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Explanation, Prediction, Causation – An Unholy Trinity? Appreciative Comments on *The Philosophy and Methods of Political Science*

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Abstract

In this short but critical appreciation of Keith Dowding's seminal *Philosophy and Methods in Political Science* I reflect on the distinctive treatment of both realism and explanation in contemporary political science that its author offers, expressing rather more sympathy for the former than the latter. I welcome his critique of the use and misuse of 'isms' in much of the existing literature, while pointing to some potential inconsistencies; I accept his broad and inclusive understanding of philosophic realism; and I praise Dowding for putting the question of explanation – and its adequacy – at the heart of the philosophy of political science (where I think it belongs). Yet I reject the idea that prediction is, or indeed, should be central to all social scientific explanation. Similarly, I take issue with the contention that we are typically distracted by questions of causation, suggesting that the presentation of a 'credibly causal' narrative is the crux of adjudicating good from bad explanation. I explore the implications of such a position and conclude with comments on Dowding's call for the reproducibility and transparency of data.

Keith Dowding's *The Philosophy and Methods of Political Science* is a most important book which is surely destined to become something of a classic of contemporary political analysis. Like the very best texts, it is important both as an intervention in the debate in its own right and as an accessible and penetrating guide through the complexities of that debate for students encountering them for the first time. Yet in one sense it is a difficult book to recommend to students – for it provides them with all the ammunition they need to ask irritatingly taxing questions of people like myself who try to teach them. But that, of course, is a very good thing.

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The text is closely, even forensically, argued and it warrants close reading – the kind of reading on which its own incisiveness is undoubtedly predicated. There is a great deal in it and, partly due to its structure and style of presentation it can, at times, appear a little aphoristic. It contains, I am absolutely convinced, a distinctive, coherent and internally consistent perspective (that of its author), but it is not always set out in such a way as to make that easy to discern. For it is written not through the stepwise unfolding and exposition of that perspective so much as through a series of specific interventions (no doubt informed by that perspective) in debates chosen for their pedagogic import. While, in a textbook, that is almost certainly the correct choice, it does make it all the more difficult to engage with as a holistic perspective in its own right.

There is, then, much to learn from Dowding's *The Philosophy and Methods of Political Science* and, given its extraordinary reach and range combined with the author's characteristically acerbic precision, something to frustrate and irritate almost everyone. Yet, strangely perhaps, and certainly given the positions we are typically assigned at opposite ends of the political analytic spectrum in debates of this kind, there is rather less to frustrate and irritate me than I was perhaps anticipating. In the end, it seems, while doing our political science rather differently, we agree on much more of the philosophy of political science than others tend to assume. For me at least, there is something rather reassuring about that.

The task assigned to me in this symposium is to reflect on realism and explanation in the light of Dowding's contribution to our understanding of each in *The Philosophy and Methods of Political Science*. To be honest, I am much happier reflecting on the latter than the former, not least since I think we should all be talking about explanation rather more – and, for what it's worth, realism rather less. I suspect Dowding would agree.

Indeed, I now tend to resist talking about realism at all having been told, variously, that I am not and never have been a realist, that I was a realist but have strayed, that when I was a realist I was not the right kind of realist and that I have committed any number of realist sins (the worst, apparently, being 'actualism'). Most of these comments came at a time in my career when, rightly or wrongly, I did think of what I was doing as realist, even if distinctively and quirkily so. But, suitably chastened, I have long since desisted from using realism are a general badge of self-identification. Dowding's book makes me realise that that was perhaps premature.

Part of the reason for this is that, quite simply, I agree with much of what Dowding has to say about realism, his interesting caveats about 'isms' notwithstanding. And I find that the position I myself hold falls fairly squarely within the (broad) realm of positions he describes as realist.

On 'Isms', Like Realism

There is a tendency in debates of the kind assembled in this symposium, in which the merits of a particularly significant intervention are debated, to register and signal only one's points of disagreement. Here, I want to depart from that tradition. For I think the points of agreement are at least as important.

I will start with Dowding's fascinating discussion not of realism per se but of 'isms' more generally. Here, I agree with the majority of what he has to say, with just a couple of minor caveats. 'Isms' are, indeed, dangerous things, no more so than in textbooks. They do, indeed (or can, at least), encourage sloppy thinking and they are – or invariably lead to – overly stylised generalisations. They are, in short, dangerous and distorting simplifications.

Yet, accepting all of these points, there is a certain inconsistency here. This manifests itself as a strange kind of performative contradiction, both in the chapter on 'isms' itself and, perhaps more significantly, in those which follow. Indeed, there are possibly two problems here. First, overly stylised generalisations are the very stuff of modern political science, whether that science proceeds inductively or deductively. We work with them all the time. Dowding, himself hardly averse to a simplifying generalisation in the right place, is well aware of this. The point is that the need for them and the use we make of them (with, of course, some cautionary qualifications) is something which is otherwise defended by Dowding in this book. Accordingly, it seems a little unbalanced to launch such a coruscating broadside against 'isms' as organising devices in academic exchanges of this kind when other simplifying generalisations (like, say, the assumption of self-interest or of instrumental rationality) are not assailed in similar fashion. Some 'useful short-hands' it seems are tolerated, while others are not. Predictably, and perhaps even appropriately, in a text on the philosophy of political science, it is philosophical shortcuts which attract Dowding's ire while their political analytic equivalents (the assumption of self-interest and so forth) are given a far easier ride. But the inconsistency remains – and it jars, just a little.

Second, and more significantly, there is in fact a strange kind of capitulation to 'isms' and 'ismic' thinking that characterises the text. Thus, having whetted the reader's appetite for a 'post-ismic' reflection on these issues (and in the first substantive chapter of the book, no less), what follows is a (presumably) reluctant and (of course) unusually sophisticated and sensitive reworking of a quite familiar and conventional collection of relevant 'isms' as a prelude to an account couched largely in their terms. What we miss is any sense of what a 'post-ismic' (the term is Dowding's) treatment of the very same issues would look like, nor even a sustained reflection on whether it is conceivable. If, as one might well surmise from Dowding's reluctant return to 'isms', the reason for this is that it is simply impossible to conduct the philosophy of political science without reference to constructions of an 'ismic' type, then at minimum I think we deserve a fuller account of the limits of 'post-ismic' reflection and their implications. In the absence of this, I remain enticed by the prospect of a post-ismic philosophy of the social sciences and just a little disappointed that the intriguing suggestion that such a thing might be possible (and the hunch that it might be desirable) is left largely unexplored.

That said, Dowding's heavily 'ismic' philosophy of political science is very good – not least on the subject of realism (in and through the reflection on which much of it is couched). His argument, in short, is that a lot more of us are realists than most self-professed realists of the tub-thumping variety (we can all think of a few) would be prepared to concede. If realism is the view that there is a world which exists independently of us or, as I would prefer, the view that there is a world which, at any given moment in time, exists independently of us, then we are all realists – or almost all of us. Some of us, of course, would want to emphasise that the subjects of social scientific analysis are so intimately embedded within this world that our independence from it dissolves the moment our reflections extend over any period of time – in that we shape through even our most trivial and least conscious of acts that world in ways which, as social scientists, should interest us (Hay, 2016). But the point is that, for Dowding, this is an internal debate among realists. And I suspect the argument itself is no longer especially controversial; although, interestingly, it is the effective point of departure of Peter Berger and Thomas Luckmann's (1966) and John Searle's (1995, 2010) constructivisms. But this only serves to reinforce Dowding's point – realism is rather more universal and rather less discriminating a label than its most vociferous prophets tend to presume.

I will leave my reflections on realism there in order to turn to what I regard to be more important questions – questions which, in a sense, are more clearly and obviously post-ismic.

Explanation, Prediction, Causation – An Unholy Trinity?

These relate to explanation and causation – and to the link which Dowding seeks to establish between these two terms and that of prediction. Again, I will try to keep my reflections brief. But a number of points might here be made.

First, this is precisely the right set of issues. Political analysts do not, in general, reflect nearly enough on what they mean by explanation – and, partly as a consequence, we tend not to encourage our students to do so either. That is, quite simply, not good enough, and Dowding shows us why.

Once again, then, I want to start by underscoring a note of agreement. I, too, would now want to place the question of explanation (what makes for an explanation and what makes for a *good* explanation) at the heart of the philosophy of political analysis. Dowding's book reinforces my conviction that that is right. It has struck me for a long time that a great number of (unresolved) disputes in political science actually boil down to the largely unacknowledged and unasked question of what might be seen to constitute an adequate explanation of a given set of outcomes. We need to acknowledge that we can have, do have and are right to have different views on that question and to begin to reflect on why that might be so. This book helps.

But the issue of prediction, which Dowding wants to place at centre stage in such debates, I think gets in the way here (or perhaps it is just indicative of one of the key rifts in political analysis around this question). The idea that all explanation is ultimately about the fashioning of predictions I find strangely misplaced. That said, I can certainly see where it comes from and also how it comes, in effect, to save as 'explanatory' a group of theories (and the formal models which are their typical stock-in-trade) which (despite their utility) I would not regard as explanatory per se. Why do I not regard them as explanatory? Because they are not *credibly causal* even in their own terms. They are slightly more credibly predictive, but only slightly (a point to which we return presently).

But even before we get to that I think it is first important to acknowledge, and then to deal with, the suggestion that not all explanation is causal. This, too, I find problematic. The claim is made a number of times (see, for instance, Dowding, 2016: 6–7, 42, 57–60, 133). But it is never really defended in any detail. Dowding suggests that we might differentiate (inter alia) between causal, functional and interpretive explanation. Yet I remain unconvinced. For me, interpretive explanation, where it is genuinely explanatory, is causal and makes a causal claim (however unconventional that claim might seem). Similarly, functional explanation – which, for me is nearly always bogus in the social sciences – in the end rests on a causal claim, too, if it is to be regarded as genuinely explanatory. Whether it should be regarded as explanatory is another matter, but here, again, I would suggest the question of credible causality returns. A putative functional claim is, or might be deemed, explanatory to the extent to which it offers (or is deemed to offer) an account which is credibly causal.

In stark contrast, for Dowding (2016: 6), the demonstration of causality is in fact something of a sideshow, with far too much time spent (presumably, wasted) by political analysts on this unnecessary endeavour. Reading between the lines, any theory or model is deemed by him explanatory if it generates or is capable of generating an

expectation – a hypothesis, in effect. Explanation, in this sense, is about aligning the expectation (the hypothesis) with the evidence (whether deterministically or, more frequently, probabilistically).

On the face of it, this might seem fine. But I think we need to be very careful here. For it is perfectly possible, I would contend, to be predictive in this sense (i.e. to generate an expectation) without explaining anything, even if the 'prediction' (typically, of course, in political science a retrospective one) is born out by the evidence (or, at least, seen to be so).

Dowding's more general point – and here I do agree – is that we are interested in explaining things (social and political facts) in and through our stake (instrumental or otherwise) in them. In other words, we explain in order to give ourselves a more informed perspective on how we might intervene to produce better outcomes in the future (our interest is prospective). That is fine. But one can be prospectively oriented in this way without claiming for oneself the power to be predictive. To do so is not so much to be predictive as it is to acknowledge the limits on our capacity to predict and to act in due recognition of those limits. The notion of prediction (which might be neutral in the natural sciences) here generates the wrong – indeed, an illegitimate – expectation.

More significantly still, Dowding's notion of explanation does not require the capacity to furnish a 'credible' causal story. And this is my real problem. For I would argue that, in order to be seen as genuinely explanatory, an account needs to be credibly causal. Constant conjunction in the Humean sense, then, is inadequate on this understanding unless and until the constant conjunction is linked to a causal story or narrative which is genuinely offered (in the sense that the causal sequence described could genuinely have produced the outcome and the author genuinely believes that it did). The question thus becomes: 'What kind(s) of putatively explanatory stories are credibly causal?' And this, I think, is where our two approaches really do part company.

Where we differ, above all and as already hinted at, is on formal models as causal – and, indeed, on formal models as explanatory and perhaps, even, on formal models as predictive. Put bluntly, I do not see formal models as causal and because of this I do not see them as explanatory. They fail, in other words, to be explanatory precisely because they do not pass the 'credible causation' test – and, because of that, they are not a valid basis for prediction. Why? Because, quite simply, their authors (the model-builders) typically do not believe the assumptions on which the models they build are predicated – with some even going so far as to defend the implausibility of their assumptions as a condition of the interest and utility of their model world (most famously, Friedman, 1953). For me, if the assumptions are demonstrably false or self-confessedly unrealistic then the model cannot credibly explain the outcome. As such, the outcome is not genuinely predicted nor, as a consequence, is the account to which it gives rise genuinely causal or genuinely explanatory. The model-builder here is a little like the oracle who believes that she has lost her powers of divination and no longer has privileged access to the thoughts of the relevant deities but carries on making her prophecies anyway. The trouble is that others carry on heeding her advice as if it were genuinely offered.

The capacity to build a model on the basis of formal stylised assumptions (about human conduct, for instance), which is capable of generating an expectation borne out by the facts (whether prospectively or, more likely retrospectively) is no mean feat. That achievement should certainly not be sniffed at – and the model itself is likely to have some considerable analytic and heuristic value (see also Hay, 2004). But – and it is a *big* but – such a model cannot and should not be regarded as explanatory if the assumptions

on which it is built are acknowledged to be false (or even just implausible). For if those assumptions are wrong (or, indeed, just extremely fallible) and acknowledged as such, the model-builder cannot genuinely claim that the process or mechanism which the model describes could credibly exist such that it might generate the anticipated outcome. Whatever else the achievement, it is not explanatory because the model to which it gives rise is not credibly causal.

The irony is that it is precisely because we seek to explain in order to improve the social and political conditions which pertain in the world in which we live (as Dowding would have it) that we cannot be so generous and undiscriminating in our views of explanatory adequacy. To explain an outcome, quite simply, has to be to tell a story which could credibly have produced the outcome. That, of course, is the one type of story which a formal model cannot offer. And, for me, that is sufficient to establish that such models, whatever their analytic value, are not explanatory.

Reproducibility and Transparency

In lieu of a more conventional conclusion, I wish to use the opportunity of this symposium to draw attention to the short passage in the book with which I have perhaps the greatest difficulty. It is completely unrelated to the previous discussion – and, indeed, Dowding's position on it is not I think a logical correlate of his broader philosophy of political science.

This relates to the question of research transparency and reproducibility (Dowding, 2016: 166–169). Here, Dowding echoes the recent calls in certain parts of the discipline for all of the data and coding frames used in empirical research to be made fully transparent and available (through open access) to the wider research community and the public more generally.

This, I think, we should reject. The argument is simple and is perhaps best put in personal terms. When I read one of Keith Dowding's empirical papers, I want to read *his* analysis and *his* interpretation of the data he collects and interrogates. I am not terribly interested in having full access to his data, his coding book and the means he uses to make the inferences he draws from that data. What I want to read is *his interpretation* and, above all, *the argument he makes* on the basis of that interpretation. It is that argument which I want to understand and to engage with. I am happy to trust him, as much as I trust anyone, to collect and process the data appropriately and for peer review to take care of that part of things. In short, I want to read *him*, not what someone trying to behave like him might write and think were they to try to replicate his research using his data and his code book.

Transparency and reproducibility sound like unimpeachable virtues, but they are no substitute for insight; their fetishisation downplays the role of interpretation in the generation of that insight, and the manifesto to which they have given rise is based on unsubstantiated fears of duplicity and mistrust. We need to be very careful what we wish for. As I am sure Keith Dowding would agree, political science cannot be conducted by robots; I wish that some of our colleagues would stop trying to pretend that it can or that things would be better if it were.

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References

Berger PL and Luckmann T (1966) *The Social Construction of Reality*. London: Penguin Books.

Dowding K (2016) *The Philosophy and Methods of Political Science*. Basingstoke: Palgrave Macmillan.

Friedman M (1953) The Methodology of Positive Economics. In: Friedman M (ed.) *Essays in Positive Economics*. Chicago, IL: University of Chicago Press.

Hay C (2004) Theory, Stylized Heuristic or Self-Fulfilling Prophecy? The Status of Rational Choice Theory in Public Administration. *Public Administration* 82 (1): 39–62.

Hay C (2016) Good in a Crisis: The Ontological Institutionalism of Social Constructivism. *New Political Economy* 21 (6): 520–535.

Searle JR (1995) *The Construction of Social Reality*. London: Penguin Books.

Searle JR (2010) *Making the Social World: The Structure of Human Civilization*. Oxford: Oxford University Press.

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