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1. How were you initially drawn to philosophical issues regarding science?

It was a mixture of science and politics that drew me into philosophy of science during my years as a physics undergraduate. To me, and I guess to other leftists and ex-marxists of my generation, the early 1980s were a period of crisis of belief. A relatively neat conceptual scheme by which the world was viewed—quite solid in its basic principles despite the many substantive differences in detail from thinker to thinker and from country to country—was collapsing. The hope that the world can change for the better via mass political action was coming to grief—at least for me. Contrary to Karl Marx’s famous eleventh thesis on Feuerbach, the point was still to interpret the world—before we try to change it. The world cannot change unless *we* change; unless we take as basic and unnegotiable some principles of rationality and the search for truth and justification. Science was, to me, the bastion of rationality and progress; the *terra firma* upon which one could base all hopes for a better world. I believed back then, and still believe now, that science is the best way we have invented to push back the frontiers of ignorance and error, to achieve a deep understanding of the world and of our place in it, and to make the world a better place to live. But science and its claim to truth and knowledge needed justification and defence. This was a demand we inherited from the philosophers of the enlightenment—to subject to criticism even the most dearest and seemingly unassailable of our beliefs. This demand became even more topical in the age of modernity—where science itself was being dramatically transformed, delivering immense benefits to humanity, but also inflicting misfortunes. To me, looking into the scientific realism debate

was no longer optional. I came into this debate with no neutrality. I wanted to defend scientific realism, along with the objectivity and rationality of science and its method. This was both an intellectual and a political goal. Back in the 1990s, there was a pervasive thought, especially among left-wing American intellectuals, that undermining the alleged epistemic authority of science, challenging its claims to objectivity and knowledge, was an act of liberation from the strangling authority of Reason—perhaps from capitalism itself. I was never persuaded by this rhetoric. It conflated intellectual authority with authoritarianism and, at least to all of us who learned our basic politics in the European south, intellectual authority (and objectivity and criticism and the search for truth) were the arch enemies of any kind of authoritarianism.

2. What, in your view, are the most interesting, important, or pressing problems in contemporary philosophy of science?

Philosophy of science has changed a lot during the twentieth century. If I were to offer a thumbnail summary of its course during the last century I would put it as follows. It started in the noughties with huge crises in the sciences and mathematics and its agenda was shaped by philosophically minded scientists who were battling for the prospects of certain theoretical ways to view the world and for competing views about the limits and scope of science and its theories. It took the form of the logic of science in Vienna in the early 1930s, where formal methods were employed to analyse and explicate the basic concepts of science and its method. ‘Metaphysics’ became a dirty word, but objectivity (mostly in the guise of structural invariance, or the common-factor point of view) was still what was sought after. It took the naturalist turn in the USA of the 1950s, leaving behind ‘first philosophy’ and its own allegedly special method of conceptual analysis and a priori insight in favour of a view that philosophy is continuous with the sciences. It looked for history-of-science-compliant macro-models of science and its growth in the 1960s, but it soon became apparent that little useful to the several individual sciences could be said at this level of generality and abstraction. The 1970s saw an explosion of interest in the philosophy of the individual sciences (in particular in the so-called special sciences—biology, psychology, economics etc.). The metaphysics of science staged an impressive comeback in the 1980s, with its full panoply of issues: causation, laws of nature, necessity, properties, natural kinds and the like. But what

has been most impressive is that in the last quarter of the twentieth century there has been an enormous diversity in the agenda of philosophy of science—formal methods (led by an increasing interest in Bayesianism); social studies of science; cognitive models of science; computational philosophy of science; feminism and gender studies; ethical issues in science and others. The result of all this is that it is hard nowadays to share interests with more than a few other people that work in cognate areas and even harder to stay in touch with philosophy of science as a whole.

Overall, the movement in the philosophy of science in the twentieth century has been from a more globalist approach of science and its problems, exemplified in a concern with broad philosophical issues about science (such as the aim and structure of theories, the nature and limits of explanation, the relation between the ‘rational’ and the ‘factual’, as Ernst Cassirer has aptly put it) to a more localist conception of philosophical problems, where philosophy of science is seen primarily as a toolbox for fixing problems that arise in the sciences. I cannot help feeling there is a certain loss in this movement of thought—we nowadays pay so much attention to the trees that there is too little to spare for the wood. There may well be different conceptions of what the wood is, but in my own opinion, the wood is still the globalist agenda that animated philosophy of science in the beginning of the twentieth century. This is partly because I have a Sellarsian account of what philosophy should aim at. Recall what he said: “The aim of philosophy, abstractly formulated, is to understand how things in the broadest possible sense of the term hang together in the broadest possible sense of the term” (SPR, pp. 1-40). For me, philosophy of science is an attempt to start with a determinate conception of reality as described by our best science and to try to understand *it*, what the world is like according to it, how it came about, and what it implies for us and for the ways we know and transform the world.

I would single out the following issues as most important or interesting.

- The role of mathematics in scientific theories; in particular, the fact that it seems (*prima facie* at least) that the abstract and the concrete are interwoven in our scientific conception of the world. Mathematics seems epistemically indispensable for understanding the world, but it is metaphysically suspect (or so many think) because mathematical entities

are causally inert. Reworking the relation between the abstract and the concrete in physical theory will perhaps open up new ways to conceptualise the world.

- Do scientific theories imply any substantive commitments about the deep metaphysical structure of the world? This is an area where the most exciting work that has been going on in the philosophy of the individual sciences (physics, chemistry, biology, psychology, economics) can come in creative contact with the most exciting research that has been taking place in analytical metaphysics of science. Analytical metaphysics of science is empty without the philosophies of the individual sciences; the philosophies of the individual sciences are blind without analytical metaphysics of science.
- The relation between the epistemic and the ethical. Science is subject to epistemic norms but its increasing relevance to the lives, well-being and prosperity of people (and of other animals, and of the planet as a whole) requires that it is subjected to ethical norms too. There is need for a new deal between the epistemic and the ethical in science and hence a sustained development of an ethics of science.
- Competing conceptions of objectivity and rationality. Here too, there is space for a creative interaction between the formal work that has been done on logical and probabilistic models of rationality and decision-making and the strongly emerging view that values play an ineliminable (yet not algorithmically determined) role in scientific judgement.
- Philosophy of science should keep looking into its past aiming to better understand the thought and theories of the thinkers and schools of the twentieth century and before.

3. How has your work offered original contribution to discussion on science? What does your work reveal that others fail to appreciate?

It is hard to talk about your own work and its originality. This is best judged by the others (and, ultimately, by posterity). However, false modesty is no less a vice than arrogance! My own work has really fallen into two stages (and a break in between). I thought hard about scientific realism for about a decade and produced my *Scientific Realism: How Science Tracks Truth* (1999). Then,

I had a break from realism, working mostly on causation and explanation. In the last few years, I have come back to scientific realism, aiming to rework and rethink the way its defence was articulated and advanced in my 1999 book. Part of the motivation to rework my commitment to realism has come from my venture into metaphysics. I have the highest respect for metaphysics, but I want to have as little of it as possible (it's an illusion, I think, to believe that you can leave metaphysics behind altogether).

I think two are the most distinctive marks of my work on scientific realism. The *first* has to do with the abductive defence of realism. Ever since Hilary Putnam put forward the slogan that realism 'is the only philosophy of science that does not make the success of science a miracle,' the defence of scientific realism has been based on what has come to be known as the 'no-miracles' argument. This argument has had quite a long history and a variety of formulations—some of which can be traced as early as in the beginning of the 20th century. In my view, the structure and role of the no-miracles argument in the realism debate is quite complex and my own research has aimed to unravel this.

The way I read it, the no-miracles argument aims to defend the reliability of scientific methodology in producing approximately true theories and hypotheses. Following more concrete types of explanatory reasoning that occur all the time in science, it suggests that it is reasonable to accept certain theories as approximately true, at least in the respects relevant to their theory-led predictions. These successful instances of explanatory reasoning in science provide the *basis* for a grand abductive argument. The no-miracles argument, however, is not just a generalisation over the scientists' abductive inferences. Although itself an instance of the method that scientists employ, it aims at a much broader target: to defend the thesis that Inference to the Best Explanation is reliable.

One central objection to the no-miracles argument is that it is *viciously* circular. But the abductive defence of realism proceeds within a broad naturalistic framework, within which the charge of circularity loses most of its bite because it is not justification of inferential methods and practices (at least in the neo-Cartesian internalist sense) that is sought but their explanation and defence (in the epistemological externalist sense). What I added to this defence, based on a well-known (though controversial) distinction between premise-circularity and rule-circularity (a premise-circular argument employs its conclusion as one of its premises; a

rule-circular argument conforms to the *rule* which is vindicated in its conclusion), is that (a) the abductive defence of realism is rule-circular, (b) rule-circularity is *not* vicious, and (c) rule-circularity is involved in the defence of all basic rules of inference.

The *second* mark of my work on scientific realism has to do with my arguments against the pessimistic induction. The thought here is that the history of science is replete with theories that were once considered to be empirically successful and fruitful, but which turned out to be false and were abandoned. If the history of science is the wasteland of aborted ‘best theoretical explanations’ of the evidence, it might well be that current best explanatory theories will take the route to this wasteland in due course.

In order to reconcile the historical record with realism, I have claimed that realists should be more selective in what they are realists about. This led me into some work on particular past theories (like the caloric theory of heat and the nineteenth-century optical ether theories) aiming to show that those parts of them that essentially contributed to their empirical successes were retained in subsequent theories. This is what I have dubbed the *divide et impera* move. A claim that has emerged with considerable force is that theory-change is not as radical and discontinuous as the opponents of scientific realism have suggested. Realists ground their epistemic optimism on the fact that newer theories incorporate many theoretical constituents of their superseded predecessors, especially those constituents that have led to empirical successes. The substantive continuity in theory-change suggests that a rather stable network of theoretical principles and explanatory hypotheses has emerged, which has survived revolutionary changes, and has become part and parcel of our evolving scientific image of the world.

Critics of my views have raised a number of important objections. I have learned a lot from them—though so far I have not responded to most of them in an orderly and systematic way. There is, I think, a substantial issue of disagreement between me and many of my critics, which I take it to have broader implications for the way we view science and its relation to evidence and truth. I am an anti-holist in matters of confirmation and the *divide et impera* move requires anti-holism. Most of my critics rely on holism to challenge my arguments. Anti-holism can be easily misunderstood. Holistic theories of confirmation back in the 1960s did play a crucial role in the defence of scientific realism, since it was on their basis that it was shown that theoretical assertions

(assertions that make claims about typically unobservable entities) are no less confirmable than observational ones. Evidence clearly goes ‘all the way up’ to the remotest theoretical reaches of the theory—it does not stay to what the theory says about the observable entities. But in the heat of the battle, it was not sufficiently stressed that though theoretical assertions are confirmable, they are not equally well-confirmed by the evidence; nor are all of them equally contributing to the successes of the theory; nor even to its explanatory potential.

What I now think is that when we (philosophers) think about scientific theories and what they assume about the world, we need to balance two kinds of evidence. The first (let’s call it *first-order evidence*) is whatever detailed and specific evidence there is in favour (or against) a specific scientific theory—evidence that has to do with the degree of confirmation of the theory at hand. The second kind of evidence (let’s call it *second-order evidence*) comes from the track-record of scientific theories and/or meta-theoretical (philosophical) considerations that have to do with the reliability of scientific methodology. This second-order evidence feeds claims such as those that motivate the Pessimistic Induction. In assessing scientific theories and science as a whole, these two kinds of evidence need to be balanced. How exactly this balance should be stricken is an interesting philosophical issue. It seems that it will be a contextual matter. Philosophers of science can help specify what kinds of factors and considerations can determine the context. What is also important is that there should not be double standards in confirmation, based on a supposedly principled distinction between OK-entities and not-OK ones. (This distinction is drawn along several lines, most typically between empirical and theoretical entities or between observable and unobservable entities or between entities to which there is independent epistemic access and entities to which there is not.) If no absolute privilege is conferred on either the first-order or the second-order evidence that is brought to bear on theories and if there are no double standards of confirmation of supposedly distinct parts of theories, a variety of epistemic stances towards scientific theories might be enunciated, depending on the context. All this might pave the way for a rapprochement between contextualist versions of instrumentalism and scientific realism.

4. What is the relation between philosophy of science and scientific practice, science policy, or efforts for social justice? Can there be a more productive relation? Is this

desirable?

Philosophy of science is in relative isolation from scientific practice. This is both good and bad. It is good because philosophy of science is primarily *philosophy* of science. Hence, it is not a commentary on what scientists do; nor is it exclusively dependent on what trends and views there are in current science. There could not be philosophy of science without science—but the subject matter of philosophy of science is *philosophical* problems that arise within science. The current scientific worldview ought to act as a constraint on philosophy of science and on philosophical thinking in general. It will be hard to defend a view on strictly philosophical grounds, especially if it leads to consequences that are in conflict with what our best theories of the world tell us about the world and our own place and role in it. This, however, does not imply that a philosophical conception of science, let alone a philosophical stance about what the world is like, is *dictated* by current science. Naturalism will be trivialised and will trivialise philosophy if it is pressed too far.

The bad thing is that philosophy of science, at least occasionally, is practised in such a way that there seems to be no caring at all about scientific practice; nor any concern about getting the scientific facts right. Abstract philosophical argument is good, but should make some contact with the real world. A pertinent example is some of the discussion about the argument from the underdetermination of theories by evidence. This concerns a deep philosophical problem and has been discussed thoroughly ever since Descartes formulated it in a relatively precise way with his evil-demon hypothesis. The spectre of scepticism has to be reckoned with and this calls for a thorough re-evaluation of what we take to be knowledge and justification (and of what resources, including empirical ones, we use to address these issues). There has been some exciting work in epistemology here in the last two decades. But I have the feeling that some bad philosophy of science has been fostered in relation to this issue: an exercise in philosophical imagination attempting to devise empirically equivalent alternatives to scientific theories (abstractly understood) which are totally uninteresting from a scientific point of view and are motivated by purely philosophical doubts and considerations. Taking account of actual science can help philosophers come down, every now and then at least, from the platonic heaven.

Science policy is an area in which philosophers of science ought to play a more central role. In science policy there is typically

conflict between competing norms and priorities. A number of decisions are based on a mixture of facts and value-judgements. Philosophy of science can help create a balance (which, again, can be context-dependent) between facts, values and interests. Philosophical analysis (and inter-disciplinary input) can lead to the creation of practical frameworks: (context-dependent) norms of action in different cognitive areas. What is particularly important is that philosophy of science can offer insights and formal tools concerning decision-making under uncertainty and risk-management. It can explore ways in which epistemic responsibility and social responsibility reinforce each other. It can act as the mediator between what is sometimes seen (by the educated public) as the dogmatism of science and what is sometimes seen (by scientists) as social prejudice. Some other issues that philosophers of science can be particularly helpful (and in which their work might have a broader impact) are: managing scientific controversies and expert disagreement; examining the sources of uncertainty; finding new uses for evidence and for values in science policy making; managing social controversies (e.g., related to the public perception of scientific controversies, risk-perception etc.). The common factor in all this is that good science policy requires a better understanding of what science is and how it works.

I wish philosophy of science could have a stronger role to play in issues of social justice—or to promote social justice, for that matter. I think feminist philosophy of science and in particular my own favourite feminist standpoint epistemology has done much good in promoting social justice while securing claims to objectivity and truth. But I still believe that the cause of social justice is best served when we leave our papers and research projects every now and then and engage in local social action. Nowadays, the keyword seems to be ‘excellence.’ And excellence we do need—but we should not forget that we should also create opportunities for excellence where they are most needed.

5. Where do you see the field of philosophy of science to be headed? What are the prospects for progress regarding the issues you take to be most important?

Progress in philosophy is hard to measure. Perhaps, the emergence of consensus is a sign of progress—at least we come to agree that some problems are not worth exploring any more or some positions are no longer viable (though by no means dead). There seems to be not much going today for reductive understandings of the

meaning of theoretical terms, for strong instrumentalist accounts of scientific theories, for purely formal models of explanation, and for global type-reductive accounts of inter-theoretic relations. Perhaps, the whole philosophical issue of conceptual change and the alleged incommensurability is pretty much exhausted.

Devising grand theories of science, of the form that dominated the 1960 and 1970s seems to have run out of steam. There are some rich ideas connected with these grand models (for instance, the Kuhnian idea of a normal science or the Lakatosian idea of a progressive research programme). But the deep and interesting work that has been going on in the foundations of the individual sciences has tended to highlight the disunity in science. There is room and need, I think, for a new synthesis. The diversity of the sciences can be looked at again, this time with an eye to common structures (especially in methods and in the patterns of explanation—e.g., in terms of mechanisms). This may well lead to cross-fertilisation—both in form and in content.

It's a good sign that the metaphysics of science has become a hot topic. It seems that this will be a scientifically informed metaphysics. But I am sceptical about the emergent neo-Aristotelianism. The barren landscape painted by the empiricist philosophers (based as it was on the thought that the regularities there are in the world do not need metaphysical enforcers) is being redrawn, this time with the full Aristotelian panoply—active powers, essences, necessary connections and the like. To my mind, these are unexplained explainers, and though everyone has to accept *some* unexplained explainers, in this particular case, they are more poorly understood than what they are supposed to explain. I favour a Wittgensteinian attitude here: “a nothing could serve just as well as a something about which nothing could be said” (*Philosophical Investigations*, §304).

After a period of what has been called ‘the new fuzziness,’ formal methods in the philosophy of science have come back. This time the horizons have considerably expanded. New formal tools are being used, from non-classical logics to probability theory and game theory. There has been a lot of exciting work on causal modelling and causal inference using Bayes networks; on ampliative and defeasible reasoning (with a lot of input from Artificial Intelligence); and on Bayesian confirmation. Clearly, using mathematical methods is one way to reduce philosophical complexity. But there are limits to this and I do not share the view that formal epistemology holds the key to answering basic philosophical

problems. Perhaps, what makes philosophical problems distinctive (and maybe unanswerable) is precisely their complexity; their formalisation (based as it is on idealisation, simplification and abstraction) solves surrogates of these problems. For instance, I think there is an ineliminable role for explanation and explanatory considerations in ampliative reasoning—and this seems to resist formalisation precisely because there is more to explanation than satisfying an abstract or formal pattern. Or look at the prospect of understanding the role of framework principles in science using formal methods—it doesn't seem too good.

As I have noted already, issues in the ethics of science acquire urgency. One issue here is devising normative frameworks for the ethical conduct of research in science. It is plausible that there should be a core of ethical principles that ought to constrain scientific research. But then again, science is subject to cognitive aims too and, I take it, it is a very interesting question how norms of conduct in science can have a mixed (or double-sided) justification: cognitive and moral.

We need to pay more attention to the relations between central issues in the philosophy of science and topics and debates in other areas of philosophy (most notably, metaphysics, epistemology, and philosophy of language). Take the debates in epistemology about knowledge and evidence. These, to a large extent, are developed in isolation from what goes on in the philosophy of science. Conversely, debates about method and rationality in the philosophy of science have taken little notice of relevant developments in epistemology. This is hugely unfortunate. If philosophy of science cuts off its constitutive lore from the rest of philosophy, this will have disastrous consequences in the long run.

Finally, the complex landscape of the twentieth century philosophy should be re-drawn. The massrooming of interest in the history of philosophy of science, the systematic attempts to re-evaluate and re-appraise the major philosophical schools and the major philosophers of science of the twentieth century are extremely welcome. The philosophical battlegrounds of the twentieth century saw many attacks on strawmen and a number of pyrrhic victories.

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