POLITICAL STUDIES: 2001 VOL 49, 528–541

Debates:

There is More than One Way to Do Political Science: on Different Ways to Study Policy Networks

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Our recent article in this journal has provoked a series of responses to which we reply here. However, these responses are very different in tone and content and this is reflected in the balance of this reply. Dowding attacks our work in the context of a claim that, essentially, there is only one way to do social science. This critique is so fundamental that it is the focus of the first section of this reply. In contrast, Raab is mainly concerned to argue that he and McPherson cannot be classified as taking an anthropological approach to networks; indeed, he claims that their work adopts a position which has similarities with our own. Finally, Evans attempts to build upon our article, using the work of Benson, to develop what he regards as a more adequate dialectical approach. We shall deal with both of these contributions in the second section of this reply, in which we consider Dowding's more specific criticisms of our work.

Only One Way to Do Political Science?

In effect Dowding argues that there is only one way to do political science.[1] In this section we take issue with this contention. We shall argue first that Dowding is a positivist, second that there are other ways to undertake 'political science' generally and the study of networks specifically and third that epistemological issues are crucial because they shape what one studies, how one studies it and what conclusions one draws from research.

(i) Dowding's Positivism

Dowding does not discuss ontological or epistemological questions, but he adopts an empiricist, indeed positivist, position. Of course, such an assertion depends to an extent on our understanding of positivism, particularly because it is a position that has changed significantly over the recent past[2] and because Dowding may deny such an attribution.[3] As such, we need to start with a brief discussion of positivism in relation to other positions.

Published by Blackwell Publishers, 108 Cowley Road, Oxford OX4 1JF, UK and 350 Main Street, Malden, MA 02148, USA

In ontological terms, positivism is clearly foundationalist; it argues that there is a 'real' world 'out there' that is independent of agent's knowledge of it. In contrast, anti-foundationalists see the world as socially constructed.[4] Positivists also believe that it is possible, using the proper research methods, for an observer to discover these 'real' relationships between social phenomena. This brings us to the arena of epistemology.

There are a number of ways of classifying epistemological positions and there is no agreement as to the best way. Probably the most common classification, distinguishes between scientific (sometimes positivist) and hermeneutic (or interpretive) positions or traditions.[5] Researchers in the scientific tradition focus upon identifying the *causes* of social behaviour. The emphasis is upon *explanation* and in the past many felt that the use of rigorous 'scientific' methods would allow social scientists to develop laws, similar in status to scientific laws, which would hold across time and space. In contrast, the adherents of the hermeneutic or interpretive tradition focus upon the *meaning* of behaviour. The emphasis is upon *understanding*, rather than *explanation*.[6] As such, for those within the interpretist tradition it is not possible to establish causal relationships between phenomena that hold across time and space.

The chief problem with this classification is that it tends to conflate positivist and realist epistemological positions; seeing them both as within the scientific tradition.[7] Realists, like positivists, are foundationalist in ontological terms. However, unlike positivists, they do not privilege direct observation and they accept much of the interpretive critique of positivism. In particular, the realist believes that there are deep structural relationships between social phenomena which cannot be directly observed but which are crucial for any explanation of behaviour. So, as an example, a realist might argue that, although patriarchy as a structure cannot be directly observed, we can see many of the consequences of it and any full explanation of outcomes must take account of this unobservable structure.

It seems to us that there is no doubt that Dowding is a positivist. So, he argues: 'we (*that is all good political scientists*) (aim to) produce models with definite predictions … which we can then test one way or another against data gathered from the actual world' (2001, p. 92, our comment in italics). In Dowding's view political science research should proceed in a fairly uniform way; researchers need to specify assumptions, generate hypotheses/predictions and test them (2001, pp. 92–3). As such, the aim of political scientists is to establish causal relationships (2001, p. 91) and determine which are the most important independent variables (2001, pp. 91–2). This points towards the use of formal models (2000, p. 91) and quantitative data. In fact, Dowding asserts: 'qualitative work generally has low leverage' (2001, p. 95).[8] It would be hard to find a clearer exposition of the positivist position.

However, in an earlier work, Dowding (1991, p. 14) claims to be a realist and argues (p. 16) that: 'this framework is compatible with a form of behaviouralism.' As such, he sees no inconsistency between a realist epistemological position and a behaviouralist methodology. Unfortunately, Dowding's characterization of realism is more interesting for what is omitted than for what is included. Dowding is certainly right that a realist account of power would focus on the enduring resources

controlled by some individuals/groups. Nevertheless, he offers a rational choice interpretation of realism; the focus is upon agents and, in particular, an agent's ability to use incentives to manipulate other agent's preferences. In contrast, we, like most realists, would adopt a different approach in three important ways.[9] First, we would emphasize the importance of structure. As such, in our view an enduring structure, whether political, social or economic, will be reflected in the pattern of resource distribution; thus reversing the causal direction asserted by Dowding. Second, as we already emphasized, realism is based on the idea that there are deep structures which cannot be directly observed; consequently, we cannot simply study either what people say or what they do because there is often a division between appearance and reality. As such, an approach that focuses on appearances, like behaviouralism, may produce misleading results. Third, and in line with modern critical realism, we acknowledge that the world is, to an extent, socially constructed. As such, the way institutions or processes are socially constructed affects outcomes, but the extent and resonance of that social construction is constrained by deeper social relations such as patriarchy and by capitalist economic relations. We shall return to this issue below

Of course, Dowding could, and would, reject those aspects of realism, but, in our view, he cannot reject them as aspects of realism. In rejecting them, he would again reveal his positivist leanings. It is not that realists reject behaviouralism or the study of behaviour, rather they would argue that some crucial structural effects can't be directly observed and such evidence does not speak for itself, it has to be interpreted. They would argue that such interpretation would most often involve going beyond appearances. As such, Dowding's argument that there are no tensions between epistemological realism and behaviouralism is based upon a misrepresentation of realism. More crucially for us here, he is clearly a positivist.

(ii) The Ontological, Epistemological and Methodological Plurality of Political Science

Of course, many, perhaps most, political scientists are positivists,[10] although it is a highly contested position.[11] However, not all political scientists are empiricists, let alone positivists, and many of the non-positivists are among the best in the profession (think of Peter Hall, Theda Skocpol and Paul Pierson in the US or Rod Rhodes, Andrew Gamble and David Coates in the UK). The key point however is that the ontological and epistemological arguments that underpin the way different people do political science cannot be resolved by Dowding's assertions. While Dowding thinks he is right, many people would disagree, including a significant number who think there is no such thing as a right answer. In effect, Dowding wants to set the rules, referee the game and send off anyone he regards as ineligible.

As far as the policy network literature is concerned, this epistemological plurality is reflected in the work of different authors. So, while Dowding is a positivist, we are realists and Rhodes, in his most recent work, particularly that with Bevir, is firmly in the interpretive tradition (see Bevir and Rhodes, forthcoming, especially Chapter 4). The point is that, because these authors start from different epistemological positions, they look at different evidence, use different methods and, to an extent, draw different conclusions.

(iii) Why Are Such Distinctions Important?

In our view, ontological and epistemological concerns cannot, and should not, be ignored or downgraded. Two points are important here. First, these concerns should not be put in what the Australians, with typical directness, call the 'too hard basket'. Certainly, the issues involved are not easy, but neither are they difficult, if they are explained simply and with appropriate examples. Second, ontological and epistemological positions shouldn't be treated like a pullover that can be 'put on' when we are addressing such philosophical issues and 'taken off' when we are doing research. In our view, the dominance of a fairly crude positivist epistemology throughout much of the post-war period encouraged many social scientists to dismiss ontological questions and regard epistemological issues as more or less resolved, with only the details left to be decided by those interested in such matters. Such social scientists have tended to acknowledge the importance of epistemology without considering it necessary to deal with it in detail; positivism has been regarded as a comforting pullover that can be put on where necessary. In contrast, we would argue that epistemology, to say nothing of ontology, is far from being a closed debate.

This is the key point. A researcher's epistemological position is reflected in what is studied, how it is studied and the status the researcher gives to his/her findings. So, a positivist looks for causal relationships, tends to prefer quantitative analysis[12] and wants to produce 'objective' and generalizable findings. Indeed, as a consequence, much of the research is data driven rather than driven by interesting or important questions. A researcher from within the interpretive tradition, focuses on the meaning that actions have for agents, tends to use qualitative evidence and offers his/her results as one interpretation of the relationship between the social phenomena studied. Realism is less easy to classify in this way. The realists are looking for causal relationships, but think that many important relationships between social phenomena cannot be observed. This means they may use quantitative and qualitative data. The quantitative data will only be appropriate for those relationships that are directly observable. In contrast, the unobservable relationships can only be established indirectly; that is they are inferred from the researcher's theory and other observable relationships.

These points are easily illustrated in the policy network literature. In particular, it is clear that Dowding, ourselves and Bevir and Rhodes operate with different notions of theory which reflects our different epistemological positions. To Dowding, the role of theory is to generate testable hypotheses that are capable of being falsified. In contrast, Bevir and Rhodes (forthcoming, Chapter 2) talk of narratives, not theories. They reject any absolute truth claims. As such, a narrative offers one interpretation of events and social relationships. Bevir and Rhodes do believe we can make knowledge claims about the quality of any given narrative, but such claims are not absolute. Rather, they suggest that, at any given time, there is a dominant discourse about how to do social science and that, as such, the quality of any research can be assessed as against the criteria established in that dominant paradigm. To realists like ourselves, theory is what we use to establish which social relationships are observable and to interpret the results that we find. We, like Bevir and Rhodes, recognize that, as investigators who are not independent of the social world, our theories contain implicit and explicit normative assumptions. Our aim is to develop analytical frameworks that help us to interpret the complex world.

As such, theory and method are operating in a very different way for positivists, like Dowding, and realists, like ourselves. For positivists, there is an independent world that is governed by laws. The role of the social scientist is to identify those laws (and use them to predict outcomes) through the creation of theories that are able to generate testable hypotheses. For realists social structures are not independent of agents and may only be relatively socially enduring (see Hay, 1995, p. 192). The role of theory in realism is to contextualize observable behaviour by using theory to infer the underlying structures of a particular social situation (on this see Smith in Hollis and Smith, 1991, p. 207); theory provides a way of constructing a narrative that helps us identify and explain the underlying structural relationships. Indeed, it is impossible to make any sense of the world without some sort of theoretical framework.

We can illustrate this point by reference to our original case-study. The mainstream explanation of the influence of the farmers in the UK in the period between the 1930s and the 1980s focuses on their lobbying power, which is easily observable. In contrast, we suggest that a full explanation needs to start from an acknowledgement of the farmers' involvement in a tight agricultural policy network, an involvement that is less easy to observe or measure. In our view, the shared commitment to a policy of high subsidy and high production in this network led to a policy continuity which consistently favoured farmers' interests.[13]

Our model does not, and cannot, provide a definitive explanation in Dowding's terms. Rather, we present an interpretation based on empirical observation and theoretical inference. However, empirical evidence can lead us to question this interpretation because the model suggests testable propositions; in particular that networks affect outcomes. So, for example, empirical evidence might show that policy outcomes can be explained purely in terms of easily observed interest group activity. Alternatively, we may find that although the network is as we describe, the policy outcome is not as we would expect. Or, we may discover that the network is not as expected, for example the members might have limited shared values, but the outcome is as expected. As such, we do not think that our model is testable in Dowding's terms, because we do not accept Dowding's notion of social science and the role of theory within it. However, this does not invalidate the model as a means of ordering empirical material; nor does it make it untestable. As we argued above, the crucial point is that we cannot judge the quality of all social science work in terms of a single, positivist, set of criteria.

Overall then, in our view, Dowding is clearly a positivist, not a realist. Positivism has a long history and may still be the dominant position within social science. However, it has been increasingly challenged and many prominent political scientists operate within different traditions. We do not want to emulate Dowding and assert that our, realist, approach is the only way to do social science. Rather, we are arguing that we need to acknowledge the pluralism of political science.

To Policy Networks Once Again

Dowding and Raab both make criticisms of our work. In this section we shall focus on explaining our position in response to Dowding's more extensive critique, while dealing directly with Raab's narrower point.

We begin by addressing Dowding's view that those who study networks should use formal models to generate hypotheses about the relationship between the network and policy outcomes. We argue, contra-Dowding, that the absence of formal models within policy network work does not simply result from the researchers' lack of knowledge about how to undertake proper political science. Subsequently, we shall argue that: metaphors are useful, although the policy network approach has always attempted to go beyond metaphor; the utility of our own model is not dependent on our classification of the existing literature, although we think that classification is broadly fair; in our terms, Dowding's approach does privilege agency over structure; and whether the dialectical approach offers a better understanding of policy making than previous approaches is a question which must await future research.

(i) A Formal Model?

Dowding is clearly right that authors who have studied policy networks have not developed a formal model. Why have they not done so? If we take Dowding's position, they have not done so because they do not know how to do social science properly; presumably because they didn't take the right ESRC methods courses. However, there are epistemological and methodological reasons for such a position. Here, we shall focus on three points: (i) most broadly, formal models are less appropriate in social science because social reality is complex and involves reflexive agents; (ii) most formal rational choice models are partial because they assume preferences and a decision making scheme and, as such, tend to ignore crucial questions about the origins of both; and (iii) in social science the data is rarely available to test formal models.

a. An epistemological critique. Even in science, formal modeling works best when one is dealing with a limited number of variables in linear relationships.[14] For example, if a cannon ball is dropped from 100m, a physicist could predict where it would land and how long it would take to get there. In contrast, if a tissue is dropped from 100m on a windy day, the most sophisticated modeling and computer technology cannot model the flight path of the tissue. Yet, most social situations are much more complex than even the most complicated ones in the physical world precisely because interpretative agents are involved in the process. As such, formal models are very likely to oversimplify the social relationships involved as they strive for a model that achieves a balance between parsimony and explanatory power; that is one in which as much as possible of the variance in the dependent variable is explained using as few independent variables as possible.[15]

Of course, even if a positivist researcher achieved this balance between parsimony and explanatory power, his/her work would still be open to criticism from researchers operating from different epistemological positions. First, researchers operating in the interpretive tradition would deny the utility of such models, because they assume that the observer, the social scientist, is objective. Second, a realist would question the utility of formal models that focus exclusively on observable behaviour.

b. Rational choice models: assuming preferences and the decision-making framework. Actually, we do not reject the use of formal models, or the utility of rational choice

theory. Indeed, we agree with Dowding that, in order to explain how networks affect outcomes, we need a theory of action, knowledge of the preferences of the agents involved in the network and knowledge of the decision making structure with which they are faced. However, even if we know all these things, and they prove good predictors[16] of outcomes (and that is very doubtful, see next sub-section), we have not necessarily explained the outcome. In our view, we also need to know why the individuals have those preferences and the origins of the specific decision making scheme.

Of course, Dowding would claim that, if his analysis takes preferences and the decision making framework as given, it is only for convenience. In order to explain outcomes we only need to know the preferences and the decision making scheme; although, of course, he would also argue that we could explain the origins of both using rational choice models in a separate analysis. Nevertheless, it remains true that rational choice theorist do tend to assume preferences and the decision making scheme.

So, a realist might argue from a different epistemological position that these preferences and the decision making scheme are, in part a reflection of deep structural inequalities, for example based on gender, class or race, that we cannot directly observe. As such, we can't study those deep structures directly using behavioural methods. Consequently, a formal behavioural theory would be partial in both senses of the word. In this vein, we would argue that, as regards policy networks, we need to know why some groups are inside particular networks and some outside and why some policy options are excluded from the policy agenda and were, as such, not part of the decision making scheme. Moreover, preferences for us are not independent of the network. They are in part shaped by interaction within the network.

c. The availability of data. Even if, as Dowding suggests, one develops formal models they may be impossible to test. Dowding argues (2001, p. 90) that too much social science research is done cheaply. This again indicates Dowding's positivist and empiricist approach. However, it also appears to be a response to our earlier claim that it would be difficult, if not impossible, to collect the data necessary to test a formal model of the sort suggested by Dowding. Obviously, he believes that this reflects a failure by researchers (who are not adequately trained) or by funders (who fail to sufficiently fund 'proper research'). In contrast, our view is that the problem is less one of cost and more one of access and the data existing for quantitative analysis in most areas. Much of the data one would need to operationalize the formal models would involve commercial or bureaucratic confidentiality and therefore is unattainable.

(ii) On Metaphor

In Dowding's view, both the Marsh and Smith model and what he terms the Rhodes model[17] offer: 'just a metaphor and will remain so until we unpack the characteristics (of the networks), formalizing their relationships in equations or at least examining them in more systematic and quantitative ways in order to discover the underlying causal mechanisms' (p. 11).

The first point to make here is that metaphor[18] and analogy are widely, and usefully, used in social science. Evans (2001) makes that point very well. We accept that the idea that policy is made in networks invokes a metaphor, but we would suggest that it was never solely metaphorical. Richardson and Jordan, who are widely credited with coining the term, certainly in the British context, and whom Dowding refers to positively, argued that much policy is made by small groups of actors who are, more or less closely, linked; as such, the idea of linkage was invoked by using the term 'network' as a metaphor. Even at this stage however, Richardson and Jordan went beyond this metaphorical usage, because they claimed that such networks had a real existence and, indeed, that much, or even most, policy was made in these networks. Of course, we agree with Dowding that most subsequent authors who have invoked the network metaphor have wished to go further, arguing that networks affect policy outcomes. However, very few authors, and certainly not Rhodes or us, would want to go any further than Dowding did in what was presumably an unguarded moment in his 1991 book on power:

> Describing some policy communities as 'open' and others as 'closed' tells us about the nature of group and state actors. These different networks explain why one community tends to produce one sort of outcome whilst another produces another sort. (p. 123)

(iii) Classifying Existing Approaches

Both Dowding and Raab are critical of our attempt to classify the existing literature on networks. Both claim that their work does not stress agency, as against structure, in their explanations. We do not want to engage extensively with Raab on this point, partly because after re-reading his work with Macpherson we would want to revise our position. In our view, they can be classified as adopting an anthropological approach and stressing agency and personal networks; indeed, Raab acknowledges as much in his response. However, in so far as we implied that they neglect structure, then that was wrong. Nevertheless, this acknowledgement does little, if any, damage to our argument, because our classification of the literature was only a starting point to our analysis.[19] The crucial question to ask is: is our model useful in helping us understand policy outcomes in agricultural policy, and beyond? We return to that issue below.

(iv) Structure and Agency

Both Raab and Dowding take issue with our classification of their work as privileging agency over structural explanation. In particular, Dowding contends that we cannot have read his work, or we wouldn't make such assertions. So, let us look more closely at Dowding's position.

The first thing to say is that Dowding seems to have read very little of the structure/agency literature. In particular, while he cites Giddens, he hardly discusses him, and he cites none of the critiques of Giddens which have led to the development of dialectical (for a review see Hay, 1995) and morphogenic (for a review see McAnulla, 2001) approaches to the problem, although, to be fair, much of this material has appeared since the publication of Dowding's major book in 1991.

It is certainly clear that Dowding focuses on the behaviour of agents; so, he opens his major book on power by asserting (1991, p. 1): 'The subjects of this book are actors, and that means that the subject herself chooses the way in which to behave and is not merely caused so to behave.' More importantly, in Dowding's view, only agents can exercise power, structures cannot. We do not disagree with this view and nothing in our work on networks suggests that policy networks, as structures, exercise power.[20] What we would argue is that networks, as structures, constrain and facilitate agents.

Actually, Dowding appears to have little trouble with that argument. He claims:

> all agent-centred models assume agents act under constraints and the constraints are structures. I introduce the phrase 'structural suggestion' to try to capture the fact that that structures as constraints, and as enablers … suggest courses of action to free agents, though they do not determine them (2001, p. 14; see also 1991, p. 7 and p. 16).

So, it appears that there is little disagreement between us. However, what does Dowding mean when he talks about structures. His view seems to have two key elements. First, he argues that the structure of the rational choice model specified is crucial: 'Agents in formal models are just sets of preferences and the model predicts behaviour given its structure and these preferences'. Second, he contends that it is: '(the) structure of the individual choice situations that does most of the explanatory work, it is the set of incentives facing individuals which structurally suggest behaviour to them' (1991, p. 18). So, what we need to know is the structure of the model, the structure of the actor's preferences and the structure of their individual choice situation. We can then predict and explain action.

So, to Dowding, 'structural suggestion' is totally reflected in the cost/benefit calculations of agents and can be measured through revealed preference analysis: 'The degree of (structural) "suggestion" may be captured, perhaps in cost benefit calculations about our own and others' behaviour and measured through revealed preference analysis of general human behaviour (that is aggregate statistical analysis)' (p. 97).[21]

Of course, this is a very different notion of structure than that adopted by many, if not most, social scientists. It is certainly a different notion of structure than a realist would adopt. When most realists talk of structure they are talking of social (e.g. gender relations), economic (e.g. capitalist economic relations) or political relations (e.g. the relations between government and civil society or, more narrowly, policy networks). Such structures it is argued constrain and enable agents; they are a crucial aspect of the context within which agents act. So, we would argue that the structured inequalities within society, which of course may not be directly observable, are reflected in the access certain groups have to government through policy networks. Politics does not occur on a level playing field and any explanation of policy outcomes must acknowledge and explain the structural inequalities that shape the political arena and the political agenda.

We cannot finish this point without considering Dowding's most famous concept, 'systematic luck'. Indeed, even a brief consideration of this concept indicates clearly where we part company from Dowding. To Dowding, power involves achieving ends, despite resistance: 'The social power of one actor A over another actor B is

the ability of A deliberately to alter the incentive structures of B in order to get B to do something that she would not otherwise do' (1991, p. 68). If an individual's preferences are forwarded without them having to take action to alter another individual's incentive structures, then that is luck; here Dowding is following and developing Barry's usage.

At the same time, Dowding acknowledges (1991, pp. 137–8) that some individuals groups are consistently lucky:

> Whilst capitalists may be lucky, this luck may be systematic. If it is, it is still not power, but it is more than *mere* luck. Their luck is systematic because it attaches to certain locations within the institutional and social structure … this systematic luck is still luck … for individuals get what they want without trying. Systematic luck is not the same as power for, though it attaches to individuals in certain positions in society and it attaches to them *because* they hold those positions in society, they individually have no control over those outcomes.

We find nothing to take issue with in that quotation, other than the nomenclature. We recognize that this use of luck has fairly common currency in the rational choice literature, but it still strikes us as unfortunate. The *Oxford English Dictionary* defines luck as: 'success, prosperity or advantage coming by chance rather than as a consequence of merit or effort'. This definition certainly captures Dowding contention that luck doesn't involve effort. However, it also indicates that in normal usage luck involves chance. Yet, systematic or consistent luck, hardly occurs by chance. If it did the poor and powerless may get some of this luck. Surely what Dowding sees as systematic luck is more normally understood as structured privilege. Our crucial argument is that certain groups occupy privileged positions, or enjoy systematic luck. As such, their positions give them access to important policy networks and membership of these networks is a key resource that gives them greater opportunities to affect outcomes. In order to understand or explain outcomes, we need to recognize and *explain* that structured privilege.

Overall, in order to understand and explain outcomes it is important to see how structure and agency interact. In our view, and despite his protests, Dowding's approach stresses intentional explanation, because he defines structure almost entirely in terms of the preferences of agents and the structure of the explanatory model. In contrast, we would argue that one also needs to see how the broader social structure shapes the networks and the resources which members of that network have.

(v) A Dialectical Model

In our article, we identify three dialectical relationships that those who study networks need to examine, between: structure and agency; network and context; and network and outcome. Both Dowding and Raab are critical of this model, while Evans attempts to develop it. Dowding has two main criticisms of our dialectical model. First, he indulges himself by ridiculing our outline of what a dialectical relationship involves. He suggests that we don't follow classical usage (Evans makes a similar point, 2001) and that we use 4 words, when 2 words will

do, when writing of continuing iterations and interactive relationship. Both these points are easily answered. We don't follow classical usage, rather we follow the usage in the structure/agency literature which is more relevant to our concerns (see Hay, 1995). At the same time, we do make it clear what we mean by the term, even if in Dowding's view we are repetitive. As far as the repetition is concerned, we acknowledge it and it is deliberate. We were attempting to clarify our meaning to students sometimes unfamiliar with terms.

Dowding's second criticism, which is echoed by Raab is more important. Essentially, they both argue that we add nothing to previous analyses. Raab in particular suggests (2001) that his work with MacPherson covers much the same ground without: 'a highly questionable schematic diagram'. Similarly, Dowding argues that we are wrong to say that previous authors fail to appreciate the three interactions between structure and agency, network and context and network and outcomes We cannot defend ourselves at length against these charges here. However, returning to the structure/agency debate, we would argue that, even those authors who acknowledge the importance of both structural and agency factors, focus in their empirical work upon either agency or structural factors. So, as we argued above, while Dowding boldly asserts he recognizes the role of structure, in our view he focuses on intentional explanation, as all rational choice theory does.

Dowding argues more specifically that it is perfectly possible to explain the outcomes of British agricultural policy making without recourse to our dialectical model; and this view is certainly shared by Raab. Not surprisingly, we do think our model adds value and the reader is referred to the original article for our justification of that view. We offer it as a model for interpreting, in this case, agricultural policy development in Britain. However, it is up to others to judge whether our model has anything to offer in this or other cases. We do not assert the 'truth' of our interpretation. However, we do assert that no one should be put off considering its utility by Dowding's claim that political science should privilege the testing of formal models underpinned by rational choice theory.

This brings us finally to Evans' reply. He builds on our analysis drawing on Benson's work. He offers an alternative, if related, dialectical model. We find this contribution interesting because it moves the debate forward. However, we can only reiterate Evans' final statement, detailed empirical analysis is needed to establish the utility of our, or Evans', model.

Conclusion

We could defend our position at more length, but our main purpose has been to emphasize a broader point. Political science needs to take ontological and epistemological questions seriously. An epistemological position is a skin not a pullover; everyone has one and researchers need to acknowledge their own and others' positions. It also needs to be stressed that political science is a broad church and no one can dissolve such distinctions by fiat. We do not deny Dowding's right to hold epistemological and methodological preferences, although we do not share them, but that is what they are, preferences, they do not reflect the only way of doing political science.

Indeed, in our view, if formal modeling was the only method utilized in political science, the discipline's output would be much less interesting. To put it another way, which offers more insight into the social world, Goffman's (1969) analysis and interpretation of the operation of total institutions or another *APSR* article on voting patterns in the US Congress? Formal modellers often seem more interested in the elegance of models than the substance of politics.[22]

In contrast, we wish to see political science continue as a rich and varied discipline. To attempt to restrict the way it is done, in the way Dowding advocates, would impoverish it. For as Gouldner (1973: 321) warned:

> The sociological enterprise, like others, becomes edged with a tragic sense, I suspect, precisely because in confining work to the requirements of a demanding and unfulfillable paradigm, sociologists are not using themselves up in their work but are, indeed, sacrificing unexpressed parts of themselves – their playful impulses, their unverified hunches, and speculative imagination – in a wager that this sacrifice is 'best for science'.

In 1978 Barry, surely one of the best political scientists, asserted that (1978, p. v):

> It is always risky to pronounce a verdict of death on ideas even after an extended period of apparent lifelessness, but I predict that we have seen the last of 'sociologists' in political science.

Barry lauded rational choice approaches to the study of politics, even as he belittled sociology and sociologists. Unfortunately, while time has not born out Barry's prediction, neither has it diminished the exaggerated claims of some rational choice theorists. In contrast, we agree totally with Ward, perhaps the best British rational choice theorist (1995, p. 93), who argued that:

> Rational choice theory is a useful set of research methods and heuristics for the tool-kit of all social scientists. Its status is more akin to those of statistical techniques that are appropriate for certain types of data; it is not a stand-alone paradigm for understanding the whole of the political sphere.

(*Accepted*: 19 February 2001)

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Notes

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1 We use the term political science here for convenience, although, of course, many in the profession would reject the notion of science that Dowding employs. Indeed, Dowding suggests (2001, p. 90) that there are two categories of people; those who 'agree' with him and those who 'misunderstand him'. There is no category that covers those who disagree with him. In addition, the reader is told what sort of methods courses political science departments should offer (2001, p. 90) and how to footnote (2001, p. 100), while new institutionalism is dismissed in a throwaway sentence (2001, p. 22).

2 On the changes see Furlong and Marsh (2001).

3 Indeed, in earlier work, Dowding suggests he is a realist, a claim discussed below.

4 For a more extended, but still brief, discussion of these positions, see Furlong and Marsh (2001).

5 Anyone who wants a more extensive treatment of the distinction between scientific and hermeneutic approaches is directed towards Hollis and Smith (1991). This is an excellent book, which is sophisticated, yet accessible. See also Furlong and Marsh (forthcoming).

6 Although, Bevir and Rhodes (forthcoming), who operate from within this tradition, do defend a notion of explanation. However, it is a very different notion of explanation to that used by those in the scientific tradition.

7 The distinction also oversimplifies the interpretive tradition. On the variants within this tradition see Bevir and Rhodes (forthcoming), Chapter 2 and Furlong and Marsh (forthcoming).

8 The claim that qualitative work has produced less understanding of the social world than quantitative methods is itself deeply contentious. Moreover, Dowding pays no attention to the many problems that infect quantitative data from how it is collected, to how it is coded and interpreted – all of which depend on the interpretations and choices of value imbued human agents (see Devine, 1995, p. 144).

9 For an excellent exoposition of epistemological realism see Smith in Hollis and Smith (1991), pp. 205–8.

10 Particularly in the United States which is dominated by behaviouralism and rational choice theory; this may be why Dowding feels happier there.

11 On criticisms of positivism see Hollis and Smith (1991), Chapter 3 and Furlong and Marsh (forthcoming). Of course, the other positions have also been strongly questioned; this is a very contested field, see Furlong and Marsh (forthcoming).

12 This does not mean that positivists do not, and cannot, use qualitative analysis or that those from within the interpretive tradition, do not, or cannot, use quantitative analysis. However, a researcher's epistemological position does tend to lead him or her to privilege one form of data and it definitely affects what conclusions s/he is willing to draw from the data collected.

13 In Dowding's terms farmers enjoyed systematic luck, see below.

14 There have been attempts to formally model non-lineal relationships in the social sciences but they are very crude and often do not meet their own criteria for scientific rigour (see Richards, 1998).

15 This is why Dowding's analogy equating the relationship between a formal model of the world and the 'real' world with that between a model car and a real car is spurious. A model car is a copy of a real car; indeed, subject to resources, it could be a perfect copy. In contrast, formal models of the social world can never offer a perfect representation of that social world; indeed, they always simplify, and invariably oversimplify, it.

16 Perhaps in the end rational choice theorists are more interested in prediction than in explanation?

17 Actually, it is the Marsh and Rhodes' model.

18 The Oxford English Dictionary defines a metaphor as the: 'application of name or descriptive term to an object to which it is not literally applicable'. In this sense policy networks are and will remain metaphors.

19 This point also has a bearing on Dowding's article. He strongly suggests that the main thrust of our article was to criticize him; we are classified as 'one of the recent critiques and "replies"' to his work. However, in the *Political Studies* article we only deal with him in the context of our classification of the literature and spend three lines on his work.

20 However, Marsh did talk of the power of structures in early work (1983), before the error of his ways was pointed out by Ward (1987).

21 Dowding does not explain here how this revealed preference analysis would be undertaken, yet this is an important part of his argument.

22 In fact, Frey and Serna (1995, p. 343) argue that this has been the fate of economics: 'Most non-economists – as well as an increasing number of economists – would wholeheartedly agree that the economics literature has become arcane and inaccessible … This is only partly due to specialized language … Good economics is considered to be abstract and model orientated. This automatically gives a premium to formal mathematical work. Empirical relevance is of secondary importance.'

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