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Philosophy without borders, naturally: an interview with Harold Kincaid

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Harold Kincaid's research interests are wide-ranging. While he started his career tackling more general philosophy of science issues, his work addresses topics in the philosophy of the social sciences, philosophy of economics, naturalism, philosophy of medicine, and he also conducts empirical work. He is the author of *Philosophical foundations of the social sciences: analyzing controversies in social research* (1996) and of *Individualism and the unity of science: essays on reduction, explanation, and the special sciences* (1997). He also edited or co-edited a large number of books: *Toward a sociological imagination: bridging specialized fields* (Phillips, Kincaid, and Scheff 2002), *Value-free science?: ideals and illusions* (Kincaid, Dupré, and Wylie 2007), *Establishing medical reality: essays in the metaphysics and epistemology of biomedical science* (Kincaid and McKitrick 2007), *Distributed cognition and the will: individual volition and social context* (Ross et al. 2007), *The Oxford handbook of philosophy of economics* (Kincaid and Ross 2009), *What is addiction?* (Ross et al. 2010), *The Oxford handbook of philosophy of social science* (2012), *Scientific metaphysics* (Ross, Ladyman, and Kincaid 2013), *Classifying psychopathology: mental kinds and natural Kinds* (Kincaid and Sullivan 2014), *Routledge companion to philosophy of medicine* (Solomon, Simon, and Kincaid 2017). His work has been published in *Philosophy of Science*, *Journal of Economic Methodology*, *Philosophy of the Social Sciences*, *Synthese*, *The Monist*, and *Analyse & Kritik*, among others. He currently carries out a research funded by the South African National Research Foundation titled *Understanding*

EJPE'S NOTE: this interview was conducted by Philippe Verreault-Julien. He is a PhD candidate at the Erasmus Institute for Philosophy and Economics and co-editor of the Erasmus Journal for Philosophy and Economics. His research concerns the epistemology of economic modelling.

addiction: using economic experiments to understand the dynamic of tobacco smoking.

The Erasmus Journal for Philosophy and Economics (EJPE) interviewed Harold Kincaid about, amongst others, what led him to have this diverse set of interests, his views on laws and causality, the relationships between philosophy and science, between economics and the social sciences, and conducting empirical work.

EJPE: Professor Kincaid, most of your formal education is in philosophy. What brought you to be interested in the social sciences in general and to economics in particular? Were you interested in them from the beginning or is it something that you developed during your studies?

HAROLD KINCAID: I started out as a political science major as an undergrad and was involved in various left wing social causes at the same time (this was in the early 1970s when the civil rights, anti-war, and women's movements were very present), so I had a deep interest in the social sciences from early on. I found the US political science of the time however not that interesting or rigorous and had some really terrific philosophy mentors so switched majors. Then in grad school I was required to have a minor and I did economics. So I have always had a deep interest in the social sciences and my philosophy interests were primarily in philosophy of science, thus the two combined naturally.

And were you attracted to economics because of some perceived shortcomings of the discipline, perhaps similar to those you identified about US political science, or because you thought it was by and large successful? And what did you think back then was the role of philosophical reflection with respect to the social sciences?

I found the models very rigorous compared to political science at the time (political science has much improved in this respect, in no small part by borrowing from economics). But, yes, I also went into my economics education as a critic, because I thought economics left out much—what sociology provided such as class and social structural analysis—and because there was little concern about dealing with the problems of highly unrealistic models. I had a heavy dose of Quine from age 18 on, so I always thought philosophy of science was continuous with and constrained by the science. I don't think I thought philosophy was going to fix the problems of economics, but then it was clear philosophy of science issues were there and needed to be addressed.

Do you consider that the criticisms you had towards economics have been dealt with satisfactorily during the past thirty years? For instance, do you see economics as having incorporated relevant insights from sociology? And do you believe that philosophy was instrumental in bringing about (positive) changes to economics, or these were mostly internal to the discipline?

Well, no. I think the insights incorporated from sociology are pretty thin and uninfluential. Basically it is one insight—that institutions matter. Yet the notion of institution is basically the rules of the game. I am no historian of economics, but my guess is that the development of game theory was more important here—the rules of the game, of course, have a natural game theory interpretation—rather than borrowing from sociology. I do not think philosophy had any influence on this.

Especially in your earlier work (Kincaid 1988; 1990; but also 2004), you defended the existence of social laws. Do you still think they exist? How have your views evolved on this topic?

I think my views have "evolved". Maybe that is just a weasel word for "completely changed", but I do not think so. From the beginning of my little career I have thought that the social sciences can pick out causes but in a piecemeal, stepwise process. So in the 1996 book I argued that claims qualified 'ceteris paribus' could still be confirmed if we could show that unknown factors had minimal influence, that claims held up whatever the complicating factors were, etc. I did not think and still do not think that laws are the main issue. Instead, to me the big issue is when and where we can have reasonable evidence about causes. Knowing that *A* causes *B* probably entails some kind of law, but I do not think we need first to confirm the law to have evidence for the causal claim.

So you would agree with Hausman (2009), for instance, who says that 'causation' is a more useful concept for philosophy of economics than the one of 'law'? And what are the approaches to causation you find promising for economics in particular and for the social sciences in general?

Yes, I certainly do think causation is more useful and what gets at the law notion in practice is "well established association". I do not think causal notions and inferences have gotten much of a foothold in economics. Economists have randomized control trials (RCTs) and natural experiments, but outside of that, they still talk of "determinants" and other ambiguous terms, and cite the 'correlation is not causation' mantra, all the while making policy recommendations—which is inconsistent, in my view.

There are two things to note about causation in economics: First, the Pearl (2009) et al. framework is a revolution and we now know how to formulate and test causal claims with a rigor we did not have 20 years ago. Unfortunately, economics has been very slow to take advantage of these advances. Economists will talk about "endogeneity" and use instrumental variables to deal with it, but the instrumental variables framework was and still is largely motivated by concerns about consistent estimation, not causation (why we care about whether inferences converge in infinite samples escapes me by the way). Explicit causal models are still fairly rare. It is still standard to assume that the right hand side variables are each independent causes when that is obviously false. The cross-country growth regressions are an egregious example—there is no way that education, rule of law, and so on do not causally interact. Explicit causal models can try to deal with this but they are still rare. And an enormous simple fallacy shows up across the field. If cause *A* influences outcome *E* only, or partly by influencing intermediate *M*, then standard multiple regression techniques will conclude that *A* has no effect or much less effect than in fact is the case. Controlling for an intermediate cause removes the correlation between the distal cause and the effect. This is a huge problem in much applied work.

Then as much as I like Pearl-type models, it also seems that they assume a picture of causation that sometimes/often may not hold in the economy. The causal reality may involve moderating causes (causes that do not cause the effect, but influence how much influence various causal factors have) as well as necessary causes (factors that have no independent influence but are part of the causal constellation as it were), thresholds (nonlinear relations), and a host of other complexities. The Pearl framework does not easily handle these, though maybe useful extensions are possible—to me, the jury is still out on these issues.

Are you then mostly skeptical of inductive approaches such as "mostly harmless econometrics" (Angrist and Pischke 2008)?

Well these are complicated issues and if I had this all figured out I would have published a block buster article. Maybe that will happen soon… I would look at articles by Deaton (2010), Cartwright (2011), and Harrison (2011) among others. A first point: the economics literature on instrumental variables is a mess. Instrumental variables (IVs) were first introduced to handle consistency issues in the statistical technical sense of converging in infinite samples (I ask again in my Bayesian mode while I should care about samples I will never have?) and then slowly and ambiguously morphed into solutions for dealing with confounding by variables economists do not include, because they do not have measurements or even know what they are. The literature still is not clear on this. If I can find an IV that I am sure does not cause the dependent variable and does cause the effect variable, then great. But that is hard. Many empirical studies, e.g., in the growth literature (Barro [1998] is a prime example), never show that and are not even aware of the problem. It seems to me that "structural models"—ones that do not claim to mimic randomization but try to test a full causal model against the data—are just as likely to be convincing. I am pretty sure that a claim that inflation rates over three hundred percent reduces growth has more evidence going for it than any economics result from an RCT. But, again, these are complicated issues, and the best I can do is to put some bugs in peoples' ears, as we would say in the US Midwest where I grew up.

You have been an active participant in debates over methodological individualism and reductionism (e.g., Kincaid 1986; 1990; 1993; 1997; 2014). How would you qualify the progress we have made in our understanding of these issues in the past decades?

I think four things have been conclusively demonstrated. This does not mean everybody has gotten the message, especially social scientists. The first result that has been established conclusively is that even if society is composed of individuals (and the story is really more complex than that), nothing inevitably follows about our ability to explain in terms of individuals. This is an instance of general moral about parts, wholes, levels, and explanation that was first shown in debates over physicalism in the 1970s—e.g., Putnam's peg example—and has been elaborated on in various ways since.

Secondly, reduction in the Nagel (1961) bridge law sense in the social sciences from social explanation to purely individualist explanation is quite implausible. It is not clear there are any successful cases and a variety of reasons it is unlikely to be done, chief among them simply that good social explanations are almost always of individuals and institutions, organizations, and other social entities in mutual interaction.

A third result is that individualism as a claim about mechanisms is really a bunch of independent claims, some of which are very plausible and others that are not. For example, in the social sciences, as in science in general, there can be well-confirmed explanations entirely in macro terms, but supplying detail at the micro-level is usually evidentially and explanatorily useful. I think we have learned that quick, blanket pronouncements about mechanisms and individualism are not helpful.

Fourthly, there are many current empirical debates in the social sciences about how far we can go in explaining in terms of individuals and how much social organization, social structure, etc. do we need. I have argued in a couple of recent pieces (e.g., Kincaid 2014; 2015) that many social science controversies turn on this question and that the evidence has to be assessed case by case and in a modest way that says 'here is how things seem at the current time'. Sometimes we can be relatively individualist, and other times not.

Are there issues in particular where you wish social scientists, especially economists, would have received the message? Or are there areas where you think a commitment to methodological individualism impedes rather than foster scientific progress?

I have a long list that I keep. Two important and obvious ones that interest me now are macroeconomics and studies of race. Dynamic stochastic general equilibrium (DSGE) models dominate macroeconomics so far as I can see and have had pretty unfortunate consequences—they are strongly motivated by the idea of individualism, though what they do is a bit of a farce if you want to have individualist foundations, since the 'individuals' are representative agents. In studies of racial disadvantage, individualism shows up as an emphasis on individual attitudes leading to discrimination and individual characteristics such lack of human capital leading to differential outcomes for blacks and whites in the US. These factors are not irrelevant, but they miss really important structural, macro forces—

spatial segregation and character of neighborhoods, quality of schools, government policies on sentencing, etc. in the present, and then a host of past institutional/social forces—slavery, legalized discrimination in education, housing, employment, wages, and so on, that have systemic effects now and will indefinitely.

And to what extent do you consider that behavioral economics deviates, or is consistent with methodological individualism? Does it sufficiently take into account the social embeddedness of individuals?
Nice question which I have not thought much about and do not know that others have either, but I may be missing some literature. In very general terms, to the extent that behavioral economic models have a richer preference structure, then they implicitly presuppose a more robust social influence to explain them. Framing effects would be a prime example. Yet major criticisms of behavioral economics along the lines that in real market environments laboratory results supporting behavioral economic models do not hold up because the social environment channels behavior to a more traditional rational choice pole might seem to argue in the opposite direction.

You have in various occasions (e.g., Kincaid 1993; 1996; 1997; 2014; 2015) warned against philosophical blanket claims in the individualism-holism debate. Yet, you also seem to believe philosophers have a genuine contribution to make. Could you elaborate on how you see the specific contribution of philosophy to this debate?
If philosophers want to provide an all-purpose answer on the individualism-holism debate by conceptual analysis or whatever, they will not add much. Yet if they get into the nitty gritty details of social explanations, they certainly can help with much more localized claims. Philosophers I still think are well trained to sort out theses at issue and the kind of arguments made for them. For example, Don Ross has done some nice and interesting work on versions of individualism in economics (e.g., Ross 2005). So has Kevin Hoover (2001; 2008)—who is a closet philosopher—on microfoundations. Economists and social scientists in general are still inclined to give weak meta-empirical arguments for or against—for example, the argument mentioned before that society is composed of individuals and therefore everything can be explained in terms of individual or the holist argument that society

causes or constrains individuals and therefore explanations in terms of individuals must always fail. Philosophers have helped to keep social scientists honest when they invoke bad philosophical arguments of this sort.

This seems to be related to the more explicit naturalist position you have advocated recently (e.g., Kincaid 2012; Ross, Ladyman, and Kincaid 2013). Could you tell us what sparked this interest in naturalism (Quine perhaps?) and what is it, for you, to be a naturalist?
It is hard to give a sufficiently careful formulation within the context of an interview, but naturalism for me as a general thesis—not one specifically about the social sciences—is just the claim that there is no specifically philosophical method or knowledge independent of scientific evidence. Science and philosophy (where ethics and political philosophy fit in is a big question I will not pronounce on) are continuous. There are lots of avowals of naturalism out there among philosophers where all the doctrine comes to is that you should consider scientific results "relevant" to philosophical concerns. Well, on a total evidence or Bayesian perspective, that is trivial and no real constraint. So, on my view, attempts by scientific realists or antirealists to provide general arguments showing that science is or is not believable are misguided. Philosophers are in no position to show such things (illusions of grandeur sparked by being in a profession without data?). Philosophers can make good arguments about what the data show or do not show in specific instances, I think, and to me that is the way to go. The general view I am advocating here gets a very nice presentation in Penelope Maddy's *Second philosophy* (2007). Yes, Quine is behind much of this, but as Maddy argues, Quine can be improved on in these regards.

Your own work has often been about very general concepts like 'laws', 'individualism', 'explanation', 'realism', etc. How do you conceive the prima facie tension between naturalism and the philosophical investigation of these concepts?
Off and on I get lulled into doing general conceptual analysis (that was my training in large part, so I cannot help but look for necessary and sufficient conditions if I am not careful). Yet on my more consistent days—which I hope are the majority—I have argued that all these issues are local, empirical ones that have no general answers. I certainly have

argued repeatedly that the individualism question is really many different questions, ultimately empirical questions, that have to be assessed case by case. My piece on inference to the best explanation (IBE) in the early 1990s (Day and Kincaid 1994) had that as its main point as have other more recent pieces on realism (Kincaid 2000; 2008), including two forthcoming pieces on realism in economics and pieces on race and class (Kincaid 2016). Also, I have several articles on causation with a similar theme (e.g., Kincaid 2009; 2011; 2012). Hopefully most of my work is about what the science shows us and how to understand (social) scientific controversies, not to give a general theory of laws, for example, which I suspect is a dead end.

I guess we can fairly say that philosophers of economics and of the social sciences now pay more attention to how science is actually conducted. In what areas do you think this 'naturalist turn' has been successfully applied and the ones where it has not?

Again, a hard question and not one I think I can pronounce on with any confidence (and there is a philosophy of social science/economics community that I value over making partly thought out judgements). The problem is telling who really is trying to be naturalist and when the label is only used rhetorically. If I am going to throw stones, the social ontology literature comes to mind immediately. Much of that has no clear connection to real social science. But I do not know how naturalist its advocates intend to be. Searle (e.g., 2009) is quite explicit in saying we need to first get clear on the social ontology before we do the social science, a very un-naturalist approach, though I assume he would describe himself as in the naturalist tradition. Certainly, there is still plenty of general philosophy of social science that is armchair conceptual analysis very distant from real social research. But on the whole philosophy of economics seems to me to have become over the last 20 years much more attuned to real economic research in good naturalist fashion.

You have been an active participant in philosophy of the social sciences and of economics and have edited handbooks (Kincaid and Ross 2009; Kincaid 2012) on the fields. What is, according to you, the relationship between philosophy of the social sciences and philosophy of economics? How do you see the connection between the two?

I only see difference where there is justified difference in problems, methods, etc. of the fields themselves. How much difference there is and ought to be is a great question. As a factual matter I think the differences in the social sciences has narrowed as game theory, other formal models, and experiments are much more common across fields than they used to be. A related big issue is the place of formal models that are highly idealized. That is still a huge issue for philosophy of economics, but less so for general philosophy of social science, though it is increasing important there as well. Economics has also been realizing that institutional details, social networks, etc. matter, though on my view there is still a long way to go. I have been putting this issue in terms of the slogan "How individualist can we be?" and I think it gives a way of framing lots of debates that are up in the air in economics.

You say that the differences in the social sciences have narrowed. What do you think of the discussion over 'economics imperialism' (e.g., Fine and Milonakis 2009; Mäki 2009; Vromen 2009)? Are the narrowing differences justified?

I wish I had a fully worked out answer but I do not—its on the to do list for my research. There are lots of complicated questions here; see for example Don Ross's work (e.g., 2005; 2014). But here are some general considerations.

First, there is no right answer about what economics is about. Instead you have to look at various modeling approaches and ways of getting evidence and see how far they get you in explaining social phenomena.

Second, if you think of economics as providing a perfectly general theory of choice, then of course it should generalize across multiple social domains. Yet there are large caveats here even if you buy this idea. First, there is the individualism issue: many social science explanations are about macro, aggregate phenomena where the role of individual behavior and choice in explanation is up for grabs. Second of course is the behaviorist critique—if you want a perfectly general theory of choice, expected utility theory may be too narrow, though I think there is a fair amount of hype and bandwagon effects around behavioral economics. And, of course, many applications of economic rational choice models outside economics are only as if-just so stories with questionable empirical warrant I would guess.

Third, if you think of constrained maximization in markets as the key to economic analysis, as I would tend to do, then to what extent can you think of most social phenomena as market like? And more basically, to echo the work of Phil Mirowski (1994), what is a market? It is not so clear there is a defensible generic conception of a 'market'.

Fourth, a certain pragmatism however seems to be called for. If I can get interesting successful predictions out treating aggregate social entities as maximizing a utility function, then all the better. There is plenty of work like this in political science for example.

Fifth, then, a really big question to me is how far we can get in explaining markets without doing sociology. My bet is there is no general answer—it depends on the market. "New institution economics", for example, suggests ways to bring in the social, though I think there is much more potentially to say.

How is your work in philosophy of medicine (e.g., Kincaid and Sullivan 2014; Ross, Kincaid, and Spurrett 2010; Solomon, Simon, and Kincaid 2017) related to what you have done in the philosophy of social sciences? And what have you learned about medicine that could be of interest for philosophers of the social sciences?

I think several things are relevant. The first is that explanation goes on without elaborate formalized theories; instead progress comes from piecemeal, localized causal explanations.

The second is that classification and the development of defensible categorizations is a complex and absolutely fundamental endeavor, but the basic process is one Mill or Hempel would recognize: do you have multiple ways of measuring a given concept and do the concepts allow for successful explanations. The psychometric tradition in the study of psychopathology works hard at this, though they are overly enamored of purely formal methods and do not take causality sufficiently into account—I would prefer an explicit structural equation causal approach.

The third is that classification can be quite local, pragmatic, and plural if that helps provide successful explanation; cancer is a clear case where there is not one "thing" and local context matters and that is certainly true for psychopathology.

The fourth is that classification and categories can be messy in that they may pick out well defined core groups but with lots of individuals with a fuzzy, ill-defined relation to the concept. Depression and addiction are like this. I would bet "underclass" for example is like this.

A last one is that social constructionist skeptical doubts have to be taken seriously even in the best of biomedical sciences—how the science proceeds can be a mix of social processes, some of which are truth conducive and some of which are not.

The morals for philosophy of social science and economics are just that since these are traits of biomedicine, we should not be surprised to find them to hold of the social sciences. I have a recent paper on social class (Kincaid 2016) and hopefully one coming out on race, caste and ethnicity that directly embody these ideas. But lots of other people have pursued approaches to the social sciences like these.

It does seem then that conversely the relationship is not a one-way street; philosophy of social science can also contribute to our understanding and the conduct of the biomedical sciences. Could you expand on what you think are some key areas for contribution?

There are multiple possible ways philosophy of social science might contribute, but to what extent or whether it has, is an open question, and haven't looked at this carefully. Reductionism and mechanisms is one area. Lots of thought has gone into these issues in the philosophy of social science that might help deepen these discussions in biomedical science. Discussions of causation and laws is another. Both the social sciences and areas in biomedicine seem to me have various kinds of complex causes, e.g., necessary but not sufficient causes, path dependence, etc., and both are probably short on general laws, so philosophy of social science work on these ought to be relevant to philosophy of science questions in biomedical science. The role of the social in knowledge production is another and as are fact/value questions. I am sure there are more possibilities.

Don Ross has been a close collaborator of yours for now over ten years. How did Don and you start working together?

"Collaborating" is not quite the right word. We have only coauthored a couple of papers together. But you are right in that we have coedited a number of volumes and have been conducting empirical studies together (surveys and experiments on behavioral economics issues) for a decade and that is still ongoing. I was department chair at the University of Alabama in Birmingham, and Don applied for an opening we had; we were very lucky to get him to come. It turned out our interests and approaches overlap enormously—we only vaguely knew each other's work before he joined the department. But once he did, it was a natural to work on joint projects and as importantly to have regular intellectual back and forth.

The fact that you are involved in empirical studies is rather unusual for a philosopher, especially for someone who worked on very general and foundational philosophical issues. It seems natural—no pun intended—considering your naturalist position, yet few philosophers actually conduct this sort of work. How did you start doing empirical research? What relationship do you have with the other properly social scientists you are working with?
Well, yes it is no pun that my naturalist tendencies naturally lead to this, but then it is also a matter of circumstance and opportunity. My general mantra that you cannot evaluate social science on general conceptual grounds but have to look at the empirical details obviously drove me to better understand empirical methods. In the 1996 *Philosophical foundations* book I proposed some (no doubt simplistic) possible regression models for assessing functional explanations in defense of their scientific status. I have written multiple papers on growth theory and there the main source of evidence—cross-country regressions—just begs for a critical analysis of the evidence. I was teaching a graduate epidemiology course on causal inference at the same time I wrote these and it was natural to actually learn and apply those methods to actually analyzing real data.

Then, as usual, Don Ross was an influence. He got grant money for a national prevalence study of addictive gambling in South Africa and included me. That led me to learn techniques for deciding whether symptoms that we were recording should be seen as continuous or dichotomous. There are obviously more general philosophical issues about concepts, classification, etc. lurking here, and these issues tied into my interests in the status of psychiatric diagnoses. Then I moved into the School of Economics at UCT where my teaching was graduate behavioral economics and supervising empirical master's dissertations, so again incentives to learn more empirical techniques was obvious. Decisive recently is that I have been part of an international team (the experimental economist Glenn Harrison at Georgia State is the intellectual heavy weight here) that has been successful in getting grant funding for empirical work—on experimental eliciting risk and time preferences and other behavioral economic variables among farmers, urban poor, and addicts, with more projects waiting on grant success. In addition, I am now involved in a large German funded, four country empirical field study on clientelism with another set of social scientists.

My relation with these collaborators? Probably best to ask them! It is in part philosopher of science embedding with the natives, in part my research ranking in South Africa helps with grants, in part my collaborators are especially philosophically astute and see some value in having a philosopher of science on board, and then I think because there are inevitably philosophy of science issues lurking in these studies and so then I may have something to add. A philosopher of science who knows the empirical literature and has the analytic skills typical of the profession can make a contribution I would hope.

And has doing empirical work influenced your own thinking, or has your philosophical beliefs remained rather stable while carrying out this kind of research? What are the most valuable things that you have learned?

A major thesis I have defended for some time is that social science can be good science by the broad standards of the natural sciences. I have not changed my mind on this as a result of the empirical work, but now have a much greater appreciation of the complexities and nuances involved. Our survey work on the prevalence of pathological gambling was encouraging in this regard. We used a relatively simple nine-question screen about problem gambling and I was able using relatively novel taxometric methods to provide evidence that it picked out a distinct group of individuals with addictive gambling behaviors; that result has been confirmed in a different sample by different investigators. So, encouraging.

Ironically, maybe, our experimental work is actually less reassuring (reversing the usual experimental equals reliable and observational is unreliable trope that is common). I am impressed by the number of factors needed to draw conclusions in experiments that are important but that you cannot have much confidence in. We have lots of subjects doing risk and time attitude lotteries for real money who prefer, for example, $100 now over $500 now. Do they understand the task? Are they hiding their true preferences for some reason? In general, there are many decisions that have to be made in setting up the experiment and interpreting the data that are just not clear cut. The people I work with have many sophisticated ways of dealing with these issues. Still the experimental work has brought home how hard it is to get really conclusive results. This is in the abstract no surprise—see Gallison's (1987) *How experiments end* about particle physics for similar observations—but nonetheless sobering.

The many empirical theses I have supervised almost universally do correlational analysis—regressions of various kinds—on observational data with no clear causal interpretation. That is standard practice in the field, so I go along with it—unfair to students to demand much more—but still you have to wonder what this work shows. None of this is going to turn me into a postmodernist (I still love Glymour's [1980] quip that if there are only two types of people—positivists and blank blank English professors, then I am a positivist), but it definitely reaffirms my conviction that you really have to get involved with the details of social science research to make judgments.

Do you have any advice for young aspiring philosophers of economics/the social sciences? What skills should they develop? What are promising or underrated areas for research?

The no brainer is to learn lots of social science and especially social science research methods. Take stats courses, qualitative methods, etc. It is hard to do this once you graduate and have lots of responsibilities. It will pay off later.

Archeology, anthropology, demography, behavioral ecology, much political science, growth theory/development economics, RCTs in development economics, macroeconomics, network theory, social psychology, and on and on. There is an enormous range of social science that has barely been looked at by philosophers of social science. My general strategy is always to look for controversies in such areas and see what philosophical tools can contribute. No doubt there are other ways to proceed, but this has worked for me.

Of all the books (fiction and non-fiction), what are three that you would recommend or had a major influence on you?

I am not sure what the dependent variable is… my beliefs? How do we measure that and is it a single continuous variable! Probably not. So completely obvious books are Kuhn's *The structure of scientific revolutions* (1962), Marx's *The German ideology* (1998), and Mannheim's *Ideology and utopia* (1936). Less obvious ones are Quine's *Ontological relativity* (1969), Michael Williams's *Groundless belief* (1977), Penelope

Maddy's *Second philosophy* (2007), Jeffrey Paige's *Agrarian revolutions* (1975), and Brown and Harris's (1978) *Social origins of depression: a study of psychiatric disorder in women*—the last two because both are very successful pieces of social science research. Pearl's *Causality* (2009) and Abbott's *Time matters* (2001) I would also recommend.

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