

debate on the future of comparative politics: a rejoinder

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Abstract

The analysis of similarities does not involve meaningless description but rather the more systematic use of Most Different Systems Designs, temporal variation and large-scale comparative designs. Parallel to this the quantification of comparative politics should progress further. Congruence analysis between data and competing theories can be a (or the) solution with $N = 1$, but cannot be applied in settings with $N > 1$ without risks of selection bias and limitations to multiple causation.

Keywords rationality; congruence analysis; selection bias; quantification

If Darwin was a stamp collector, then fine, I am all in favour of it. But it is too big a question for me to discuss which scientific model comparative politics (let alone political science or the social sciences more generally) should adopt in the future. My goal in the original paper is much more modest, in fact, and is concerned with a number of *limitations* in the way many (and indeed my own) comparative research designs are set up. In this regard – and even allowing for distortions, exaggerations (and omissions) for the legitimate sake of the show – Schneider (2010) does heavily misrepresent my argument. But as it would be boring to reiterate what I

actually write in the paper I leave it to the readers to go back to it if they think it is worth the while. Of course, in engaging in a debate of this sort one also accepts to end up speaking about what the discussant thinks is important. Those are the rules of the game I suppose. So let's play it and address Schneider's points. I will then come back to the actual debate and add something to Haverland's (2010) substantial response.

What Schneider underestimates is how close my views are to his. And the point is *not* variation. It is a waste of time and paper to claim that anyone wants to 'attack' variational analysis and take comparative politics towards the analysis

of similarity. Does one really have to stress that (co-)variation remains the backbone of social sciences and that it is no-one's *bête noire*? I thought I claimed quite clearly that we need *more* difference (Most Different Systems Designs (MDSD)) and to move away from grounded theories with marginal variation. Schneider dismisses scornfully designs based on similarity because he confuses similarity in outcome with Most Similar System Designs (MSSD).¹ Yet there is a great deal of excellent (variational) research analysing similar outcomes, in which the dependent variable is a constant (for example, the occurrence of revolutions or civil wars), but in which the cases are different on a large number of properties. Such similar outcomes are far from being 'inexplicable', as Schneider claims. And they are far from being descriptive. These are variational designs based on empirically testable hypotheses about relationships between a set of independent variables and a dependent one. Under these circumstances, there is nothing 'senseless' or 'theory-free' in the way in which case selection is done and in the way in which data are collected in designs stressing similar outcomes (and, for that matter, simultaneous change over time). But again, I better refer to my original paper.

The other point raised by Schneider is *rationality*. For a start, I find it unfair to claim an exclusive use of rationality for one's approach. I tend to regard myself too as a 'diehard rationalist' (last time I checked this was not the preservation of game theoretical modelling). Theories are not pets but tools to be used if deemed useful to address a research *problem*. In my book on *The Nationalization of Politics*, for example, I make the point (and I think that the data support it) that parties are vote maximisers spreading through territories to increase their electoral support, independently from socio-economic and institutional (electoral systems)

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structures; a competitive pattern in the geographical space that later takes place in the ideological space and that is described by Downs. It is competition that drives nationalisation.² I am also fine with *assuming* rationality, as I do not think that political science should necessarily be concerned with the motivations of human behaviour – whether it is rationality, instinct or social convention (I would tend to leave this question to experimental sciences such as psychology and biology).³ Of course it makes perfect sense to assume that human beings are rational (and no approach should have a monopoly of this assumption). But the point here is rather: *what use is this in comparative politics?* What strikes me is that *rationality is not a variable* that can be inserted in variational designs. In designs aiming at explaining differences among countries, rationality is a constant.⁴ In other types of designs, too, where cases are institutions, groups or organisations, rationality is not a variable. This would amount to saying that some countries or organisations are more/less rational than others or differently rational.⁵ Therefore, in variational designs, we need to introduce a number of variables to account, under the same assumption of rationality, for different outcomes. Such explanatory factors are socio-economic, institutional, cultural and so forth. The question therefore is: how do we insert rationality in cross-country or cross-organisation variational designs? In my own work, I have been confronted with this problem. In spite of rational considerations being at the centre of the

'How do we insert rationality in cross-country[...] variational designs?'

competitive behaviour of parties in their process of nationalisation (everywhere), at one point I had to account for quite significant differences between countries in the levels of nationalisation. Again rationality not being a cross-country 'variable', the independent factors explaining differences were economic, institutional and cultural. Although rationality can explain a general mechanism leading to nationalisation, it cannot explain why it takes place to different degrees in different countries (and in different party families).

The point with rationality is not that it is an explanatory variable, but that it is associated with *hypothetico-deductive theory-building that leads to rigorous explanatory statements empirically testable also in the absence of variation*, indeed in the absence of more than one case. As Haverland (2010) notes, they can lead to explanatory statements in designs with $N=1$. Here again rationality has little role to play. But it is true that models that go under the label of rational choice tend to be rigorous, testable and deductive statements from which a large number of inductive comparative studies could learn a great deal. The basic point that Haverland makes and that should be taken seriously in comparative politics is that variation among cases is not the only route to scientific explanation and that what he calls *congruence analysis* (pattern matching) provides an alternative to variational designs for research questions in which variation is absent because $N=1$. It is therefore the 'degree of fit' between cases and theory (rather than the association between co-varying values) that is at the heart of this alternative. In this regard the piece by Haverland is useful. Should comparative politics move more decidedly towards the test of the congruence between empirical observation and the deductive implications of theoretical models? If yes, we would have a convergence between comparative politics on the one hand, and

international relations and European studies on the other.

I find this debate fruitful and would add four short remarks only:

- First, I am unsure whether or not it is possible to equate (as Haverland does) – and therefore to apply indistinctly to both – designs in which $N=1$ (case study) and designs in which $N>1$ but with a constant value in the dependent variable. This distinction is relevant as, if $N>1$, one still needs variation to test hypotheses (MDS) for which the dependent variable is a constant. Furthermore, if it is the case that congruence analysis applies also to designs with $N>1$, then I fear it does not allow for multiple causation.
- Second, I wonder if there is some risk involved in this alternative strategy in the way in which cases are selected. The risk is dependent upon the extent to which empirical observations are treated merely as supporting material of a theoretical deductive model of which one simply seeks as wide an empirical confirmation as possible. In designs with $N=1$ this is not a risk (there is no selection bias in picking the only existing occurrence of a phenomenon).⁶ Also, risks of selection bias are reduced when the 'fit' of empirical evidence is evaluated against two or more competing theories. But in designs with $N>1$ this can lead to looking deliberately for cases that confirm the theory, which is probably equivalent to selecting on the dependent variable.
- Third, before deciding which alternative explanatory strategy to employ, one needs to make sure what degree of

variation there is, and this is a descriptive task, not a deductive-theoretical nor a variational-confirmatory one.

- Fourth, I suspect that since with congruence analysis we are again looking for confirmation (or 'fit'), it lessens our potential to discover the unexpected and therefore does not replace the role of a descriptive phase of analysis.

Be that as it may, at this stage I hand over to the methodologists. I have tried to provide an input from the perspective of a comparativist whose work has been mainly concerned with variational analysis. My original paper in this debate is a critique of what people like myself do in the first place, certainly not a plea to impose any cure. The reason for discussion is to question what we do, as individual scholars and as a discipline as a whole. This is not being 'conservative'. What seems conservative to me is to say that what we have done has taken us a long way (which no one contests) and we should therefore keep on doing it. The reference to the behavioural age is not meant to be 'nostalgic'. On the contrary, it refers to *new* possibilities for comparative analysis in an age of *unprecedented*

technical, statistical and archival progress that behaviourists would not even have dreamt of. This is a huge opportunity. In such a *new* setting, I do not see deductive theorising and 'stamp collecting' as mutually exclusive.⁷ To improve the latter does not mean to neglect the former, nor separate the two. I do however believe that the balance should move from limited 'puzzles' or 'games' to broad research *programmes*. If economics is really the model we want to follow, then let us not forget that the advantage that discipline may have over ours is not (just) methodological or theoretical, but mainly empirical (data). And here I must state again my personal preference for a stronger push towards *quantification* in comparative politics. But there is no wish (and neither would there be the authority) to impose any view (let alone force anyone into 'straightjackets' as Schneider fears). I (and I am sure I can speak for van Kersbergen as well) do not propose any 'super-pill' to be forced down the throat of comparative politics. Rather, I propose to increase the *awareness of certain side-effects* and, in this sense, my paper is nothing more than a package insert.

Notes

1 Also Haverland (2010) worries that the MDS cannot be squared with a dependent variable that does not vary. That is not correct. In fact, most potential independent variables vary in this design (Method of Agreement) and therefore it is a design that demands variation to eliminate, and control for, possible causes.

2 Of course, I embed all this in a 'Rokkanian' approach. I invite everyone to read Rokkan, however, as I think many would be surprised to see how much importance this author attributes to choices, preferences, strategies, interactions, alliances, etc. of the different actors (be it parties, churches or 'nation-builders').

3 In this sense, I regard myself as a 'diehard Durkheimian'. I should add that I have no problems in assuming rationality in individuals, not so for collective actors. Moreover, I should point out that I am deliberately leaving aside the discussion about the origin of preferences, which goes beyond the scope of rational choice.

4 Or, if the specifications of micro-foundations are wished, a trivial necessary condition (like gravity) in the sense defined by Braumoeller and Goertz (2000).

5 This may be possible but again it would need to be explained in the light of, for example, different cultural or biological features, geographical or geological factors such as distribution of resources, as well as social-institutional structures.

6 I leave aside the usually associated point of 'process tracing', which implies that the method of pattern matching and congruence analysis is supported by several observations (over time) even when the number of 'cases' is one. This would lead to a too lengthy discussion of what 'case' is.

7 Schneider actually distorts this quote which, originally, was not meant critically against approaches not confirming physics' model, but rather in the sense that most of science consists anyway of tedious labour. This reminds me of Salvador Dali's remark that only 1% of an artist's creation is 'genious' and that the rest is something equivalent to stamp collecting – and that thinking differently was either pretentious or lazy. I tend to believe that this applies to science in very much the same way.

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About the Author

Daniele Caramani is Professor of comparative politics at the University of St. Gallen, Switzerland. He is the author of *The Nationalization of Politics* (Cambridge University Press, 2004), for which he received the 'Stein Rokkan Prize'. He has also authored *Introduction to the Comparative Method With Boolean Algebra* (Sage, QASS, 2009) and edited the textbook *Comparative Politics* (Oxford University Press, 2008).