated, methodological and value-theoretic questions intertwine in considering the evidential bases of policies with large human impact (Munro et al. 2017). Perhaps most urgent of all, is the tangle of problems – epistemological, ethical, economic, technological, social and political – that are generated by anthropogenic climate change. I applaud the fact that some philosophers of science (Wendy Parker, Nancy Tuana, Matthias Frisch and Eric Winsberg prominent among them) have begun to study the simulations used by climate modelers. But a far wider philosophical focus is needed.

Perhaps you worry that these questions are too hard, and that devoting effort to them pursues nothing worthwhile. If so, I recommend looking at the series of articles written about the concept of race by scientifically-informed philosophers. Since the earliest such articles, roughly twenty years ago, the progress has been extraordinary.

But I also want to outline a philosophical task beyond these particular endeavors, an enterprise I take to be almost unappreciated. Start with a question akin to the one with which I began. Why do we continue to read the great philosophers of the past? Perhaps a doctrinaire “analytic” philosopher would reply: “Because of their arguments. The intricacy of their reasoning is really neat, or cool, ... or whatever”. I find that response unsatisfying. Not just because of the deeply disappointing moments – the Cartesian circle, Mill’s attempt to prove the principle of utilitarianism, Kant’s second analogy (maybe Strawson was right – it was “a non-sequitur of numbing grossness”?). The real achievements of the western philosophical tradition lie in the magnificent syntheses provided by thinkers who reflected widely on the achievements of the past and the conditions of life as they encountered it. Philosophy at its greatest is *synthetic*. It doesn’t work beside the various areas of inquiry and culture and practice. Instead, it works between and among them. As Dewey puts the point, it tries to offer the *meanings* of what human beings have come to know. In that consists the successor discipline we need to replace the metaphysics of the past.

In a highly complex world, the project of offering an illuminating synthesis of every significant aspect may be too ambitious for any individual thinker. Philosophers are often disinclined to collaborate, but perhaps it is worth considering a more cooperative future. An obvious alternative would be to focus on a particular facet of the world as we find it. Philosophy of science would contribute a general picture of the sciences and of science as an important institution in human societies.

For all their faults, logical positivism and logical empiricism undertook part of that task. So too did Popper and Kuhn. Their greater influence may stem from their willingness to connect, from time to time, with social and political issues. Today, perhaps because we recognize the heterogeneity of scientific practices, the philosophy of science has not yet offered any convincing substitute. Ironically, though, the best versions we have emerge in the writings of prominent “disunity theorists” – Longino, Cartwright, Dupre, Ian Hacking and Hasok Chang.

So I offer a threefold answer to my original question. The audience for philosophy of science *should* include philosophers, scientists, and thoughtful members of the public (the supposed, possibly mythical, “educated general reader”). If, as a matter of fact, any

of these groups – *especially* the last – is playing truant, it's part of our responsibility to try to bring them back. If the organization of our profession creates perverse incentives, so that philosophers of science end up talking only to one another (or to themselves), then (echoing Marx again), the point is to change it. We shall probably never achieve as wide a readership as those who write most eloquently about Shakespeare. But we ought to give it a try.<sup>6</sup>

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In my revisions, I have tried to preserve the relatively informal voice of the original, and have inserted references only in places where it might be hard to track down my allusions.

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<sup>6</sup> The first question posed during the discussion period in Exeter was so apposite that it deserves a brief answer here. The questioner, a young philosopher, wondered whether the recommendations I had urged could be followed by people near the beginning of their academic careers. I responded by recognizing the wisdom of following the conventions and standards of the profession until one is established – and thus postponing more socially relevant inquiries to a time when one's position is secure. But, as I added, senior philosophers of science have the responsibility of scrutinizing the prevailing conventions, revising them where they interfere with important work, and thus attempting to relieve the pressure on younger scholars torn between prudence and their sense of where they should focus their efforts. This is only a sketch of an adequate reply to a question deserving more discussion than I have so far offered.

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