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Chapter 1

Introduction

This is a book on the philosophy of political science. It could have been a book on the philosophy of social science, but to cover the broader remit of this discipline in the way I have tried to do for political science would have involved my learning a lot more about what goes on in other social sciences, such as economics, sociology, history (yes, history is a social science) and particularly psychology and social psychology, of which I know less. Nevertheless, I hope it might be of some interest to social scientists in disciplines beyond political science. It is not a methods book as such. It does not try to teach any political science methods, although I discuss various methods and what one might achieve with them, and make some recommendations as to how to go about studying some issues. The principal aim of the book is to examine some philosophical issues through the lens of political science methodology. In order to do that, I need to cover a little ground on relevant methods and discuss their justification; for a full grounding in methods, the reader will need to consult the texts referred to in this book.

There are many textbooks on the philosophy of social science and many more books on social scientific methods – both general ones and those dealing with specific methods. Why, then, another one? Well, Steven Kennedy of Palgrave Macmillan pressed me to write this book with the enthusiasm for which he is famous, for one reason; for another, I think there is a disjunction between texts on the philosophy of social science and politics methods texts. The problem with some books on the philosophy of social science is that their authors are philosophers who do not engage in empirical social science. These books tend to be rather general, and whilst they cover important issues in philosophy they do not make it clear why these issues should matter to working social scientists. Does it make any difference to a legislative studies scholar whether scientific realism can be justified? If not, why should that issue be covered in a book on the philosophy of political science?

On the other hand, when political scientists tackle philosophical questions they do not always realize the technical issues involved, and make what philosophers consider to be rather naive inferences and blunders. Furthermore, what political scientists say about philosophy tends often to be a generation or two out of date. While I am by no means a philosopher, I have some training in the discipline, and have tried to ensure the claims

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made here have some philosophical pedigree. I am experienced in using many different empirical methods, both qualitative and quantitative. I try to expose the reader to philosophical arguments that are relevant to high-level political science research – while attempting to avoid the dual pitfalls of philosophical naivety and ignorance of empirical practicalities. Doubtless some people will think I have failed in both regards.

This book is an exploration of the philosophy of social science and a defence of mainstream empirical analysis. I make no bones about the fact that I think that some anti-empirical tracts are based on a general ignorance of both empirical techniques and philosophy. Several recent books and articles suggest that political scientists need to be aware of their ontological and epistemological commitments prior to embarking upon research. The authors of these works discuss ‘ontological and epistemological commitments’ as though they are preferences that one simply picks or chooses from the menu (that they thoughtfully set out for the reader). They suggest that these commitments cannot be empirically examined, but then seem to infer from that claim that they cannot be examined at all. That is not true: one can examine the logic, the coherence and the honesty of ‘ontological commitments’. In other words, do these commitments make sense? Are the writers consistent in their claims? And, just as importantly, do they live their lives as though they believe the ontological and epistemological claims they profess in their academic writings, or are their academic publications simply ‘cheap talk’ – a tool to further their careers, but not something they live by? In short, are they academic hypocrites? Chapter 10 of this book explains why I think academic honesty matters.

Another aspect of this book that contrasts with many others is the fact that I use a lot of examples drawn from many parts of political science and public administration. Some merely illustrate a point I am trying to make. Others are discussed in some depth because I think that the philosophical issues I examine require them to be explained in some detail. I also draw on a lot of examples from outside of political science; some readers will think too many. However, they are used to show complexities that exist even in simple cases before we turn to examples from political science that are usually more complex still. Philosophers like to use the simplest possible examples to illustrate just how problematic they can be.

I draw extensively upon some of my own empirical research in this book. I do so because I am intimately aware of what my colleagues and I were trying to achieve in that research. I know why we designed it as we did, what mistakes we made, how we could have done better. Textbooks often set out the grounds of good research design, but the reality is always messier than the ideal. One sometimes realizes as the research is being conducted that some of the design needs to be altered, and indeed – being brutally honest – how some of the cracks need to be plastered over in order for it to be published.

This book is directed at undergraduate and graduate students (though I hope academic colleagues can get something from it too), and both need to be aware of the strength of the evidence in the work they read. Doctoral students also have to get into the publishing business and should think about compromises they might need to make whilst keeping their work honest.

This book also differs from many others on the philosophy or theory of social sciences in that they are often structured around ‘isms’ – positivism, realism, constructivism, and so on. The common book plan is to look at each ism, define it, critique it, and then move on to the next one; until finally we get to the favoured ism, which is defined and defended rather than criticized. I believe this standard textbook technique (not only in this field, but in others) is pernicious, and responsible for the poverty of much social science. Dealing with issues in this manner is kind of *ad hominem*: the ism, rather than the arguments around the issues being discussed, becomes the target. I have (at least) four problems with this way of presenting material:

- Often those labelled as, say, ‘positivists’ or ‘relativists’ have greater disagreement with others within the same ism than with those labelled with a different ism. Rudolf Carnap, Carl Hempel, Ernest Nagel and Hans Reichenbach can all be justifiably termed positivists, but they had some very different ideas. Karl Popper, laughably, is labelled a positivist by some academics, although he – in his usual modest manner – saw himself as positivism’s greatest critic. (In fact, everyone can be described, with some justification, as a positivist; but the label is now usually applied to identify people with logical positivism, currently rather out of fashion.) Likewise, Paul Feyerabend and Michel Foucault can both be regarded as ‘relativists’, but their views on the conduct of research are far removed from each other.
- Partly for the above reason, I simply do not always recognize the labels as they are defined – what someone defines as positivism, or realism, or critical theory, say, just isn’t described in the manner I would describe it. My point here is not that the author has got the definition and account of the ism wrong, whereas I (of course!) have got it right. Sometimes I simply do not see how a particular writer can be allocated that particular label (given how the ism was defined); or how anyone in that ism can be thought of as having those beliefs. But my point is rather that the author has chosen to focus upon certain aspects of the thoughts of writers within the ism rather than others. They have chosen to chop up the world differently from the way I would slice it.
- I find it offensive when lesser writers (and great writers rarely write textbooks or books like this one, getting on, instead, with their more important work) dismiss some of the world’s greatest thinkers with rather puny critiques of the particular ism into which the lesser writer pigeonholes them. I sometimes read these critiques and ask myself, ‘Does this writer really

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think that Karl Popper or Willard van Orman Quine or ... was so stupid that they would be floored by *that* criticism?’

- Finally, given that isms are not ‘natural kinds’ or even coherent theories, the whole intellectual enterprise seems on shaky foundations from the start. One creates a way of cataloguing the world into isms, for each of which no person, living or dead, would assent to every aspect as defined, then proceeds to knock them down by demonstrating their logical incoherence or contradictions (in the best texts) or dismiss them with poorly thought-out critiques (in the worst). In this sense the entire enterprise seems to me to be ad hominem, a form of critique that is informally fallacious. You dismiss a writer or a claim by associating them with the ism you have criticized, without having to do the hard work of proper analysis and contextualization.

This bizarre way of behaving seems, boa-like, to have constricted the entire discipline of international relations (IR). A few years ago I sat on an interview panel for a post in an IR department. Each candidate at their presentation, and then again at the interview, labelled themselves in terms of an ism: ‘I am a neo-realist’; ‘I am a constructivist with some realist leanings’; ‘My work is informed by post-constructivism’, and so on. It was noticeable that each wanted to hedge their ismistic bets a little, lest they commit some faux pas and upset a member of the panel. IR seemed to be engaged in some battle where self-identifying colours must be nailed to the mast prior to any discussion of topic or research.

In fact, a strong swing away from this form of thinking is now under way in IR (Sil and Katzenstein 2010). What is doubly confusing for a person like me, who reads a bit of philosophy, is that in IR what is meant by ‘realism’ and ‘constructivism’ (the two main contenders, it seems) has, at best, a glancing connection with what those terms mean in the philosophy of (social) science. Indeed, in philosophy, as I explain in Chapter 2, in a strong sense many modern realists are constructivists. (Constructivism in analytic political philosophy, by the way, has only a tangential relationship to constructivism in science or in IR.)

So this book is not designed in that way. I am afraid, though, that I am unable to avoid mentioning isms, in part because I need to engage with the literature out there. Indeed, in Chapter 2 I discuss isms in much the way I have criticized others for, largely due to pressure from my students, who have suffered the rambling lectures on which this book is based and who have demanded I provide a roadmap to help them understand the course reading. But I do so uneasily. I try to map out the grounds of the isms as I see them. Whilst I demarcate specific views that I attach to those isms, I also point out how one can coherently hold various of these positions simultaneously (or at least aspects of these various positions simultaneously).

I am indeed an ismistic pluralist. Rather than dismissing any ism (or, to be honest, virtually any ism), I embrace them all, choosing those bits I like and discarding the rest. Some of my colleagues insist I am a positivist, though from that label what they then infer I believe is often mystifying to me; but for myself I am happy to be a positivist. I self-identify as a realist, a constructivist, a naturalist, an interpretivist and an objectivist; I have been greatly influenced by Karl Popper (though I am not a Popperian, finding too many problems with his method when it comes to ‘theory’ or what I call ‘model’ testing: see Chapter 5); I am weakly relativist in certain contexts; reductionist in explanation; foundationalist with regard to theoretical models, but holistic with regard to putting them together; an instrumentalist and pragmatist.

All of these positions are underwritten for me by Darwinian evolution, which must underpin the natural world and the way in which we perceive it. And, of course, if it underpins both the natural world and the way we see that world, it must underpin the social world, our science and our philosophy of science. So, along with strong or radical relativism, one of the few isms that I am not prepared to encompass within my set of beliefs is creationism – intelligent design (ID), as it has now been rebranded – and its ilk (and creationism is the only other game in town when it comes to our planet’s biological heritage). Any epistemology or ontology we adopt needs to take account of Darwinian evolution and its effects on our social world and the ways in which we form our beliefs.

I will explain in Chapter 2 in what sense I can identify with all these isms. I will also explain in other places why I am a methodological pluralist, believing in the worth of quantitative and qualitative analysis; survey- and interview-based evidence; interpretive shadowing; discourse analysis; good old-fashioned archival work; formal modelling and inductive data dredging; large-*n* analysis and individual case studies. I also believe in the value of straightforward description (Gerring 2012a). What I do not believe, however, is that each method is equally good at answering each and every question. Some questions can only be answered by certain methods, whilst other methods are more efficient at answering some questions.

Perhaps the most important lesson of all to learn in social scientific research design is the constraints upon what each method can explain. Indeed it might be that the best way of thinking about any particular method of study is less what questions it can be used to address than what questions it cannot be used to answer. In other words, what assumptions do we need to hold in order to reach the inferences we have made? What logical constraints are there upon those inferences and what are the problems with our assumptions?

Many techniques in the social sciences have been developed because people have interrogated extant techniques specifically with regard to their constraints and failings, and through that process developed new techniques. (A word here to the eager student about to embark on a PhD. If you insist on

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writing about the difficult and important questions you would like to answer, then you need to learn the techniques that can enable you to address those questions. If you insist on using only certain techniques, then be sure to ask questions that those methods can answer.) Mixed-method research is now fashionable, but when engaging upon it one must be sure what each method is adding to the analysis. That is, whilst mixed methods might enable one to get a handle on different aspects of the research problem, they will not necessarily all support each other with regard to the aspects each method addresses.

One of the most controversial aspects of my account is how social science should handle causation. Some scholars (Quine 1960, for example) hold that all explanation is reduction to causes. A great deal of modern empirical science is about how we capture causal processes – various statistical techniques such as structural equations; the experimental turn in political science; process tracing; the unification of the logic of political science explanation in King et al. (1994). Indeed King et al., despite writing sensibly about descriptive inference, seem to assume that all explanation is causal. In that regard I am completely heretical and think far too much is made of trying to demonstrate causation.

It is not that I think causation is unimportant, but I do not believe that demonstrating causation is the only useful thing to do, nor always the most important aspect of any science. The notion of ‘cause’ has almost completely disappeared from theoretical physics; has no role in the building-blocks of chemistry; it is not at all clear that we can represent ‘natural selection’ in evolutionary biology in any ordinary sense of ‘cause’ (Matthen and Ariew 2009); and an awful lot of good social science simply has not pinned down causation. I believe that noting structural conditions and identity relationships is still important. Furthermore, philosophers have struggled with the whole notion of causation for hundreds of years. Indeed, people’s views of it vary according to their education. Lawyers (and most philosophers) tend to look at causation in terms of ‘but for’ conditions (see Amsel et al. 1991), whereas scientists look at causation probabilistically (see Chapter 6).

This is not a minor issue. It has big implications for our understanding of the world, for explanation, for research design, and indeed for the conduct of law in the courtroom (where often what scientists would consider the best evidence available is deemed inadmissible). One of the big divides in political science, between quantitative and qualitative researchers, mirrors the divide over the nature of causation, with quantitative scholars utilizing a probabilistic account of causation, and qualitative ones a ‘but for’ account (see Goertz and Mahoney 2012). I discuss this issue in Chapter 6, making no claim to analyse causation in any original manner.

Physicists are not so interested in causation, engineers are; biologists not so much, medical practitioners and environmentalists are; chemists not,

pharmacists are. We become interested in causation when we want to intervene in the world. One way of demarcating the divide between political science and policy studies/public administration is by how far we are interested in causation in our analysis. For example, as political scientists we should be content with noting, say, the functional relationships between characteristics of polities, their environment and historical situation and, say, the rise and success of insurgency. Only if we want to intervene and help or hinder insurgency (in other words, become policy analysts), should we care about the precise causal effect of any aspect of those relationships and what will happen if we intervene to alter some of them. As far as I am concerned, narrative history is data (and a good yarn) and I do not think historians should worry too much about whether they correctly identified the ‘causes’ of any particular historical event, such as the First World War, the rise of Hitler or the weakness of contemporary US presidents. The fact that political scientists and historians do worry (too much) about these matters is a fault of methods courses that concentrate excessively on pinning down causation. If physicists, chemists and biologists do not agonize, why should we? Abandoning our fixation on causation might lead to greater precision and point in our models, as I discuss in Chapter 6.

I am not saying these causal questions should not be asked, but they should be framed in a more general sense. We cannot prevent the rise of Hitler, but we might be able to intervene to stop the rise of murderous dictators. As policy analysts, we should not be concerned, as such, with the cause of the rise of Hitler, but rather the cause of the rise of dictators of which Hitler is one data point. It is the general question that holds our interest because we are interested in the broader policy issue. What could have prevented Hitler’s rise might be a fun question, but I am not sure it is good history. In this example, good history is narrating Hitler’s rise.

Indeed, even if we can plausibly argue that there is a unique event without which Hitler would not have risen (a ‘but for’ condition), this is not much help to the broader policy issue. The broader policy issue concerns the conditions that facilitate the rise of dictators, not the chance (‘but for’) events that enable given ones to arise in specific cases. In that sense we are interested in the structural features around causation just as much as, if not more than, specific causes of unique events. This statement already assumes some aspects of the analysis of causation with which some might want to take issue. But it also reveals why I am not so concerned with pinning down causation as some of my colleagues.

My sense from discussion with historians is that they are uncomfortable with my suggestion that they should be content with the narrative rather than pinning down causes. They think that it somehow downgrades their subject and makes it non-explanatory. But they can only think like this if they feel that all explanation is reduction to causes, and they are required to

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give causal explanations; getting out of that discursive trap would be a real freedom, I think. However, my view of the role of causation in social science is subversive and whilst it will appear several times later in this book, I will endeavour to ensure that it does not overly colour my characterization of good political science.

This book is an introductory text designed for high-level undergraduates, who are increasingly subject to research methods courses, and for higher-degree research students. I have taught parts of it at the Australian National University and some time ago at the London School of Economics. Some of it is quite difficult to understand and appreciate. I have tried to make everything as simple and readable as possible, but I myself find some topics difficult to get on top of; and that is, I think, because they are inherently difficult to grapple with. Any student (indeed any person) who does not find the issues difficult to comprehend has probably missed the point. I tend to think there are quite a few social theorists around who do miss the point on a regular and almost systematic basis. One of the things I sometimes say to students (at both graduate and undergraduate level) who complain about the difficulty of the material on my various courses is, 'Of course it is: this is a university.'

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