

## Notes and Comments

### ***Understanding Government Survival: Empirical Exploration or Analytical Models?***

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We should begin our reply to Paul Warwick by stating how very flattered we are by the diligent attention that he has given to our work on government formation.<sup>1</sup> In particular, we are delighted that he has taken up and applied the simulation technology that we suggested as a way to explore cabinet stability, a matter on which we were beginning to feel like voices wailing in the wilderness.

This is not the place to bore readers of the *Journal* with detailed points of issue between us, of which there are obviously many. We want here to concentrate upon some larger issues provoked by Warwick's critique. These involve a debate on cabinet stability and duration between a school of thought, in Warwick's corner, that is essentially empiricist in its outlook and a school of thought, in ours, that takes analytical modelling as being more fundamental. This is a theme touched upon by Kaare Strom in his recent review essay on parliamentary democracy.<sup>2</sup>

*Analytical* models are based upon assumptions, often sweeping and heroic assumptions, that are welded by the analyst using a system of logic into a stylized, and thus inevitably simplified, description of the core features of the process under investigation. If the model is to say something of relevance, the assumptions must derive in some way from the real world, so that there must thus be some empirical basis to a good analytical model. The bottom-line objective of such a model of cabinet stability, however, is not to identify the observable phenomena that provide the best empirical fit with the observed durations of past cabinets: it is rather to build an explicit and well-reasoned model of the making and breaking of governments that broadens and deepens our understanding of what might be going on. Empirical correlations may be of only limited utility for this purpose. The ultimate aim of such a model is to expand our intuitions about government stability, and in this way explore all sorts of interesting counterfactuals, perhaps teasing out future possible ways in which a particular case under investigation might develop in the face of completely new circumstances. As aids to intuition and understanding, useful analytical models should be simple and

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<sup>1</sup> See preceding article (Paul V. Warwick, 'Ministerial Autonomy or Ministerial Accommodation? Contested Bases of Government Survival in Parliamentary Democracies', this *Journal*, 29 (1999), 369–94).

<sup>2</sup> Kaare Strom, 'Institutions and Strategy in Parliamentary Democracy: A Review Essay', *Legislative Studies Quarterly*, 23 (1998), 127–44.

parsimonious, with a logical structure that is clear and explicit, and therefore accessible to interrogation with 'what if' questions.

In stark contrast, building an empirical account of government stability by assembling the collection of operationalized variables that best predicts the observed durations of cabinets that have previously formed will almost certainly involve variables that do not necessarily hang together in a carefully reasoned logical structure – it would be remarkably good fortune, indeed, if they did.

It is thus very unlikely that the most coherent and parsimonious analytical models of the making and breaking of governments, those that most aid our intuition, will be the very best at explaining observed cabinet durations. Conversely, the best empirical models will be unlikely to be the most helpful in developing our theoretical intuitions about government stability.

This states the distinction between the two approaches very starkly and we accept that Paul Warwick is no kitchen-sink regression junkie who jams variables into his model with reckless abandon, and with the sole aim of pumping up his *r*-squared. However this deep philosophical and methodological distinction between the two approaches does illustrate why it is not a particularly fruitful exercise to set up, as Warwick has done, a regression tournament between an essentially inductive empirical account of government stability and an essentially deductive analytical account, to be judged solely on the basis of the relative ability of each to fit observed patterns in data derived from past governments. To do so is fundamentally to misunderstand the whole point of analytical theory, since we know before even starting that the empirical account is almost certain to do better; after all, this is what it was solely designed to do. Such accounts, however, rarely provide an *explanation* for what is going on. At best the statistical associations they uncover hint at some factors that *might* be at work; they do not provide an account of how or why. Thus, if we use logical rigour and coherence as our criterion, we know before starting that it will be open season for us to shoot holes in the logical structure of a best-fitting empirical model.

Before going any further, we should set out in our own terms what we take to be the core features of our theoretical approach to the analysis of cabinet stability, elaborated in much greater detail in a recent article by Laver and Shepsle.<sup>3</sup> First, we identify an equilibrium cabinet in the government formation process. As we shall see, we can use any model that generates precise predictions of equilibrium cabinets to do this. The model that we have used in our work to date on cabinet stability is the portfolio allocation (PA) model of the making and breaking of governments,<sup>4</sup> but any other fully specified analytical theory would serve our purpose. If the cabinet that actually forms is not an equilibrium cabinet, either because we can identify no equilibrium cabinet or because the cabinet which forms is an alternative to some equilibrium that we do identify, *then we make no prediction whatsoever about the stability of the incumbent cabinet.*

We should immediately note that Warwick ignores this fundamental point and begins his empirical analysis by testing the assertion, derived as far as we can see as a logical *non sequitur* from a discussion of measurement error, that 'equilibrium governments, as identified here, ought to show some tendency to be more durable than the governments

<sup>3</sup> Michael Laver and Kenneth A. Shepsle, 'Events, Equilibria and Government Survival', *American Journal of Political Science*, 42 (1998), 28–54.

<sup>4</sup> Michael Laver and Kenneth A. Shepsle, *Making and Breaking Governments* (New York: Cambridge University Press, 1996).

deemed out of equilibrium'.<sup>5</sup> In fact we never argue that equilibrium governments last longer. Compare the statement that Warwick tests with what we actually do say on this matter: 'we have nothing at all to say about the stability of out-of-equilibrium governments ... If the equilibrium in place is not anticipated by the model, then its collapse must depend upon factors outside the model's scope'.<sup>6</sup> Asserting an implication that we explicitly rule out is clearly not the 'weak test' of our model that Warwick claims it to be.<sup>7</sup> It is just plain misleading, as, therefore, is the early part of his empirical analysis.<sup>8</sup>

The reason why Warwick conducts this particular empirical analysis is puzzling, since he immediately goes on to show that he does in fact understand very well what we are talking about. In his own words, 'What matters for survival is not so much whether a government conforms to a PA equilibrium as the robustness of that equilibrium'. His empirical findings on the raw impact of equilibrium robustness are that, 'Unlike equilibrium status', which we had never claimed to be related to cabinet stability, 'equilibrium robustness ... does seem to make a difference: its impact on survival is not only highly significant ... but is also much stronger than was the case for Equilibrium Status'.<sup>9</sup> Enough said!

Let us return to what we actually do say about cabinet stability. We surmise that any government, once in office, is subjected to a stream of 'shocks' – political events of lesser or greater significance for its survival. Having identified a PA equilibrium cabinet that is in office, and which is therefore appropriate subject matter for our model, we argue that some equilibria will be more robust than others. That is, some are better able to withstand shocks, modelled as perturbations in model parameters that simulate the critical political events in the real world. We can derive an estimate of the robustness of a given equilibrium in this sense by using Monte Carlo simulations to add random shock terms to model parameters, recalculating the equilibrium after each shock, and estimating the proportion of all shocked cases in which the incumbent equilibrium cabinet is likely to be destabilized. The greater the proportion of shocks of a given amplitude that destabilize the equilibrium cabinet, the less robust we assume that equilibrium to be, and the less stable the corresponding cabinet.

Let us reiterate that this generic approach to the analysis of cabinet stability does not depend at all upon the PA model of government formation. What it does require is *some* model that predicts a precise equilibrium cabinet in a given case, the stability of which can then be explored using the simulation technology we have proposed.

That, in very brief terms, is our approach to the analysis of cabinet stability, which

<sup>5</sup> Warwick, 'Ministerial Autonomy or Ministerial Accommodation?', p. 380.

<sup>6</sup> Laver and Shepsle, 'Events, Equilibria and Government Survival', pp. 44–5.

<sup>7</sup> Warwick, "Ministerial Autonomy or Ministerial Accommodation?", p. 381.

<sup>8</sup> Even the quotation from Laver and Shepsle, *Making and Breaking Governments*, p. 78, that Warwick uses (p. 372) to justify his assertion about our approach explicitly misrepresents the position we have just stated. He has removed from the beginning of the quotation that he takes from our book our very clear statement that 'We have nothing specific to say about government formation in those situations in which there was no strong party or when at least one alternative cabinet is majority-preferred to the dimension-by-dimension median cabinet.' Having suppressed this sentence from the quotation, Warwick goes on to assert (p. 387) a 'contradiction' between this quotation and our later statement that 'we have nothing at all to say about the stability of out-of-equilibrium governments' (Laver and Shepsle, 'Events, Equilibria and Government Survival', pp. 44–5). We might be accused of being repetitive in these two sentences taken from work published two years apart but, once more, it is very misleading to accuse us of contradiction, an accusation that can only be sustained by cutting a crucial sentence out of the quotation.

<sup>9</sup> Warwick, 'Ministerial Autonomy or Ministerial Accommodation?', pp. 381, 383.

Warwick sets up in a regression tournament with what he describes as a 'profoundly different perspective on the challenges of building and maintaining governments in parliamentary democracies'.<sup>10</sup> We do wholeheartedly agree that Warwick's perspective is profoundly different, but for reasons that in our terms make Warwick's regression tournament a rather fruitless exercise.

First, Warwick does not in fact have an alternative model against which to pit the very specific predictions made by the PA approach. He very explicitly admits this himself although, unfortunately for the reader, he makes his confession only in the final paragraph of his article, the entire *raison d'être* of which is ostensibly pitting model against model and finding a winner. Talking of the 'perspective' he has pitted against the PA model, he finally admits that 'whether this perspective can be elaborated into a fully specified theory that holds up to empirical scrutiny remains to be seen'.<sup>11</sup> In fact, Warwick never compares the PA model with a fully elaborated alternative that makes specific predictions of equilibrium cabinets. What he does is pit the PA model against a collection of empirical variables in a regression equation. Indeed, since he offers no logical model at all of government survival, the best we get is some seat-of-the-pants discussion of the face validity of incorporating particular *ad hoc* variables into his empirical analysis.

All of this is a pity because, over the last few years, a number of alternative analytical models of government formation have been elaborated, each carefully argued, and each making predictions about equilibrium cabinets. (Works by Austen-Smith and Banks;<sup>12</sup> Baron;<sup>13</sup> Grofman;<sup>14</sup> Huber;<sup>15</sup> and Schofield<sup>16</sup> provide examples. Lupia and Strom, indeed, have put forward a formal model that deals not only with government equilibrium but explicitly with cabinet stability.<sup>17</sup>) Equilibrium predictions derived from any or all of these alternative models could have been subjected to shock streams using our simulation technology, robustness estimates could have been derived, and these estimates could then sensibly have been pitted against ours in a regression tournament. Given the literature in this area, there was no need to use an *ad hoc* set of empirical 'control' variables rather than an alternative model when evaluating our approach.

<sup>10</sup> Warwick, 'Ministerial Autonomy or Ministerial Accommodation?', p. 392.

<sup>11</sup> Warwick, 'Ministerial Autonomy or Ministerial Accommodation?', p. 392.

<sup>12</sup> David Austen-Smith and Jeffrey Banks, 'Elections, Coalitions, and Legislative Outcomes', *American Political Science Review*, 82 (1988), 405–22.

<sup>13</sup> David Baron, 'A Spatial Bargaining Theory of Government Formation in Parliamentary Systems', *American Political Science Review*, 85 (1991), 137–65.

<sup>14</sup> Bernard Grofman, 'A Dynamic Model of Proto-coalition Formation in Ideological n-Space', *Behavioural Science*, 27 (1982), 77–90; Bernard Grofman, 'Extending a Dynamic Model of Proto-coalition Formation', in Norman Schofield, ed., *Collective Decision-Making: Social Choice and Political Economy* (Dordrecht: Kluwer, 1996), pp. 265–80.

<sup>15</sup> John Huber, 'The Vote of Confidence in Parliamentary Democracies', *American Political Science Review*, 90 (1996), 269–82; John Huber, *Rationalizing Parliament: Legislative Institutions and Party Politics in France* (New York: Cambridge University Press, 1996).

<sup>16</sup> Norman Schofield, 'Existence of a "Structurally Stable" Equilibrium for a Non-Collegial Voting Rule', *Public Choice*, 51 (1986), 267–84; Norman Schofield, 'Stability of Coalition Governments in Western Europe: 1945–1986', *European Journal of Political Economy*, 3 (1987), 555–91; Norman Schofield, 'Political Competition and Multiparty Coalition Governments', *European Journal of Political Research*, 23 (1993), 1–33; Norman Schofield, 'Coalition Politics: A Model and Analysis', *Journal of Theoretical Politics*, 7 (1985), 245–81; Norman Schofield, 'The Heart of a Polity', Chapter 8 in Schofield, ed., *Collective Decision-Making*; Norman Schofield, 'Coalition Politics and Representative Democracy', *European Journal of Political Research*, 31 (1997), 183–92.

<sup>17</sup> Arthur Lupia and Kaare Strom, 'Coalition Termination and the Strategic Timing of Legislative Elections', *American Political Science Review*, 89 (1995), 648–65.

Warwick's lack of sympathy for what is involved in analytical modelling can be seen very clearly in his comments on the success rate of the PA model in predicting equilibrium governments. He notes that PA equilibrium governments actually form in only 31 per cent of the situations in which they could form. This is of course an utterly meaningless number, absent any indication of the difficulty of the predictive task at hand, measured in terms of the number of possible coalition governments that might possibly have formed. If there is only one alternative to the equilibrium prediction, as there never is in reality of course, then getting things right 31 per cent of the time is a disaster, indeed poorer than random guessing. If there are over a thousand alternative governments that could form, as there sometimes are in reality, then a 31 per cent hit-rate is little short of brilliant. We cannot ourselves see where Warwick's 31 per cent figure has come from, but we do provide the relevant information in the proper context in *Making and Breaking Governments*. In relation to our equilibrium concept of the very strong party, for example, we reported that there were 55,634 country-days in the period covered by our cross-national time-series dataset on which this equilibrium prediction could have held true. Given the configuration of possible alternative cabinets in each individual case (quite a tedious number to calculate case by case, it is true, but one that any theorist will appreciate must be calculated to set raw success rates in context) the prediction would have held true by chance on 2,795 days. In fact the model prediction was observed to hold true on 39,111 days.<sup>18</sup> In other words, such was the diversity of possible alternative cabinets in the real world that there was about a 5 per cent probability of our model getting a prediction right by chance, but the actual success rate of the very strong party concept was 70 per cent. Quoting raw success rates without setting these in context, in short, adds nothing to our knowledge. In fact, it is downright misleading.

Moving beyond these big issues to look at the substance of what Warwick actually did, we are afraid that his lack of sympathy with what is involved in analytical modelling has led him to conduct what we can see must have been a very time-consuming data analysis to discover something that he could have learned from carefully inspecting the logical structure of the model in the first place. In a very real sense he has reinvented the wheel, discovering after his extensive empirical work that the equilibrium concepts of the PA model are very intimately related to the sizes of parties in and out of government and to the distribution of party positions on key policy dimensions. We are well aware of that. The conclusions of our book make this point quite clearly. The entire purpose of our model was to explore the precise ways in which these variables interact, within the institutional setting that circumscribes the making and breaking of governments in parliamentary democracies.

Indeed, this permits us to call attention once again to our very different perspectives on what it takes to understand the stability of cabinets in parliamentary democracies. For Warwick, it is sufficient to demonstrate a significant statistical association between observed cabinet durations and the variables measuring party sizes and policy positions. In contrast, for us it is necessary to show *why* these variables matter, a task accomplished by specifying an explicit model and logically teasing out empirical consequences from this – as we have done.

Warwick has fundamentally misunderstood this point in the way he has set up his tournament between the PA model and his 'profoundly different perspective'. The first evidence of this is a startlingly incorrect inference from our theoretical conclusions, one

<sup>18</sup> Laver and Shepsle, *Making and Breaking Governments*, p. 167.

that informs all of his subsequent empirical work. Warwick claims that our model has the ‘surprising implication’ that ‘the degree of diversity in the policy positions of government parties should have no bearing upon government survival’.<sup>19</sup> The implication is certainly surprising since we can find nothing in anything that we have written that can support it.

On the contrary, all of the equilibrium concepts of the PA approach almost always involve the participation in government of parties at the median on at least one key policy dimension. The closing chapter of *Making and Breaking Governments* dwells at some length on the centripetal tendencies implied by our approach.<sup>20</sup> It is logically obvious that any cabinet which includes a median party will have less ideological diversity, on any reasonable measure including the one actually used by Warwick, than a cabinet of equivalent size that excludes this party. Warwick’s asserted implication from our approach is thus incorrect. Especially given our lengthy discussion of the centripetal tendencies in government formation highlighted by our model, it is yet again just plain misleading to set up a contest between our model and some ‘profoundly different’ notion that ideological compactness is a good thing for government survival.

The bottom line in all of this is relatively simple. As Warwick will know very well from his use of our data and computer program, the only data that are input into our model are the policy positions of the various parties and their relative size. (This sets aside the matter of the party-specific salience of the policy dimensions, which is not at issue for Warwick.) Equilibrium status, and in particular equilibrium robustness, is calculated using no more than these data. Our equilibrium concepts are thus, axiomatically, no more than a complex function of the size and policy preferences of the parties in the system. In effect, our computer program, WINSET, calculates the value of that function for the input data associated with a particular case. Far from ‘denying the causal efficacy’<sup>21</sup> of size and policy, as Warwick sweepingly claims we do, our equilibrium robustness predictions are precisely and unequivocally a function of these variables – indeed of these and nothing else! We have not invoked any magic from Mars to help us make our predictions. We have done no more, in a computational sense, than specify a function of size and policy, a point that Warwick simply does not seem to see.

This function is alas complex and ugly, which is why we use simulation experiments to explore the impact on it of party size and policy location. Significant chunks of *Making and Breaking Governments* were devoted to these results, which are nowhere referred to by Warwick. On size, we actually considered a party’s position in the decisive structure of the voting game, which is of more theoretical relevance to the making and breaking of governments than mere size which did not interest us in itself. None the less, all other things being equal, increasing a party’s size tends to improve its bargaining position. The simulations we reported did show that, other things being equal, larger parties are very, very much more likely to be strong or very strong parties than smaller ones.<sup>22</sup> On policy positions, as we have seen, occupying a median position on at least one policy position is effectively a prerequisite for strong-party status, while occupying a median position on more than one policy dimension enhances the odds of being in government to a huge extent.<sup>23</sup>

<sup>19</sup> Warwick, ‘Ministerial Autonomy or Ministerial Accommodation?’, p. 373.

<sup>20</sup> Laver and Shepsle, *Making and Breaking Governments*, pp. 285–7.

<sup>21</sup> Warwick, ‘Ministerial Autonomy or Ministerial Accommodation?’, p. 383.

<sup>22</sup> Laver and Shepsle, *Making and Breaking Governments*, pp. 102–3.

<sup>23</sup> Laver and Shepsle, *Making and Breaking Governments*, pp. 108–9.

We also explored the empirical relationship between size, policy position and strong party status, in results that are both crucial to the argument at hand and once more completely ignored by Warwick. Using the same data set as that used by Warwick, we found very strong relationships between size, policy centrality, and our equilibrium concepts. Our conclusion was: 'What is very clear from all of this is that real political parties are much more likely to be strong, or to be partners of strong parties, if they are large and if they are central.'<sup>24</sup> We could hardly have been clearer than that.

What our model does is to offer a precise statement of why, and in what way, size and ideological diversity are important. These empirical variables are hardly champions for a profoundly different perspective from ours. What we have done is to go beyond a simple gut feeling that size and ideology are important to derive statements about their more precise importance in a *fully-specified model*. Since our equilibrium robustness predictions are a complex function of size and ideological diversity, we should not be surprised that the coefficients of variables associated with our model diminish when Warwick adds size and ideological diversity to his regression model.<sup>25</sup> How could it be otherwise?

The next challenge in Warwick's regression tournament is presented, without warning, right in the middle of his data analysis. It involves a spectacular redefinition, out of the blue, of the key concept under investigation. Remarking in effect that the PA model has too easy a ride on ideological diversity, since it predicts single-party minority governments that obviously have low diversity, Warwick now fundamentally changes his definition of what he means by a government. He now defines a government to include parties that are explicitly *not* in the government. Precisely, what he does is 'to expand the definition of what constitutes government membership to include parties that formally ally with or openly declare support for governments without taking government positions.'<sup>26</sup> This remarkable change of theoretical horses in the middle of his empirical analysis is presented once more on the basis of a few seat-of-the-pants arguments. No mention is made of the fact that the 'confrontation of approaches'<sup>27</sup> is no longer confronting like with like. The PA model, after all, is of its very essence concerned with predicting the allocation of portfolios between parties in the cabinet and, by extension, the stability of these portfolio allocations. It has nothing whatsoever to say about parties that do not hold cabinet portfolios, and for this reason it is very difficult to interpret the precise substantive meaning of all of Warwick's subsequent regressions.

At the end of the day, Warwick does indeed have a profoundly different perspective on government stability from ours. The difference, however, is epistemological rather than substantive. We both demonstrably agree that size and ideology are important and that, furthermore, there are centripetal tendencies in the making and breaking of governments. We profoundly disagree upon whether the construction of analytical models, or the search for the best fitting regression equation, is the most appropriate way to understand this process.

<sup>24</sup> Laver and Shepsle, *Making and Breaking Governments*, p. 185.

<sup>25</sup> Warwick, 'Ministerial Autonomy or Ministerial Accommodation?', p. 383.

<sup>26</sup> Warwick, 'Ministerial Autonomy or Ministerial Accommodation?', p. 384.

<sup>27</sup> Warwick, 'Ministerial Autonomy or Ministerial Accommodation?', p. 385, Table 2.

## ***Getting the Assumptions Right: A Reply to Laver and Shepsle***

PAUL V. WARWICK\*

For some time now, formal modelling has been touted by its supporters as a panacea for political science – or at least as a major step forward in the discipline's development. Certainly, it embodies a number of praiseworthy elements. Its insistence on starting with a parsimonious and precisely formulated set of assumptions cannot help but constrain slippery thinking, for example, and its rigorous working out of implications, while often demonstrating the obvious, occasionally leads to unanticipated and intriguing results. Moreover, the combination of precision and rigour holds forth the promise of generating relatively clear-cut tests of rival explanations, a major boon – if it proves true – in a discipline more inclined to abandon theories than to disconfirm them.

How much better the analytical or formal orientation is, then, than the 'funnel of causality' approach of empiricists whose quest for the highest explained variance seldom produces more than a miscellaneous grab-bag of influences on the dependent phenomenon. Empirical work of that sort may have some limited utility in identifying possible causes, to be sure, but at some point the scholarly enterprise must move to the higher level of elaborating a clear logical structure among causal factors. Here, empirical success in accounting for observed phenomena cannot be the sole guide: the best theory is the one that provides the most accurate idea of what is actually going on in the real world, not the one with the best correlations.

This is the mould into which Laver and Shepsle have attempted to fit my recent effort to evaluate their 'portfolio allocation' (PA) theory of parliamentary government.<sup>1</sup> The mould casts theirs as 'an explicit and well-reasoned model of the making and breaking of governments that broadens and deepens our understanding of what might be going on'; what I test it against is merely a 'collection of operationalized variables that best predicts the observed durations of cabinets'.<sup>2</sup> Although this collection of variables generates a better empirical account of cabinet survival, it would be 'remarkably good fortune' if these variables turned out to 'hang together in a carefully reasoned logical structure'.<sup>3</sup> If you optimize for explanatory power, in other words, you get the best explanatory model; if understanding of the true causal mechanisms is the goal, some explanatory capacity may have to be sacrificed – but the gain is immeasurably greater.

In this Note, I shall argue that this mould misshapes my efforts in almost every respect. To begin with, a formal model only broadens and deepens our understanding to the extent that its truth-value has been established; without proper evaluation, it is just another untested contender. My efforts to evaluate PA theory's truth-value did not involve pitting it against a diverse collection of independent variables chosen for their

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<sup>1</sup> Michael Laver and Kenneth A. Shepsle, 'Understanding Government Survival: Empirical Exploration or Analytical Models?' this *Journal*, 29 (1999), 395–401. My evaluation of their theory appeared in Paul V. Warwick, 'Ministerial Autonomy or Ministerial Accommodation? Contested Bases of Government Survival in Parliamentary Democracies', this *Journal*, 29 (1999), 369–94, and the theory itself is presented in Michael Laver and Kenneth A. Shepsle, *Making and Breaking Governments* (Cambridge: Cambridge University Press, 1996).

<sup>2</sup> Laver and Shepsle, 'Understanding Government Survival', p. 396.

<sup>3</sup> Laver and Shepsle, 'Understanding Government Survival', p. 396.



explanatory power; instead, I pitted it against a single hypothesis based on a premise that is fundamentally opposed to PA theory's core assumption of ministerial autonomy. The aim was not to prove that it is possible to assemble a more powerful set of explanatory variables but to discover which fundamental assumption – ministerial autonomy or its opposite, ministerial accommodation – provides the better basis from which to construct a theory of parliamentary government. The test was motivated by a conviction that the key to making progress in this or any other topic lies not in replacing a proliferation of empirical models with a proliferation of formal ones; rather, it lies in getting the assumptions right.

#### THE ISSUE OF FIRST PRINCIPLES

Despite Laver and Shepsle's assertions, there is no dispute between us that, other things being equal, it is preferable to have a fully specified theory of parliamentary government, logically premised on a reasonable and reasonably parsimonious set of assumptions about human behaviour, than it is to have a set of correlates justified by 'a few seat-of-the-pants arguments'.<sup>4</sup> This topic, like many others in political science, has seen a proliferation of formal models generated with the very aim of achieving this higher state of knowledge. The problem is that things are not equal in one very important respect: by and large, the empirical viability of these models is unknown. In fact, formal models relating to parliamentary government are typically advanced with little more than anecdotal evidence.<sup>5</sup> As for the few models that do come with systematic evidence – and I would have to include Laver and Shepsle's own theory here – the standard procedure has been to measure only those variables that are implied by the model and to show that they are significantly related to the dependent phenomenon. This establishes an initial plausibility, but it does not take us very far in terms of establishing causality.

The reason that causality remains elusive is that almost all of the factors that have been proposed in the literature, whether as part of formal models or not, turn out to be related in some fashion to the composition and/or duration of parliamentary governments. In my full-scale assessment of government survival,<sup>6</sup> I was virtually overwhelmed by the number of alternative causes that had been advanced in the literature – and the proportion of them that showed significant bivariate relationships with government survival. The challenge of producing an independent variable that correlates with survival turns out to be no challenge at all; the real task is to determine which associations are causal and which are not.

In order to make these critical determinations, it is necessary to bring theories and hypotheses into confrontation with one another. The question is, which ones should they be? Laver and Shepsle argue that I ought to have tested their theory against one of a variety of other formal theories that they regard as worthy contenders, as if a fully-specified theory can only be properly tested against another fully-specified (i.e.

<sup>4</sup> Laver and Shepsle, 'Understanding Government Survival', p. 401.

<sup>5</sup> This is largely true, for example, of the collection of models cited by Laver and Shepsle, 'Understanding Government Survival', p. 397. Those proposed by John Huber, *Rationalizing Parliament: Legislative Institutions and Party Politics in France* (New York: Cambridge University Press, 1996), are perhaps the most thoroughly tested, but they deal with institutional features unique to one system, the French Fifth Republic.

<sup>6</sup> Paul Warwick, *Government Survival in Parliamentary Democracies* (Cambridge: Cambridge University Press, 1994).

formal) theory.<sup>7</sup> But even assuming such tests could be conducted (these theories tend to be very difficult to grapple with empirically), the very specificity of the theories creates the possibility that the tests would not go to the heart of the matter. For PA theory, the heart of the matter is the assumption of ministerial autonomy.

The ministerial autonomy assumption that underpins PA theory paints a picture or 'intuition'<sup>8</sup> of coalition governments that is fundamentally at odds with almost everything that has previously been supposed to take place. It assumes, in essence, that coalition partners do not bargain over policy except to the extent of deciding which parties will get which portfolios; once that allocation is decided, the government's policy output consists entirely of individual ministers implementing their own parties' policies in the areas under their jurisdiction. In this intuition, finding policy compromises among parties with differing policy stances is not the fundamental factor, indeed not a factor at all, in the formation and survival of coalition governments.

This is a very bold assumption. If it were true, it would relegate the common-sense notion that coalition government entails a need to reach policy compromises, and any theories that depend upon this notion, to the scrap-heap. Conversely, finding out that the ministerial autonomy assumption is incorrect – that coalition partners do expect to bargain out policy differences even in areas controlled by portfolios not allocated to them – would not only eliminate PA theory from the ranks of contenders but would also identify a core assumption whose presence is likely to be required in any future theoretical work.

It was this stark contrast of fundamental assumptions that suggested to me that the best foil for PA theory would be a very simple hypothesis that encapsulates the alternative assumption of ministerial accommodation – the hypothesis that the policy distance or ideological diversity among government parties is a major influence on the government's prospects for survival. This hypothesis assumes that policy is something that has to be negotiated and compromised among government parties and that the greater these compromises, the more difficult it will be to sustain them. The intuition, in short, is that coalition government depends upon the ability of member-parties to reach common policy stances involving all major policy issues or dimensions.

One recent formal model that incorporates the ideological diversity hypothesis is Tsebelis's 'veto players' model.<sup>9</sup> Like PA theory, it assumes that the survival of governments follows from their robustness with respect to changes in the parliamentary configuration of forces. Robustness in this case refers to the capacity to adopt new policy positions to meet changing circumstances. Ideological compactness enhances this capacity because it expands the range of policy alternatives that would leave all members of the government at least as well off as they are with the present policies and whose adoption would therefore pose no risk to its survival. The degree of ideological diversity among coalition parties is thus a good indicator of non-robustness and hence of vulnerability to changes in the parliamentary environment.

From this perspective, my decision to test PA theory against the ideological diversity hypothesis can be seen as a decision to evaluate the stability implications of robustness as conceptualized in two different formal models, something that Laver and Shepsle

<sup>7</sup> Laver and Shepsle, 'Understanding Government Survival', p. 398.

<sup>8</sup> Laver and Shepsle, 'Understanding Government Survival', p. 395.

<sup>9</sup> George Tsebelis, 'Decision-Making in Political Systems: Veto Players in Presidentialism, Parliamentarism, Multicameralism, and Multipartism', *British Journal of Political Science*, 25 (1995), 289–325.

castigate me for not doing.<sup>10</sup> But, in fact, the test involves more than this. There is no reason to suppose that the veto-players model is the only model that can incorporate the ideological diversity hypothesis and the assumption of ministerial accommodation that underlies it; the assumption, in particular, can be found in most theoretical work on coalition governments.<sup>11</sup> This fundamental quality of the hypothesis – the fact that it follows so directly from so common an assumption – is precisely what makes it a better foil for PA theory than any one theory that incorporates or implies it. If theory-building is to progress, what we really need to know is which of these two basic premises, ministerial autonomy or ministerial accommodation, constitutes the superior foundation for constructing models of parliamentary government. In bringing PA theory into confrontation with the ideological diversity hypothesis, my objective was to get as close as possible to this issue.

#### A FAIR TEST?

I doubt very much that Laver and Shepsle would deny the usefulness of attempting to find out whether ministerial autonomy or ministerial accommodation provides the sounder basis for understanding parliamentary government, but they certainly do object to the methodology I used to make that determination. One of their principal concerns is that testing their theory against a collection of variables selected solely on the basis of their ability to explain government survival does not constitute a fair test: as they put it, the collection of variables is ‘almost certain to do better; after all, this is what it was solely designed to do’.<sup>12</sup>

A natural response to this objection would be to ask why a set of variables that reflect other causal hypotheses should have outperformed variables suggested by PA theory if the latter theory is the one that captures most accurately the underlying causal mechanism. Could it have happened for essentially extraneous reasons or would it suggest that perhaps PA theory has mistaken that mechanism? We could quite legitimately ask these questions, but we don’t have to – because *there was no set of best-fitting variables against which PA theory was tested*. In contrast to my own empirical work on government survival, which ultimately identified ten such variables, the only variables that PA theory had to confront were a measure of the maximum range of policy positions held by government parties (‘Maximum Policy Range’) and a

<sup>10</sup> Laver and Shepsle, ‘Understanding Government Survival’, p. 398.

<sup>11</sup> Another theory that contains the ideological diversity hypothesis is the theory of democratic party government proposed in Ian Budge and Hans Keman, *Parties and Democracy: Coalition Formation and Government Functioning in Twenty States* (Oxford: Oxford University Press, 1990), one of whose implications is that, for coalitions, ‘the more ideologically mixed are more likely than those less ideologically mixed to terminate for involuntary internal reasons’ (Budge and Keman, *Parties and Democracy*, p. 51). Similarly, the framework proposed in Bernard Grofman and Peter Van Roozendaal, ‘Toward a Theoretical Explanation of Premature Cabinet Termination with Application to Post-War Cabinets in the Netherlands’, *European Journal of Political Research*, 26 (1994), 155–70, at p. 159, incorporates the hypothesis that ‘Ceteris paribus, parties that suffer from great discrepancies between their most preferred positions on the relevant policy dimensions and the collective positions of the cabinet are more likely to precipitate a cabinet crisis than parties that do not or suffer less from such discrepancies’. Recent theoretical efforts that assume ministerial accommodation include David Baron, ‘A Spatial Bargaining Theory of Government Formation in Parliamentary Systems’, *American Political Science Review*, 85 (1991), 137–64, and Christophe Crombez, ‘Minority Governments, Minimal Winning Governments, and Surplus Majority Governments in Parliamentary Systems’, *European Journal of Political Research*, 29 (1996), 1–29.

<sup>12</sup> Laver and Shepsle, ‘Understanding Government Survival’, p. 396.

dichotomous indicator of the government's majority or minority status ('Majority Status').

Not only was the ideological diversity hypothesis represented by just two variables, but neither of them was chosen because it performs well in a statistical sense. In fact, in the case of the principal variable of interest for the hypothesis, Maximum Policy Range, I went out of my way to avoid any such bias by operationalizing it from policy scales generated to test PA theory. Far from favouring the ideological diversity hypothesis, these scales actually incorporate a bias that favours Laver and Shepsle's theory: among extant approaches, only the focus on portfolio allocation leads to the identification of attitudes towards the Soviet Union as the second key policy dimension in most West European parliamentary systems.<sup>13</sup> If the decision to base the theoretical confrontation on these scales advantages either alternative, that edge must go to PA theory.

The real reason that Maximum Policy Range and Majority Status were chosen is because they encapsulate the contrast in basic assumptions between the two approaches. For the ideological diversity hypothesis, what matters for government survival is how much diversity the government has to accommodate and whether that diversity provides majority support. The latter is important because if the government does not command a parliamentary majority, additional support will have to be sought out and this may well mean additional policy preferences that need to be accommodated in some fashion. The measurement of the full extent of ideological or policy differences that matter for government survival is therefore likely to be seriously under-estimated in the case of minority governments, which means that the distinction between majority and minority governments must play an important part in the analysis.

The test, then, was not stacked against PA theory in a statistical sense. But is it an appropriate test in a substantive sense? Again, Laver and Shepsle demur. They suggest that I have merely 'reinvented the wheel'<sup>14</sup> by showing nothing more than that the equilibrium concepts of PA theory are closely connected with the size and policy positions of parties. What they mean is this. PA equilibria are calculated from information on the sizes and policy positions of parties in the legislative arena. An important manifestation of this fact is that PA equilibria show strong centripetal tendencies, that is, they are very likely to be centrally located in the policy space. This being the case, it stands to reason that equilibrium governments will tend to be ideologically compact; hence the finding that ideological compactness is conducive to survival is precisely what one would expect if PA theory is true. Laver and Shepsle are therefore nonplussed that I should maintain that that their theory implies that ideological diversity is not causally implicated in government survival: 'Especially given our

<sup>13</sup> Laver and Shepsle identified this issue as key because the foreign affairs portfolio appears to be the second most important portfolio in most systems; hence, the principal foreign affairs issue in this era, which they take to be the Soviet issue, becomes the second policy dimension in these systems. I am aware of no other work that has taken this position. L.C. Dodd, *Coalitions in Parliamentary Government* (Princeton, NJ: Princeton University Press, 1976), took the clerical-secular division to be the second most important policy dimension, for instance, and Warwick, *Government Survival*, found diversity along this dimension to be a powerful influence on government survival. Analyses of party manifestos, e.g. Ian Budge, David Robertson and Derek Hearl, eds, *Ideology, Strategy and Party Change: A Spatial Analysis of Post-War Election Programmes in Nineteen Democracies* (Cambridge: Cambridge University Press, 1987) and Crombez, 'Minority Governments, Minimal Winning Coalitions and Surplus Majorities in Parliamentary Systems', have generally shown the second dimension to pit conservatism/authoritarian against libertarianism/postmaterialism.

<sup>14</sup> Laver and Shepsle, 'Understanding Government Survival', p. 399.

lengthy discussion of the centripetal tendencies in government formation highlighted by our model, it is yet again just plain misleading to set up a contest between our model and some “profoundly different” notion that ideological compactness is a good thing for government survival’.<sup>15</sup>

This is perhaps Laver and Shepsle’s most fundamental criticism of my tests; nevertheless, it rests on a fundamental confusion of causation and correlation. As it happens, I agree whole-heartedly that their equilibrium concepts are functions of party sizes and positions. Moreover, I agree that equilibrium governments tend to be ideologically compact (in my data, the correlation between Equilibrium Status and Maximum Policy Range is  $r = -0.314$ ,  $p < 0.001$ ,  $n = 247$ ). But if this is the reason why ideologically compact governments appear to be more durable, it would mean that the linkage between ideological diversity and survival is *spurious* – the result of their common association with PA equilibria. What PA theory denies, as my article made very clear, is not the connection between ideological diversity and survival but its causal import.<sup>16</sup>

What we essentially have are two interrelated variables, both derived in part from the same information (party positions) and both correlated with government survival. Because the variables embody fundamentally opposed assumptions about the mechanics of government formation and maintenance, they cannot both be causal factors. One of the correlations, in other words, must be spurious. This is precisely what my data analysis set out to determine. Laver and Shepsle elide this distinction between cause and correlate, first, by (mis)interpreting my position to be one of assuming that PA theory must rule out any connection at all between ideological diversity and survival and, secondly, by assuming that ‘their’ correlation must be the causal one. As noted earlier, it is woefully easy to get bivariate correlations in the study of parliamentary government; the real challenge is to identify the causal ones.

Close followers of this debate may have noticed another slippage in Laver and Shepsle’s line of reasoning: it is premised on the argument that equilibrium governments should last longer – a position that they have emphatically denied. In fact, there are two senses in which they are trying to have it both ways on this issue. First, they assert that they have nothing to say about out-of-equilibrium governments and also that such governments should be short-lived.<sup>17</sup> Secondly, while insisting that the former view is

<sup>15</sup> Laver and Shepsle, ‘Understanding Government Survival’, p. 400.

<sup>16</sup> Warwick, ‘Ministerial Autonomy or Ministerial Accommodation?’, p. 373.

<sup>17</sup> Laver and Shepsle, ‘Understanding Government Survival’, p. 396, still dispute this interpretation and, indeed, accuse me of ‘suppressing’ part of their statement in order to generate a false contradiction in their position. The situation is as follows. In Laver and Shepsle, ‘Events, Equilibria, and Government Survival’, *American Journal of Political Science*, 42 (1998), 44–52, at pp. 44–5, they state that they have nothing to say about the stability of out-of-equilibrium governments. That seems clear enough, but in *Making and Breaking Governments*, p. 78, their position is that they ‘have nothing specific to say about government formation’ in situations where no PA equilibrium government can be formed. I take this to mean that they have nothing to say about such matters as the government’s party composition or portfolio allocation, partly because the statement refers to government formation, not stability, and partly because they immediately go on to explain in very clear terms that their theory anticipates that such governments will be short-lived: ‘there is nothing in our model to imply anything other than a chaotic sequence of proposal and counter-proposal. Any cabinet that might take office would appear to be generically unstable, since some alternative must be majority-preferred to it, and this alternative cannot be prevented from forming by the vetoes of a strong party’. In my view, to say that these governments are generically unstable is to say something about them. Moreover, since out-of-equilibrium governments formed under other circumstances also face majority-preferred alternatives, the conclusion of generic instability must apply to all out-of-equilibrium governments.

the one that should be taken seriously, they use the latter view to account for the ideological diversity relationship. Clearly, the argument that ideologically compact governments appear to last longer because they are more likely to be in PA equilibrium assumes that equilibrium governments last longer. Without this assumption, their explanation for the ideological diversity effect crumbles.<sup>18</sup>

It is not my intention to argue that Laver and Shepsle's interpretation of the ideological diversity effect fails because it is logically incoherent. On the contrary, I believe that the proposition that out-of-equilibrium governments should be less durable on average is a reasonable inference from their work and, consequently, that their argument that the ideological diversity effect follows spuriously from this fact should be taken seriously. My point is simply that determining which effect is spurious constitutes an entirely appropriate test of the validity of the differing assumptions that underlie them. It is not sufficient simply to assert that PA theory can provide an explanation for the ideological diversity relationship; it must be shown that the assertion bears up to empirical scrutiny.

#### A COMPARISON OF TESTS

Let us turn, then, to the evidentiary basis of the debate. Unlike the case with respect to many other formal theories, the full elaboration of the portfolio allocation model in *Making and Breaking Governments* comes with a substantial array of empirical support. Typically, however, the evidence consists of showing that the success rate of the theory's predictions surpasses what would be expected from chance alone, not what is generated by any other theory.<sup>19</sup> This is equivalent to a showing that a bivariate relationship is statistically significant without introducing any controls – as I have emphasized, a very easy thing to do with this topic.

The matter of the formation rate of PA equilibrium governments provides an excellent case in point. Laver and Shepsle castigate me for observing that only 31 per cent of governments in my dataset matched what PA theory would predict.<sup>20</sup> They believe that the rate of successful prediction should be compared with what randomness would produce; they also maintain that I should not have counted the proportion of successfully predicted governments but rather the proportion of 'country-days' in which such governments held office. I have no objection to this latter point, especially as it does not produce any significant alteration in the rate of success in my data.<sup>21</sup> As to the former

<sup>18</sup> A counter-argument would be that Laver and Shepsle are actually utilizing the premise that equilibrium robustness (as opposed to equilibrium status) is related to survival. Against this interpretation is the fact that their discussion of the centripetal tendencies of PA equilibria, both in 'Understanding Government Survival', pp. 400–1, and in *Making and Breaking Governments*, pp. 285–7, concerns the equilibria themselves, not their levels of robustness. Nevertheless, in refereeing the first draft of my article, they did indicate a way in which robustness could be associated with compactness, which I duly noted (Warwick, 'Ministerial Autonomy or Ministerial Accommodation?', fn. 14). Moreover, it turns out that there is a significant tendency for equilibrium robustness, as I have measured it, to be linked with ideological compactness. Further analysis reveals, however, that most of the effect (80 per cent) is due to equilibrium status.

<sup>19</sup> The one exception is the probit regression of government membership, in which the size and centrality of parties were included as controls (Laver and Shepsle, *Making and Breaking Governments*, pp. 185–90). This analysis provided only partial support for the theory, however: 'very strong parties' were significantly more likely to end up in governments, as predicted, but this was not the case for 'merely strong parties'.

<sup>20</sup> Laver and Shepsle, 'Understanding Government Survival', p. 399.

<sup>21</sup> It does seem to me, however, that the only valid rationale for using country-days as the standard is to incorporate the notion that equilibrium governments tend to last longer, which Laver and Shepsle vehemently deny (see above). Note that the 70 per cent success rate that Laver and Shepsle cite concerns only governments based

point, however, it would surely be more valuable to compare the rate of successful government predictions against one or more contending perspectives, rather than against that produced by a random process of government formation which must inevitably generate many outcomes that no reasonable theory would countenance.

The limited nature of their empirical testing is even more apparent with respect to the issue of government survival. Having determined which governments in their dataset are in equilibrium, one might suppose that Laver and Shepsle would have utilized the simulation capacity of their computer program to estimate the robustness of these equilibrium positions and then related this to their subsequent survival records (this, after all, is what they propose as the appropriate approach for investigating cabinet stability).<sup>22</sup> Instead, they employed that simulation capacity primarily to explore the robustness of equilibria in hypothetical party systems, confining their analysis of actual parliaments to just two cases (Germany in 1987 and Ireland in 1992–93).<sup>23</sup> The upshot is that we are given no indication as to whether the robustness of PA equilibria is a statistically significant predictor of government survival, much less whether it outperforms other explanations in this regard.

My work was intended to tackle both lacunae: to assess the existence and robustness of actual equilibrium governments and to evaluate the explanatory capacity of these variables *vis-à-vis* those based on the ideological diversity hypothesis. As we have seen, the fact that all of the variables in question have significant linkages with government survival means that the critical issue is to identify the spurious one(s). In particular, we need to determine whether Equilibrium Robustness appears to be related to government survival because it is a correlate of the true cause, Maximum Policy Range, or whether robustness constitutes the cause and policy range the spurious correlate.

My findings conform to the ideological diversity interpretation in every respect: in majority cases, where the full extent of the government's policy range is likely to be captured reasonably accurately, the superior explanatory power of the policy range variable is evident; in minority cases, the evidence makes it equally clear that the residual explanatory power of equilibrium robustness exists only because ideological diversity has been under-estimated. The latter point merits some elaboration because it has been challenged by Laver and Shepsle. Specifically, they argue that my attempt to capture some of the unmeasured ideological diversity inherent in minority government situations involves a 'remarkable change of theoretical horses'<sup>24</sup> that renders the test meaningless. The change they have in mind is the expansion of the definition of government membership to include parties that formally ally with or openly declare support for the government.

Redefining government membership to include one or more parties not represented in the cabinet would certainly seem to be a major conceptual shift. Appearances, however, can be deceptive: all that it really means is that we are now counting the policy

(*F*'note continued)

on 'very strong parties', one of their three types of equilibrium. I, too, find a high rate for these governments (49 per cent), but the rate drops off substantially to 30 per cent when all types of equilibrium are considered. It is also fair to note that Laver did not see fit to impose the 'country-days' standard when Budge and he evaluated other formation theories (Ian Budge and M. L. Laver, 'The Relationship Between Party and Coalition Policy in Europe: An Empirical Synthesis', in M. L. Laver and Ian Budge, eds, *Party Policy and Government Coalitions* (New York: St Martin's Press, 1992), pp. 415–20).

<sup>22</sup> Laver and Shepsle, 'Understanding Government Survival', p. 397.

<sup>23</sup> Laver and Shepsle, *Making and Breaking Governments*, pp. 125–46. The same two cases are investigated in Laver and Shepsle, 'Events, Equilibria, and Government Survival', pp. 44–52.

<sup>24</sup> Laver and Shepsle, 'Understanding Government Survival', p. 401.

positions of parties that have publicly committed their support to the cabinet as factors that the cabinet needs to take into account. The theoretical basis remains the same; the change was simply an attempt to measure ideological diversity in a way that better captures its full operative extent. Since PA theory takes no account of these parties, its measurement was unaffected by this change. Each concept, in other words, was measured according to its own logic.<sup>25</sup>

What I found is that the inclusion of the policy positions of support parties in the measurement of Maximum Policy Range does strengthen that variable's effect on government survival, as anticipated. But it must be emphasized that the interpretation I placed on minority cases does not depend on this result or on the definitional change that produced it. Rather, it hinges on my demonstration that Equilibrium Robustness appears to be a factor in the survival of these governments only because the ideological diversity of equilibrium minority governments is less seriously under-estimated than that of other minority governments. The robustness effect for minority governments, in short, shows every sign of being spurious. In focusing on the change in the definition of government membership, Laver and Shepsle are isolating a side issue and missing the main point.

At bottom, my data analysis involved a one-on-one confrontation of two variables, both derived from the same data on party positions. Majority and minority cases were distinguished only because there was good reason to believe that the policy range variable seriously under-estimates the concept it is intended to measure in minority cases; I then presented evidence to show that the appearance of an equilibrium robustness effect in these situations is entirely due to that fact. No amassing of independent variables was undertaken in order to produce these results. Nor can it be said that the variable measuring policy range was chosen for its explanatory power; as noted above, I deliberately cast its measurement in a way that ought to have handicapped the ideological diversity hypothesis. Nevertheless, it emerged as the clear winner.

It is, of course, always possible to argue that the results prove only that ideological diversity has been measured more adequately than equilibrium robustness, not that it is the more likely cause. Let me therefore point to a piece of evidence that cannot be cast in this mould: the evidence for a majority status effect. Unlike the situation for ideological diversity, it is difficult to see how PA theory can accommodate the tendency I found for majority governments to survive longer. In order to interpret this relationship as a spurious manifestation of PA equilibria, it would be necessary for PA equilibrium governments, or perhaps robust equilibrium governments, to show tendencies both to be majoritarian and to survive longer. The former condition, however, is not met: PA equilibrium governments are actually more likely to be minoritarian. Similarly, the robustness of PA equilibria is negatively associated with majority status.<sup>26</sup> It is not surprising, therefore, that the majority status effect should have been ignored in Laver and Shepsle's attempts to come to grips with my results.

<sup>25</sup> In order to maintain consistency, counting support parties as part of the governing coalition requires that the addition or loss of a support party be treated as marking a government termination. The number of cases affected by this stipulation is minuscule, however, and it has no appreciable effect on the results of the data analysis.

<sup>26</sup> In my data, Majority Status correlates with Equilibrium Status at  $r = -0.19$  and with Equilibrium Robustness at  $r = -0.137$ . Both correlations are significant at the 0.05 level.



## CONCLUDING REMARKS

My hope is that the preceding discussion has dispelled any notion that I believe that 'it is sufficient to demonstrate a significant statistical association between observed cabinet durations and the variables measuring party sizes and policy positions'.<sup>27</sup> To the contrary, my entire analysis had the aim of separating causality from mere statistical association. Those who advance theories on the basis of bivariate relationships rather than controlled tests, it seems to me, are the true perpetrators of this type of fallacy. My goal was and remains the identification of causes.

Identifying which relationships are causal and which are spurious is, of course, no easy matter. Perhaps the most fundamental difference between Laver/Shepsle and myself is that they believe the 'logical rigour and coherence'<sup>28</sup> of the explanations that underpin them can do the job, whereas I maintain that the issue must ultimately be settled through evidence. In the present case, we are faced with two explanations for government survival, one predicated on the assumption of ministerial autonomy and the other on the assumption of ministerial accommodation. I have shown that both can generate statistically significant relationships with government survival; moreover, both can be interpreted in terms of robustness, which Laver and Shepsle see as the key to understanding survival. Given the opposed nature of the assumptions, however, it is clear that one of the explanations must be false and the relationships emanating from it spurious. Logic alone will not determine which explanation ought to receive this designation.

Indeed, if logic were the ultimate guide, it might militate against Laver and Shepsle's theory. It might suggest, for example, that the ideological diversity explanation ought to be preferred since Laver and Shepsle's attempt to discredit it is inconsistent: the interpretation of the association of ideological diversity with survival as a spurious consequence of their common association with PA equilibria relies on the notion that PA equilibrium governments tend to be durable, a notion which Laver and Shepsle strenuously reject. Alternatively, it could be argued that since (1) PA equilibria are 'complex and ugly'<sup>29</sup> functions of party sizes and positions, whereas ideological diversity can be measured by the simple range of positions held by government parties, and (2) the latter seems to perform better empirically, then it ought to be preferred on the principle of Ockham's razor. Neither of these avenues is acceptable to me, however: I seek a firm evidentiary basis.

The finding that the equilibrium effects disappear when ideological diversity is adequately measured and introduced into the analysis constitutes only a part, albeit an important one, of this evidentiary basis. This result is consistent with the argument that the equilibrium effects are spurious; nevertheless, the possibility remains that it is simply a consequence of superior measurement of the ideological diversity concept. A consideration that mitigates against this possibility is the fact that both the equilibrium concepts and ideological diversity were operationalized on the basis of party positions designed for PA theory. A more significant consideration, to my mind, is that the overall pattern of findings with respect to equilibrium robustness is consistent with a verdict of spuriousness: not only is the equilibrium robustness effect eliminated by ideological diversity in majority cases, but the evidence clearly indicates that its residual effect

<sup>27</sup> Laver and Shepsle, 'Understanding Government Survival', p. 399.

<sup>28</sup> Laver and Shepsle, 'Understanding Government Survival', p. 396.

<sup>29</sup> Laver and Shepsle, 'Understanding Government Survival', p. 400.

among minority governments is entirely attributable to the under-estimation of the relevant extent of ideological diversity that these governments have to cope with. A final empirical signal is the finding of a significant net tendency for majority governments to survive longer, which is not anticipated by PA theory and cannot be interpreted as a spurious outcome of it.

The evidence thus appears to favour the ideological diversity hypothesis quite strongly. Nevertheless, few tests are conclusive and I am reluctant to apply that designation to my own. But my hesitation stems not from any defects in their design – the tests involved a focused testing of PA theory's capacity to account for government survival against a very basic hypothesis that flows directly from an incompatible assumption, not some 'regression tournament'<sup>30</sup> involving an *ad hoc* collection of variables, as Laver and Shepsle would have it. Instead, my hesitation stems from the fact that PA theory opens up so much scope for measurement error: have key policy dimensions and portfolios been correctly identified? Are the simulations appropriately specified to capture levels of equilibrium robustness accurately? And so forth. This broad scope for measurement error does not constitute a fault of PA theory, to be sure, but it does make it difficult to pin down empirically.

Despite the risk of mismeasurement, theories must be tested. As noted earlier, one of the predicaments of political science is that the field contains so many theories, so few of which have been thoroughly subjected to rigorous empirical scrutiny. A growing accumulation of theories whose empirical validity is uncertain does no service to the advancement of the discipline in itself; it simply makes things messier. As in a good murder mystery, it may be intriguing to have lots of suspects – but things only move forward when the investigator begins to eliminate them one by one. I believe my work constitutes a step along this path.

<sup>30</sup> Laver and Shepsle, 'Understanding Government Survival', p. 398.

## ***Government Formation and Survival: A Rejoinder to Warwick's Reply***

MICHAEL LAVER AND KENNETH A. SHEPSLE\*

When an intellectual exchange gets 'curiouser and curiouser', it is probably time to bring it to a conclusion in the hope that scholars other than the principals involved will bring a fresh perspective to the issues in dispute. Certainly when the dispute begins to focus on fine-grained details that are of interest only to the disputants, it is time to move the discussion back to a more general plane. This we shall do, and do in the briefest of ways.

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For those just coming to the debate at this point, a review of the bidding shows the following:

- In the beginning there was our book on government formation and Warwick's on government durability.<sup>1</sup> These volumes set out, respectively, an analytic theory of government formation in parliamentary democracies (with some supporting evidence) and an empirical assessment of the survival of parliamentary governments.
- Warwick wrote an extensive empirical piece in which government survival issues were used to assess the Laver–Shepsle government formation model. In particular, he wished to take issue with the 'ministerial autonomy' assumption of Laver and Shepsle's portfolio allocation (PA) model.<sup>2</sup>
- We took exception to his paper on several grounds.<sup>3</sup> Epistemologically, we felt he was not confronting like with like. His was a purely empirical effort in which he provided no theoretical sense of what one should expect under the condition of 'ministerial accommodation', his alternative to the ministerial autonomy assumption of our PA model. Empirically, his focus was much more on a theme – government survival – with which his own work was associated (see footnote 1) than was ours. Indeed, our PA model made only occasional and tangential reference to issues of government survival, though we did address the latter issue (in a context broader than our own PA model) elsewhere.<sup>4</sup>
- Warwick has now replied to our critique.<sup>5</sup> The editors of this *Journal* kindly provided us with one more opportunity to clarify this increasingly murky exchange.

Warwick, in this latest piece, is intent upon 'getting the assumptions right'. He is convinced that parliamentary party leaders engage in negotiations and bargaining which, when consummated in the formation of a government, require cabinet ministers to accommodate their own actions to the deal that has been struck. Ministers, according to Warwick, are not 'autonomous' as the Laver–Shepsie PA theory would have it, but instead conform fully with the compromises implicit in any policy deal that underpins the formation of a new government, *quite regardless of what the substantive content of this policy deal might be*. The question at hand is thus the one posed succinctly in the title of Warwick's original article in this *Journal*, 'ministerial autonomy or ministerial accommodation?'

Our critique of this approach takes several forms. First, there is an implicit assumption by Warwick that the negotiations and bargaining which precede the formation of a government are autonomous and exogenous. We take a different view, namely, that the negotiations and bargaining of government formation *are predicated on what the actors expect to happen if this or that deal is finalized*. This, in turn, means that the talking, posturing, threatening, promising and dealing that constitute the exciting stuff of

<sup>1</sup> Michael Laver and Kenneth A. Shepsle, *Making and Breaking Governments* (New York and Cambridge: Cambridge University Press, 1996); Paul Warwick, *Government Survival in Parliamentary Democracies* (New York and Cambridge: Cambridge University Press, 1994).

<sup>2</sup> Paul V. Warwick, 'Ministerial Autonomy or Ministerial Accommodation? Contested Bases of Government Survival in Parliamentary Democracies', this *Journal*, 29 (1999), 369–94.

<sup>3</sup> Michael Laver and Kenneth A. Shepsle, 'Understanding Government Survival: Empirical Exploration or Analytical Models?' this *Journal*, 29 (1999), 395–401.

<sup>4</sup> Michael Laver and Kenneth A. Shepsle, 'Events, Equilibria, and Government Survival', *American Journal of Political Science*, 42 (1998), 28–54.

<sup>5</sup> Paul V. Warwick, 'Getting the Assumptions Right: A Reply to Laver and Shepsle', this *Journal*, 29 (1999), 402–12.

government formation are themselves *endogenous*. Put differently, the relevant actors entertain conjectures (that is, maintain a private theory in their heads) about what would happen if this or that allocation of portfolios and this or that coalition agreement were to be instituted. The thing to which Warwick has ministers ‘accommodating themselves’ would, in our view, have been assembled in the first place with an eye to what these partisan ministers could be expected to do once in office. Our first point, then, is that forecasts about the future policy behaviour of putative cabinet ministers drive the bargaining over the allocation of portfolios and a coalition agreement on policy. Warwick, however, thinks (implicitly at least) of the bargaining and deal making as autonomous and exogenous influences on subsequent policy formation and implementation. (In a related matter, Warwick narrows bargaining and compromise to things that happen within policy jurisdictions, whereas our approach enlarges the notion of compromise to include horse trading *across* jurisdictions – ‘You get extraordinary influence in this ministerial jurisdiction and I get it in that one.’)

Secondly, even after having said this, we would surely concede that ministers are not automatons – that the assumption of ‘ministerial autonomy’ is but a provisional and convenient fiction. Recall that when we wrote our book, theories of government formation rarely mentioned ministers at all, except to treat portfolios as a sack of trophies to be allocated. Portfolio allocation was thought of not as something that affected subsequent policy formation and implementation, but rather as a form of side payment to compensate those who might otherwise be disappointed by the policies of the government, policies formulated by the *legislature* and described in the coalition agreement. The government, as such, never figured much in models of government formation. So, we took something of the opposite tack, suggesting just maybe that the composition of the government was a crucial part of government formation! In *Making and Breaking Governments* we were committed to the belief that the identity of these ministerial agents does matter – they were not ‘merely’ unhappy politicians compensated by office perks for their policy disappointments, but more likely were significant movers and shakers on policy in their respective jurisdictions. The particular modelling assumption we made was one that allowed us to see how far (an extreme version of) this intuition would take us. We were never making a descriptive claim. And we would expect scholarly advances over our initial effort to take the form of refining the ‘ministerial autonomy’ assumption, *not accepting or rejecting it as Warwick thinks should be done*.

Thirdly, and here we get to the philosophical heart of the dispute, our view is that Warwick needs to write down a model of government formation *in which he substitutes for our ministerial autonomy assumption one that is more to his liking*. We can then (and only then) see, by teasing out implications from this revised model, exactly what Warwick’s proposal entails. If Warwick seeks a contest, it should be one in which implications from these two different models are pitted against each other empirically (nesting them appropriately in some empirical specification). This is what we mean when we say that ‘like should be pitted against like’. By doing this, of course, Warwick would have to confront a number of issues that now remain suppressed and implicit (and which may thus very easily be glossed over). For example, Warwick’s assumption of ministerial accommodation would require him to ask, ‘accommodate to what?’ – that is, where did the deal come from? – and thus force him to consider exactly how coalition bargaining could take place without any forecasts of subsequent policy implementation. (In passing we should note that the reason we were forced to pit our model against random outcomes – something which Warwick criticizes as inconsistent with the spirit

of ‘pitting like against like’ – as necessitated by the fact that there were, and still are, *no other government formation models dealing with portfolio allocation*. So there is no ‘like’ against which to pit our implications.)

In sum, while we wholeheartedly agree with Warwick that ‘the real task is to determine which associations are causal and which are not’,<sup>6</sup> we very firmly believe that this task cannot be accomplished in the absence of a theoretical model. Causal relations are not matters simply for statistical evaluation; they are logical statements of necessary and sufficient conditions for the existence of a given state of affairs.

Since the philosophical gulf between us on this matter is so wide, a point-by-point rebuttal of Warwick’s latest set of comments would not advance matters much. We can only argue as strongly as possible that Warwick should begin his analysis of the important matter of government survival with a theory, write down an explicit model of the process under investigation, ensure that the assumptions driving this model are explicit, and then tease out implications of his model that can be confronted with data and compared with the alternatives. Then we will be able to compare like with like.

<sup>6</sup> Warwick, ‘Getting the Assumptions Right’, p. 403.