1. To begin, I would like to start with a few questions regarding philosophy, broadly construed. I think the best way to start these questions off is to ask about a philosophical tool very close to the hearts of undergraduates everywhere, namely that of the Stanford Encyclopedia of Philosophy. I understand you have a Stanford Encyclopedia of Philosophy entry on scientific realism forthcoming. How did you become involved with the Stanford Encyclopedia? What types of considerations go into writing such an entry?

Anjan Chakravartty: Yes, all the carefully guarded secrets of how one can and cannot understand scientific theories and models as yielding knowledge of the world will be revealed in the *SEP* article, which should be out before this issue of *Sophia*. The editors of the *SEP* are very crafty; only they know what criteria they use to identify their victims, and they know just what to say to trick people into contributing. But seriously, I took the invitation as a challenge and an honour. It was a challenge because so much has been thought and written about scientific realism (in essence, the view that our best scientific theories yield truths, or something close by, regarding both observable and unobservable aspects of a mind-independent world, or that they correctly describe the ontology of such a world). It is, perhaps, the most central issue in the philosophy of science, and with that comes a voluminous literature. My initial search turned up more than a thousand articles and books of immediate relevance, and the idea of trying to do justice to that many ideas in the space of an article was daunting. It was gratifying, though, to shoulder the responsibility of attempting to do that well. Knowing that this would be the first place many people turn to understand what scientific realism is was a great motivator. The aim is to be accessible, so that those without a background in the area can learn, but also not to shirk the complexity of the issues involved, so that readers learn what's really at stake, as opposed to gliding over an oversimplified gloss. That's a lot to ask, but I enjoyed the exercise very much.

2. After finishing your PhD at Cambridge, you returned to the University of Toronto as an Assistant Professor. You seem to have spent a good portion of your academic career between Canada and Britain. What do you think are some notable differences between philosophy of science in Britain and in Canada? Are there different problems addressed, or different expectations in terms of publications?

AC: Along a number of dimensions relevant to philosophy, as in all things, Canada seems to occupy a position somewhere between Britain and the U.S. The differences I noticed were more pronounced, I think, even just a few years ago, and I see greater convergence today. But at the time I was a graduate student in Cambridge in the later 1990s and shortly thereafter, two things in particular struck me. One was that graduate students in Britain seemed to be significantly less "professionalized" than their counterparts in the U.S., which had both positive and negative implications. On the positive side, there seemed to be much less calculated structuring of one's work around the question of how to get a job, and as a result, a purer focus simply on the glorious pursuit of philosophical knowledge. On the negative side, there was less of the sort of training that's standardly part of U.S. graduate programs, designed to equip students with the skills they'll need to enter into and then thrive in the profession, such as teaching, publishing, interviewing, and yes, landing a job. The second thing that struck me was a very specific exemplification of this perceived difference: there was significantly less attention paid in Britain to philosophical "trends" – if you wanted to work on a topic that was genuinely philosophical, you would find support. I was shocked when I came to my first graduate student conference in the U.S. to hear all my trans-Atlantic peers saying things like '*x* is hot right now, so I'm shifting my focus to that', or '*y* is going to be the next big thing, so that's my project'.

Of course, it's silly to take these sorts of generalizations about departments and universities across entire countries too seriously, but as I say, my experience of Canadian institutions was somewhere in between. And as I say, I've seen a great deal of convergence since, so that the differences now are less striking. No doubt some laudable, Aristotelian-Canadian balance between the extremes is the right way to go! Students do need to be equipped with professional skills, because graduate schools must function not only as places of glorious higher learning in the narrow academic sense, but also as links to a life of professional scholarship (for those who wish to go that route) more broadly. The worry about trendiness provides an excellent illustration of the need for balance, I think. Unless you're entirely sure that you have a novel take on what is universally regarded to be a philosophical dead end, that will result in a stupendous rebirth of the subject matter, you might want to think carefully about pursuing it. But there's really no sense in obsessing about what's hot or likely to be by the time you graduate. Philosophical winds blow one way and then another, so my advice to students is always to work on what you're most passionate about. That's where you're likely to do your best work, and if you do your best work, then you're more likely to succeed professionally.

3. In 2009, you acquired the position of Director of the Institute for the History and Philosophy of Science and Technology (IHPST) at the University of Toronto. How do you see the relationship between History of Science and Philosophy of Science, in general and in your own work? The IHPST seems to be a fairly unique department within the greater structure of the University of Toronto. In your opinion, what is special about its mandate and place within the university? Is there anything uniquely valuable about the IHPST?

AC: 'Acquiring' the position makes it sound suspiciously like I won it in a game of chance, or picked it up at an estate sale. Come to think of it, that's often how it happens (but luckily, not in my case!). Becoming the chair of an academic department certainly isn't something one agrees to do lightly, but I was attracted by the fantastic potential of the unit, and the thought of helping to facilitate its contributions to the wonderful area of scholarship that is the history and philosophy of science (HPS). As in any interdisciplinary field, there's no shortage of opinions regarding what is the right relationship between its components. The goal of fusing history and philosophy so as to understand the nature of the sciences was really the founding vision of most of the HPS departments that exist today (many founded in the 1960s, inspired in no small measure by influential work by Thomas Kuhn, Paul Feyerabend, and others), but that vision has fallen on hard times since. Some people still do think that it's the "right" way to study the sciences from a humanistic perspective, but I suspect that most scholars in the area are less rigid about this prescription these days (though their own visions are sometimes equally rigid in other ways). There is very historical work that isn't noticeably inflected with a deep understanding of the philosophical ideas that were a formative part of the relevant historical context, or that could be used to think about it, and there is very philosophical work that betrays little understanding of the history or indeed the practice of the science that it aims to consider. There are obvious dangers here in too strong a separation.

That said, I myself hold very strongly to a particular brand of pluralism when it comes to HPS. Work that is simultaneously both deeply historical and deeply philosophical is simply marvelous to behold, but I don't think it's the *sine qua non* of good work in HPS. (Lucky thing, too, because it's very hard to do, and it would be asking too much to expect everyone in the field to be capable of it.) I think HPS is, in fact, and rightly, an extremely broad tent. There is integrated work of the sort originally envisioned, but there is also largely historical or philosophical work. There is work that has emerged from HPS that has embraced yet more diverse forms of scholarship (anthropological, ethnographic, sociological) and sometimes focuses on more contemporary science (often under the heading of 'Science and Technology Studies' or 'Science, Technology, and Society' – STS). There is room in HPS for all of these things. What unites us all is a fascination with and a commitment to

furthering the humanistic understanding of the practices we call science, in all of their dimensions. That's my own vision of HPS.

As for my own work, well, I'm very much a philosopher of science, though I hope to avoid the pitfalls associated with too narrow a view I mentioned above. I have aspirations to develop the historical dimensions of my personality – time will tell! The beauty of HPS as a discipline is that all of the kinds of work that fall under its broad tent are fair game. One might easily vary what one does considerably over the course of one's life and still call HPS home. This captures one of the respects in which the IHPST is, as you say, a fairly unique department at the University of Toronto. There is an incredible number of highly diverse things going on here, and yet we are all unified by this absorption with humanistic understandings of the sciences. And because of this, the IHPST functions as a hinge between the sciences and the humanities at the University, allowing students and faculty to reach across that barrier and ultimately equip themselves with skills with which to break the barrier down in all sorts of productive and stimulating ways. This, I think, is something that is absolutely crucial for us to do if we are to be literate and reflective citizens of the world in which we live.

4. After finishing your undergraduate degree in Biophysics, you then moved on to philosophy. What encouraged the transition from science to philosophy? Do you see it as necessary for philosophers of science to have a formal grounding in the sciences?

AC: I have a confession to make – something I've told almost no one (okay, okay, almost everyone I've ever met). While I was specializing in Biophysics, I secretly did a major in Philosophy on the side. A simple disguise and a few extra courses here and there, and I was able to do both. So in one sense, I didn't actually transition from science to philosophy; both were there at the outset. This was part of my master plan to try not to give up on either the sciences or the humanities, because I loved them both. But in another sense, there was indeed a transition, because my desire to do everything notwithstanding, there comes a point at which you really do have to choose a primary focus, and for me that meant becoming a philosopher of science, as opposed to someone who just wants to learn as much as possible about science and philosophy. I suppose the transition began when I had the revelation that most of the questions I'd been asking in my science courses were foundational in character: what is the epistemic significance of the fact that every year, we seemed to be told that what we learned last year was merely a primitive version of, or a limiting case of, the theory we were going to learn this year?; how should apparent inconsistencies in assumptions and ontology across some of our best theories be understood?; how strongly should the scientific evidence we have compel us to believe in various kinds of otherwise mysterious entities and processes? My science professors were very indulgent, but in the end they pointed me in the direction of where I needed to go to dig into these questions as deeply as I could: the philosophy of science.

Formal training in the sciences, or for that matter, formal training in a particular science or some particular theoretical domain, is clearly very useful if your aim is to reflect on these things philosophically: it can't help but be important to know something about what you're reflecting on, and formal training is a great way to acquire that knowledge. But is it necessary? I don't think so. There's no question that you need to acquire whatever knowledge of the relevant science is necessary in order to do good philosophical work concerning it, and I would never advise someone *not* to acquire some formal training if they can. By the same token, though, I would never discourage someone who hasn't had such training if they have the aptitude and the determination to acquire the relevant knowledge however they can. The fact of the matter is that people almost always come into an interdisciplinary field with only some of the pieces of the overall puzzle they

will need in order to do what they aspire to do – it's rare that someone enters with prior training in all the component disciplines. So if you want to work on ancient science, you have to learn Latin, ancient Greek, and so on, and if you want to work on the interpretations of quantum mechanics or the modern synthesis in evolutionary biology, you'd better learn the relevant theories. Some people are extraordinarily gifted and are able to work up the missing pieces of their puzzle on their own, but most need help. I don't think there's any one correct answer to the question of how much help one might need, though, or how much of it should come in the form of training in a classroom setting. The level of detail one needs depends on the sort of work one hopes to do, and there's a vast range of aptitudes and ways in which people learn best.

5. How have you seen the scientific community respond to the work of philosophers of science such as yourself? How do you envision the proper role of philosophy in relation to the sciences?

AC: That's a terrific question, and answering it is tricky, in part because there are different ways in which we might want to think about what's meant by 'science' and 'philosophy of science' here. One way to distinguish the two would be in terms of some precise definitions that clarify the ways in which they're distinct, but that's easier said than done. For one thing, there's something of a consensus that the tidy caricature we all learn in high school regarding "the scientific method" is just that: a rather idealized representation of scientific practice. In reality, the sciences encompass a number of interestingly disparate forms of investigation, from the highly experimental and empirical to the highly theoretical and conceptual, employing a great diversity of methods. So it's hard to say what science is in a way that collects all of those things under one definition and excludes more putatively speculative activities like philosophy at the same time. On the flip side, a number of the ways in which one might define philosophy of science – for example, as an inquiry into the foundations of scientific knowledge and practice, including the epistemology and ontology suggested by scientific theorizing, explanation, prediction, and modeling – sound like ways of characterizing what scientists often do themselves.

Another way of distinguishing science and philosophy of science is in sociological terms: there are people we call scientists and people we call philosophers, each with identifiable communities, institutions of education and employment, and so on; whatever these respective communities are doing is what constitutes the difference between scientific and philosophical inquiry. This allows for the possibility that some scientists may engage in philosophical thinking (and *vice versa*, with respect to some philosophers and scientific thinking), because nothing here precludes some overlap in the relevant communities and job descriptions. In these terms, the answer to your question about how the scientific community has responded to the work of philosophers of science depends a lot on the details of what particular scientists happen to be interested in, and on historical contingencies concerning what sorts of thinking are regarded as "scientific" and "philosophical" at any given time. For instance, in the early stages of the development of the quantum theory (our best theory regarding the very small "particles" out of which everything else is composed), some of its originators were deeply engaged with questions surrounding how best to interpret the theory ontologically. Subsequently, for much of the twentieth century, these questions were ignored in scientific practice, and thus came to be regarded as more strictly philosophical. More recently, a significant number of physicists have again become interested in the foundations of quantum theory. So are these questions scientific (again)? Or are they philosophical questions that are of interest to scientists?

Perhaps you can see why I have such difficulty answering your question! But one thing I *can* say is that if philosophy of science can enhance our understanding of the sciences in any way, shape, or form, that's a good thing. I've been gratified by the interest I've witnessed, on the part of scientists,

in the work that we do in our field (including my own, on occasion). Admittedly, though, for most, a benign neglect of philosophy is more often the rule, sometimes due to a lack of interest, and sometimes due to a genuine bemusement concerning what we're up to!

6. Three years after the publication of your CPA Book Prize winning work, *A Metaphysics for Scientific Realism: Knowing the Unobservable*, how have you found the response to this work in the wider academic community? Your critics have had a chance now to formulate responses to your work. Have there been any critiques you've found useful? Do you feel that there have been any serious misinterpretations?

AC: I've been extraordinarily lucky with the book. With almost complete uniformity, I've found the critical engagement with the ideas I put forward there eminently charitable, and the criticisms and responses more generally inspiringly constructive. It's an occupational hazard in academia that one can't always count on being lucky in these ways, so I'm grateful. The greatest challenge any philosopher faces is not in actually thinking the thoughts (though clearly that's no piece of cake either), but rather in making his or her ideas clear – clear enough to be understood by someone not inside one's own head. So some misinterpretation and confusion is inevitable, and critical engagement often presents a wonderful opportunity to think about how one might prevent such misinterpretation and allay such confusion in future. I've found some critiques especially useful in getting me to think about how the account of scientific realism I argue for might apply to particular cases, and others in forcing me to think in more detail about the relationships between realism, empiricism, and metaphysics. I often find that I understand better what *I* mean after I've read and thought about what other people think I mean. ☺

7. Conferences seem to play a special role in the philosophical community. How do you view this role?

AC: There are a number of roles that conferences fulfill. They introduce initiates, whether students or more senior scholars who are moving into (what is for them) a new area, to what people are saying about topics of contemporary interest. They allow us to put faces to names, to meet people working on similar issues, and to establish and extend fruitful exchanges and collaborations. One side benefit of all of this is that they can become quite fun after a while. There's something inevitably daunting about going to conferences the first few times you do it, especially the big ones. You may not know anyone, and there's no question that it can be downright awkward joining in – interjecting yourself into others peoples' conversations, and so on. But it gets better, and the longer you do it, the more fun it can become, not least catching up with old friends. One of the main reasons I enjoy the Canadian Philosophical Association (CPA) annual conference so much is that it gives me a chance to catch up with one of my best friends from grad school days; I might never see him otherwise! I suppose the most profound role that conferences play is in moving philosophy forward. Philosophers are often pictured (and we often picture ourselves) as highly individual seekers of truth and wisdom, ideally to be left alone in caves until we have something insightful to share with the world. But with some rare exceptions, that picture couldn't be more misleading. Philosophy is a group activity. We learn and make progress by presenting our ideas to others, by having those ideas shaped by rigorous consideration, and by helping our colleagues likewise. This doesn't always happen at conferences, and it often happens elsewhere, but it's the best role for conferences I know.

8. Now I would like to switch to some more theoretical questions about your work and thoughts on contemporary views in philosophy of science today. Because of the substantial role of value theory in the University of Victoria's philosophy department, I will start these more theoretical questions

by asking for your view on the role of ethics in philosophy of science. Do you see a strong link between the two? Is there a role for values, more broadly, in philosophy of science?

AC: Interestingly, the ethical dimensions of science and technology haven't traditionally figured as part of the core of philosophy of science, and this is certainly true of HPS as well, conceived as a professional discipline arising in the 1960s. The philosophy in philosophy of science has been dominated, traditionally, by the epistemology and metaphysics of the sciences. That isn't to say that moral philosophy was excluded entirely, but it certainly wasn't central. I think this is changing, for a number of reasons. For starters, one consequence of the "historical turn" in the philosophy of science in the 1960s was greater mainstream attention paid to the social contexts in which science is practiced and knowledge is generated. These include economic and political dimensions, and inevitably, often intertwined with those, ethical dimensions. A number of influential scholars have since taken up this banner in different ways, including sociologists of science and feminist critics, and many of their revelations have been internalized into the philosophy of science more generally (they certainly figure centrally in much STS). There are, perhaps, more opportunistic forces at work here too. For example, bioethics was never really part of mainstream philosophy of science, but in recent years, as more and more public and private funding has been offered to promote research in this area, some departments with strengths in philosophy of science or HPS have become more open to making it part of their mandate. More broadly, if one were to extend the remit of 'values' beyond strictly ethical values, and include things like pragmatic and epistemic values, there's no question that values have always been an inextricable part of the philosophy of science. But more narrowly, I think that interest in the ethical dimensions of science has never been greater, as philosophers increasingly ponder the social relevance and impact of the sciences, not to mention the social relevance and impact of their own work.

9. After the fall of logical empiricism, Bas van Fraassen is said to have made anti-realist empiricism viable again by formulating a position he called "constructive empiricism". Thirty years later, do you think that position is rationally tenable? If not, why not? If so, why do you nevertheless count yourself as a realist?

AC: Van Fraassen's accomplishment was really quite wonderful. There have always been other motivations for antirealism – essentially, the negation of realism, as I described it above – but when his book introducing constructive empiricism (*The Scientific Image*, 1980) was published, the idea of empiricism as a primary motivation had lost a lot of its luster. This was due in large part to the semantic theory associated with logical empiricism, to the effect that terms for unobservable things (things you can't detect using only your five senses, like an atom of gold, or DNA) are meaningful only in so far as they can be linked to or re-expressed by means of terms for observable things, or possible observations. For a number of reasons, this was ultimately judged to be an unworkable theory of meaning. But constructive empiricism cleverly escapes these worries by adopting a realist semantics; terms for unobservable things are interpreted here in just the way a realist would. The weight of its antirealism is borne instead by the epistemology of the position: constructive empiricists are agnostic about scientific claims regarding unobservable things, whereas realists generally are not.

I suppose that whether one thinks that either constructive empiricism or realism is rational will depend a great deal on what one thinks rationality is, exactly. Certainly, many have argued that if one is going to believe in various observable things, there is no good reason to stop there and not believe in some of the unobservable entities and processes of scientific interest as well. I'm sympathetic to this view, but that isn't quite the same thing as saying that it's irrational to believe otherwise. Van Fraassen himself has a very permissive understanding of rationality, according to

which one is rational so long as one doesn't believe things and act in such a way as to sabotage one's own epistemic projects. To the extent that the constructive empiricist and the scientific realist might be viewed as thinking of science in terms of different epistemic projects (the former holding that science aims to tell us about observable things, and the latter that science aims to tell us about unobservable things too), they might reasonably have different views about what to believe, or so one might argue. I'm sympathetic to this too. So empiricism and realism might both furnish internally consistent and rational ways of interpreting scientific knowledge, though they disagree about what to believe. I'm a realist myself because I believe that, in certain cases, where the evidence is strong enough, we have good reason to think that we know things about unobservable entities. But I'm not sure I'd want to condemn those who are epistemically less adventurous than me as irrational. There are no certainties when it comes to inferring substantive things from the data of scientific observation and experiment, and different judgments as to how far we should go may well be rational.

10. In your book, *A Metaphysics for Scientific Realism* (2007), you noted "Some think there are as many versions of scientific realism as there are scientific realists. That is probably a conservative estimate! There are probably as many versions of realism as there are realists and antirealists." You call your brand "semirealism". What is that and how do you see your form of realism and approach to metaphysics relating to other forms and approaches? For example, following a 1989 paper by John Worrall ("Structural Realism: The Best of Both Worlds?"), there was a lot of interest generated about a position that he called "structural realism". What do you think about the viability of that position today, and how has it influenced your formulation of semirealism?

AC: Semirealism is an attempt to fuse together what I take to be the most compelling insights developed by realists over the years in response to various forms of antirealist scepticism. One thing that has become abundantly clear in the dialectic between these views is that it doesn't make sense to think of realism as some sort of totalizing, absolute position regarding all scientific claims. There is simply too much variation in the evidential support for different parts of our best scientific theories and models for it to be reasonable for anyone to be equally confident about all of them. Some unobservable entities and processes are better confirmed than others, so the trick to being a sophisticated realist is to identify criteria that allow one to identify which parts of theories are most likely to be true, and to make one's degrees of belief (that is, the confidence one has in various propositions) responsive to that sort of assessment. I think this more nuanced approach to realism deflates the most pressing antirealist objections, but of course, there are promissory notes here. What sorts of criteria are indicative of greater epistemic warrant? What sorts of things might one end up being a realist about as a result? These are the questions I tackle in the book. And given that some of the most convincing responses to antirealist scepticism to develop in the literature have come from advocates of particular versions of realism – like so-called entity realism and structural realism – I take semirealism to be closely related to these positions in a number of ways.

Another thing I attempt to do in the book is to give a coherent, unified proposal for what the ontology of the world could be, such that the picture of realism I've just described is tenable. To a large extent this metaphysical proposal is independent of the account of realism it underpins, in the sense that there may be other coherent and unified proposals that would do the same job. But merely to say that there may be others is easy; to demonstrate that there is even one such proposal is challenging. Part of the goal in providing my own demonstration was to show that it can be done – that when, in the defense of their view, realists appeal to things like causal processes, laws of nature, and natural kinds, this is not just a bunch of hot air. Ultimately, whether the metaphysical underpinning I provide is convincing should be judged according to the standards applicable to metaphysics, but as a conceptual resource, I think having it, or something like it, is indispensible to

realism. In this I think my approach to the subject differs significantly from that of many others, who simply take it for granted that the primary concepts most often employed in defending realism are not a lot of hot air after all. But since this is precisely what some antirealists dispute, I don't think it's sufficient to be lackadaisical about this.

There has been an explosion of work on the subject of structural realism in the past ten years. In part this explosion has been fueled by a great deal of confusion about what structural realism is, and this isn't helped by the fact that it's main proponents actually intend a number of very different things. But that isn't necessarily a bad thing – it's evidence of a highly fertile area of philosophical inquiry, and I do think that semirealism can be viewed as a kind of structuralism. All of this work owes a debt to Worrall's resuscitation of the idea from the history of twentieth century philosophy, where it cropped up in different forms in the work of a number of important philosophers and scientists. Worrall's own mature explication of what *he* means by structural realism is a lot closer to empiricism and instrumentalism and further from what most people have in mind by realism than was obvious from his seminal 1989 paper (I elaborate this a bit in a recent paper: 'Scientific Realism and Ontological Relativity', *The Monist* 94: 157-180). His view and others in the same vein (including more robustly realist variants) are epistemic versions of structuralism, in that they claim that our best scientific theories tell us only about the structural features of unobservables, and nothing else. There are also a number of so-called ontic versions, which agree that we have only structural knowledge of the unobservable, and deny that there is anything other than structure in the world; that is, they deny that there are entities that stand in the relations that putatively constitute structures, or contend that if there are such entities, they're ontologically dependent on their relations in some way. Determining the viability of these positions is very much an ongoing research program, and I think that some are more promising than others! It's the parts I find most compelling that I've attempted to build into semirealism.

11. In *A Metaphysics for Scientific Realism*, you draw distinctions between detection properties and auxiliary properties, and between detectables and undetectables. What do you think the relevance of these distinctions is for our current epistemic attitudes towards science?

AC: I believe that these distinctions are at the heart of what it means to be a sophisticated realist – one who takes antirealist challenges seriously and aims to elaborate her epistemic commitments in a measured, non-totalizing, defensible sort of way. Much of the debate between realists and antirealists concerns the epistemic significance of the distinction between observables and unobservables (in the sense I mentioned above), but I think finer-grained distinctions are also epistemically relevant, and that's one reason I divide the category of unobservables into detectables (entities and processes that we've putatively detected using instruments) and undetectables (entities and processes that we haven't detected, but have posited for theoretical or explanatory reasons). I think we typically order our degrees of belief, going from stronger to weaker, as we move from claims about entities that are detected routinely and in a number of different ways, to ones that are detected with less ease or perhaps in only one way, to those that are merely posited but for extremely important theoretical or explanatory purposes, to those that are posited for less convincing reasons. There may be exceptions, but that's the general pattern. It's evidenced, for instance, in the greater confidence we typically have in the existence of an entity when it passes from the realm of the undetectable to the realm of the detectable. That sort of nuance should form part of any sophisticated realism.

The distinction between detection properties and auxiliary properties is likewise intended to capture something about our epistemic situation: indeed, one might think of it as a special case of the distinction between detectables and undetectables, in which the relevant entities are

properties. The idea is that our confidence in the reality of the properties of things should likewise vary in just the way I described confidence varying in the case of detectables and undetectables more generally – with our putative success in detecting them. Detection properties are those we have putatively detected, and auxiliary properties are any other properties described by our theories. A property that's auxiliary given our current scientific capabilities may become detectable over time, in which case it becomes a more defensible part of a realist interpretation of the theory. In the book, I recommend belief in detection properties and agnosticism about auxiliary properties, hence the term 'semirealism': a maximally defensible realism, I argue, is one that takes some parts of science more seriously, ontologically speaking, than others. Of course, there's much more to be said about how one should differentiate these parts, but in the end, determining how well detected a property is and what degree of belief should follow constitutes the hard work of realism, and inevitably, there's room here for some differences in assessment and opinion. Once we determine what scientific properties are good targets for realism, I argue that our conceptions of scientific objects, laws, and kinds can be built up from there, but that's a longer story!

12. You defend a metaphysical thesis called the "dispositional identity thesis". Can you tell me a bit about that position and why you favour it over alternatives?

AC: One of the questions I'm interested in is that of what these properties of scientific interest are, precisely. This is ultimately a question about the fundamental nature of properties, or their identity. What is it that makes electric charge the property that it is? One answer to this question, proposed by a number of philosophers (differing somewhat in the details), is that what makes a property the property that it is, are the dispositions it confers on things that have it. So if entities having negative charge are disposed to repel other entities with negative charge, and to attract entities with positive charge, these dispositions are part of the nature of charge – they constitute (in part, since negative charge is also associated with other dispositions) the identity of charge, hence the 'dispositional identity thesis'. There are reasons one might be concerned about this idea. For one thing, some empiricist-minded philosophers are suspicious of the notion of dispositions (and other notions that are often invoked more or less synonymously, like those of capacities, tendencies, and causal powers), which they hold to be insufficiently empirically grounded. Also, this dispositional identity thesis has the consequence that laws concerning these properties are strongly necessary, since if they were different, given the identity thesis, they would no longer concern the same properties. But some philosophers have a strong intuition that, for example, laws concerning the behaviours of objects with negative charge might have been different than what they are.

One important thing to keep in mind about these disputes is that at this level of fundamentality, we are reaching conceptual bedrock, so inevitably, primitive notions come into play. Some may be uncomfortable with the notion of dispositions as primitive elements of one's ontology, but I think the alternatives are even stranger. For example, on the rival, "categoricalist" theory of properties, what makes a property the property that it is, is itself something primitive, usually labeled with the term 'quiddity'. This may satisfy the intuition of some philosophers that one and the same property might have figured in different laws of nature, but it also has the consequence that *any* causal profile *at all* is compatible with the identity of a given property – in the lingo, for any given causal profile, there is a possible world in which that property has it. This seems to me a bizarre way to make sense of our ordinary talk of properties, let alone property talk in the scientific domain. The empiricist concern about the supposed spookiness of dispositions is strongly mitigated, I think, by the challenge of making sense of talk of modality (that is, necessity and possibility) in the sciences and more generally. The metaphysics of modality is notoriously difficult to elaborate, and dispositions yield a neat way of talking about what sorts of things are possible, must happen in certain circumstances, and so on. At the end of the day, I think metaphysical arguments of this sort

can only be so convincing, but recall my aim, which is to show that there is, in fact, a coherent understanding of properties that can be unified within a framework of likewise coherent accounts of the various other crucial concepts underpinning a defensible scientific realism. To that end, I argue that a number of interesting objections to the dispositional identity thesis can be dissolved.

13. *A Metaphysics for Scientific Realism* seems to be primarily aimed at furnishing a metaphysical picture for a plausible version of scientific realism. Does your current work share that aim, or have you moved on to new projects?

AC: Every time I think I've said what I want to say about realism and its metaphysical basis, and that it's time to move on, something comes up… So never say never, I suppose. (I mean that in a dignified Sean-Connery-as-James-Bond sort of way, not an I-just-changed-my-haircut-Justin-Bieber sort of way.) My current work has branched out from this foundation along several different paths. Some of it focuses on the constellation of issues surrounding scientific models, representation, and related topics like the nature of abstraction and idealization, and how these practices should influence our thinking about scientific knowledge. Another bundle of work focuses on some meta-philosophical issues concerning the relationship between science, metaphysics, and the philosophy of science. Although these new philosophical obsessions are clearly connected to the earlier projects, they've taken on a life of their own now, and though a number of apparently disparate papers have resulted, they're all connected in my mind, around the theme of how we should best think about the aims of these things we call science…which means that there's another book in the offing. So stay tuned! And thanks so much for your interest and these excellent questions.