SYMPOSIUM

tracing the progress of process tracing

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Abstract
Building on a Lakatosian approach that sees Social Science as an endeavour that confronts rival theories with systematic empirical observations, this article responds to probing questions that have been raised about the appropriate ways in which to conduct systematic process analysis and comparative enquiry. It explores varieties of process tracing, the role of interpretation in case studies, and the relationship between process tracing and comparative historical analysis.

Keywords process analysis; interpretation; causal inference; Lakatos; theory; case studies

Recent years have seen growing interest in methods that use process tracing to investigate causal relationships in the political world. While those methods offer many advantages for making causal inferences, they also raise issues with which scholars are only beginning to grapple. The methodologists to whose queries I am responding are at the forefront of these developments: each is writing a book about case studies or process tracing. They ask important and challenging questions, to which it is impossible to give adequate responses in a short space. Therefore, the major contribution of this symposium lies in identifying the questions, and my responses should be read as tentative efforts to grapple with them. The rationale for them will be more apparent if I begin by outlining some of the premises from which I approach research in Political Science.

From time to time, all political scientists should ask the fundamental question: what are we doing when we do Social Science? On this issue, my views are deeply influenced by Imre Lakatos (1970) who argued that Science is a project that entails formulating theories and examining them in the light of empirical observations. Those theories may be of greater or lesser generality. To take a very general example, we might be arguing that political outcomes are driven by a contest over material resources, but usually we are examining theories of greater specificity. We might

be arguing, for example, that states are more likely to democratise when income inequality increases or decreases (cf. Boix, 2003; Acemoglou and Robinson, 2005). The key characteristic of a theory is that it does not simply identify an empirical regularity, but adduces reasons as to why this empirical regularity should exist, and setting out those reasons usually entails outlining causal mechanisms associated with the phenomenon at hand (Waltz, 1979). All theories also generate a subset of more specific 'propositions' about empirical regularities in the world and, from them, we deduce hypotheses – which I define as statements about what we should see in the data if those propositions are correct and if they are false.

Thus, at the heart of Social Science is a confrontation between our theory, on one side, and empirical observations, on the other. Moreover, after we have gathered observations against which to test a theory, the results are almost always ambiguous. Our observations usually tend to support the theory in some ways, but incline us against it in other ways. This ambiguity poses the core challenge at the heart of Social Science – the challenge of making a judgement about whether to reject our theory as false or to reject our observations as inadequate for testing the theory. Hence, the well-known saying 'never let one ugly fact get in the way of a beautiful theory' and debates about methodology, because methodology is all about finding better ways of making this judgement.

Influenced by Kuhn's (1970) argument that our observations are always conditioned *a priori* by existing theories, Lakatos also argues that, in order to make this judgement, we cannot simply examine our own theory, but must consider it in the light of at least one other rival theory. Comparing the fit between our observations and two different theories makes it easier to judge whether that fit is tight enough to justify accepting one of those theories, and it takes a theory to defeat a theory, because we ultimately see the world only through a theoretical lens. Thus, Lakatos describes the scientific endeavour as a 'three-cornered fight' between observations, a theory and a rival theory, and this confrontation is central to the difficult judgements every social scientist must make when she sits down to decide what conclusions to draw from observations that are always to some extent ambiguous.

In this context, it is also useful to pose a second foundational question, asked all too rarely even by methodologists. Political Science is about explanation. Description is crucial to the enterprise and more important than some acknowledge, but what our field really prizes is good explanations for political outcomes. Therefore, it is fitting to ask: what does a 'good' causal explanation for social or political phenomena consist of?

In Hall (2006), I identify three kinds of answers that can be given to that question, without claiming that no other viable answers exist. One approach to explanation is historically specific. Scholars who take this approach seek to explain an outcome in only a few cases, and they usually see a good explanation as one that provides a full account of the causal factors leading up to that outcome with some indication of their relative importance. A second approach to explanation might be described as multivariate. On this view, a good explanation consists of precise estimates for the impact of a small number of variables on a general class of outcomes, ideally with confidence intervals attached to them. A third approach is theory-oriented explanation. Its proponents see a good explanation as one that elaborates and tests a theory identifying the main determinants of a broad class of outcomes along with the causal mechanisms associated with those determinants. This list covers much of the work done in Political Science.

In this context, Joachim Blatter asks about the value of distinguishing between alternative approaches to process analysis based on the distinction between 'causal process tracing' and 'congruence analysis'. There are certainly arguments for drawing such a distinction, but much depends on just what it comprehends. On my reading, what Blatter and Blume (2008a, b) describe as 'causal process tracing' resembles the influential approach to 'process tracing' that George (1979) advocated in pioneering articles (cf. George and Bennett, 2005; Campbell, 1975). However, despite the sophistication of George's formulations, many scholars, sympathetic to, or critical of, process tracing, saw this as a technique that supplies a narrative account of the cases without providing a clear basis for generalising beyond them. On this point, Blatter and Blume are more restrictive than George. They note that the principal objective of 'causal process tracing' is to explain the outcome in the small number of cases being examined rather than to generalise beyond them to a larger universe. Thus, the 'causal process tracing' outlined by Blatter and Blume is oriented towards what I have called historically specific explanations.

There is certainly some value in such explanations, but I think this is not what most political scientists want to do. For the most part, they seek explanations based on generalisations that extend beyond the cases at hand – what Donald Campbell once called 'portable truths' (Campbell, 1975) – and, in order to secure such generalisations from one or two cases examined in depth, I believe we need to adopt the self-conscious approach to testing theories that I labelled 'systematic process analysis' in Hall (2003, 2006). Indeed, I adopted that somewhat awkward label to avoid any confusion with looser interpretations of process tracing that associate it with a largely narrative analysis. Thus, as I understand it, systematic process analysis is attentive to the general causal inferences that can be drawn from observing the sequence and timing of events and contemporary interpretations of those events. From this perspective, the key difference between 'causal process tracing' and 'congruence analysis' thus turns on the type of explanation each generates rather than on the attention each gives to issues of temporality. From this, it follows that systematic process analysis and perhaps 'congruence analysis', as Blatter understands it, are more promising approaches to political analysis than 'causal process tracing'.

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This takes me to the questions about causal mechanisms posed both by Blatter and by Derek Beach. Debate about causal mechanisms has been a 'growth field' in Political Science with salutary effects. As a result of that debate, political scientists are much less likely to confuse correlation with causation than we were a few years ago, and the new importance accorded causal mechanisms has done much to inspire research designs deploying mixed methods. As Beach's questions indicate, however, there is great uncertainty about precisely how such mechanisms should be understood, and one can find in the current literature well over a dozen different definitions of a causal mechanism (cf. Gerring, 2007; Gross, 2009; Mayntz, 2004; Tilly, 1995).

My views about this are still evolving and I am reluctant to endorse a specific definition of a causal mechanism. For the most part, however, I am sympathetic to the position taken by Beach. As he notes,

some scholars see causal mechanisms as intervening variables, more or less as *w*, where *x* causes *y* because *x* causes *w* and *w* causes *y*. But like him, I would insist that a causal mechanism is not a variable or a list of variables. It is a theoretical formulation, one that adduces properties of the relationships among phenomena with the potential to recur, which helps explain *why x* causes *y*. The mechanismic approaches that Beach cites are congruent with this definition, although they may not be the only viable ways of conceptualising mechanisms. Thus, while a mechanism might embody what we can describe as intervening variables, it is not reducible to a list of them. It is a statement about why those intervening variables and the processes or emergent properties associated with them are important to the outcome.

To take a simple example, Paul Pierson (2000a) and others have argued that, once social programmes are in place, it is difficult for governments to eliminate them, and, in order to explain why this is the case, they specify various kinds of causal mechanisms. Thus, Pierson contends that social programmes often create a sense of entitlement among their beneficiaries, which stiffens the resistance of the latter to cuts in those programmes. This is a theory that identifies a causal mechanism associated with the 'sense of entitlement', and in principle it is possible to test whether this mechanism is operative in any particular case. We could look to see if beneficiaries vote against governments that cut their benefits more often than other people vote against those governments, and we could ask whether beneficiaries regard their benefits as an entitlement. Another causal mechanism often cited in such work turns on the network effects of social programmes. On the premise that an existing pension scheme will remain in place, for instance, people make certain kinds of other investments for their retirement. Therefore, they will resist changes to this pension scheme that would reduce the value or appropriateness of such investments. Again, this is a causal mechanism that alludes to clear-cut variables, without being entirely reducible to them, and it is possible to assess empirically whether it is present or absent in any given case.

Of course, this is a relatively basic view of causal mechanisms that does not treat some of the complex issues Beach raises. But some interesting implications follow even from this simple perspective. One is that the operation of a causal mechanism always depends on the presence of a specific set of initial conditions (Falleti and Lynch, 2009). Second, while these mechanisms may turn on rational calculation, they need not do so. Here, my views differ from those of scholars who insist on seeing causal mechanisms in terms of rational action at the individual level (cf. Hedström and Swedberg, 1998). The third implication is that we can often use systematic process analysis to assess the adequacy of the causal mechanisms in our theory, by examining the course of events to see if it has the shape to be expected if those mechanisms are present.

The interesting question Blatter asks about whether 'interpretation' has a role to play in systematic process analysis is relevant here. Again, this is an issue with complex ramifications that I will treat more briefly than it deserves. Much depends on what we mean by 'interpretation'. Consider two rather different definitions. The first conforms broadly to some of the ways in which interpretation is used in cultural studies and anthropology. Here, interpretation is an iterative process of observation and conjecture whose goal is to identify an intrinsic coherence to behaviour or events that is not immediately visible to the observer. We might be interpreting the practices of governance in seventeenth century England as the reflection of debates in ancient stoic

philosophy or responses to the 'war on terrorism' as reflections of the ontological anxiety of modernity (Giddens, 1991). Here, the capacity of the enquiry to identify connections between events that are not immediately visible to the observer is a key criterion by which the success of such interpretations is judged.

There is real value in this sort of enquiry. But my formulations construe 'systematic process analysis' in terms deliberately designed to indicate that it does not entail this kind of interpretive enquiry (Hall, 2006). Instead, I see systematic process analysis as a relatively positivist mode of enquiry, focused on hypothesis testing that uses systematic empirical observations to accept or reject propositions drawn from an overarching theory. Of course, the dividing line between this type of positivist enquiry and interpretation is never entirely sharp. Regardless of the methods they deploy, political scientists are ultimately painting portraits of the world, and the variables in all their analyses reflect interpretations of that world. However, the key distinction here rests on the canons used to judge the adequacy of an analysis. If the quality of an interpretation turns in large measure on the quality of the resonances it summons up and the underlying coherence it draws out of seemingly unrelated events, the quality of most positive enquiries turns on how well their propositions about causal regularities measure up against empirical evidence that is systematically obtained and could, in principle, falsify them. While this formulation sidesteps some of the important challenges 'critical realism' poses to positivist enquiry, as I define it, systematic process analysis is largely a positive project (cf. Sayer, 1992).

However, consider a second way in which 'interpretation' can be defined. Here, interpretation is associated with Weber's (1949) method of *Verstehen* and, in particular, with minimalist versions of it which specify that good explanations of actions have to be compatible with the meanings the actors themselves associated with those actions. On this view, we cannot explain the English civil war as a struggle for material resources if the key actors were largely unconcerned with material resources and saw it mainly as a conflict over religion. In this sense of the term, interpretation has to be part of systematic process analysis. That is not to say, as a maximalist understanding of Weber's position might imply, that, once we have identified the meanings actors attribute to their actions, we have fully explained those actions; however, I do not think we can explain most actions (or identify the causal mechanisms associated with them) without reference to such meanings.

The implication is that for explaining political outcomes, political scientists should not rely nearly as much as they do on highly stylised models that deliberately avoid realistic depictions of how the historical actors thought or acted. Of course, as Friedman (1968) has noted, all theories offer simplified representations of the world. But his conclusion that theories should, therefore, be assessed only according to the accuracy of their predictions about ultimate outcomes seems unduly limiting. Given how intrinsically difficult it is to form accurate judgements about whether a theory is true or false, I think we should use all available evidence to make those judgements, including evidence about whether the theory's assumptions about the motivations and perceptions of the actors are realistic.

Ingo Rohlfing raises some interesting questions about 'comparative historical methodology' as outlined most prominently by James Mahoney (2004). No doubt Mahoney could respond more knowledgeably, but my own views are congruent with those of Rohlfing. I do not see large differences between the

methods suitable for analysing case studies and those suitable for comparative historical research. Those who associate a distinctive method with comparative historical research typically do so in order to contrast it with statistical methods rather than those of small-N research (Mahoney, 2004; Mahoney and Terrie, 2008). In my view, comparative historical analysis is distinguished primarily by its subject matter rather than by the application of a distinctive method.

Moreover, I have misgivings about some of the features occasionally said to be distinctive of comparative historical methods. One is the suggestion that comparative historical analysis focuses on providing comprehensive explanations of outcomes in specific cases rather than on generating and assessing theories of wider applicability. On my reading, such landmark works as Skocpol's (1979) *States and Social Revolutions* or Moore's (1966) *Social Origins of Dictatorship and Democracy* have value precisely because they generate theories and theoretical perspectives that can be applied to a wide range of cases, and I would hesitate to describe either as an effort to provide a 'comprehensive' explanation of the outcomes at hand, especially in comparison with many historical works about those cases. In an important early contribution to such debates, Skocpol and Somers (1980) make similar points.

Much the same might be said of the claim that comparative historical methods look for the necessary and sufficient conditions behind political outcomes rather than causal propositions couched in more probabilistic terms. This has become a complex issue now that various kinds of probabalism have been associated with necessary and sufficient conditions (Mahoney, 2010). But I see no reason why comparative historical research should not also look for the types of probabilistic propositions for which other kinds of enquiries often strive. Of course, it is impossible to establish the precise level of probability to attach to a causal proposition via research based only on a small number of cases. For that purpose, if practicable, a large-*N* statistical analysis is often more useful. However, by examining 'crucial' cases in which the relevant causal effects are *ex ante* least (most) likely to be found, the small-*N* researcher can often assert with some confidence that they are probably (not) present in other cases as well (Eckstein, 1975).

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By contrast, although one can usually use a small-*N* research design to infer that a particular set of factors are sufficient to cause a given outcome, such designs do not tell us whether other combinations of factors are also sufficient to cause it, and they do not provide a strong basis for inferring whether those factors constitute a necessary condition for that outcome. Reaching sound judgements about necessity, in particular, requires a large or medium-*N* analysis using some form of qualitative comparative analysis or QCA (Ragin, 2000). Thus, the common claim that comparative historical research looks for necessary and sufficient conditions turns on the relatively arbitrary decision to see QCA as a method appropriate for such research and most kinds of statistical analysis as inappropriate for it. It would be unfortunate if this stance led us to neglect the value of small-*N* research designs for assessing propositions couched in probabilistic terms.

What are the methodological implications of analyses that draw our atten-

tion to new kinds of causal processes associated with sequencing, path dependence, critical junctures and the like? There is no doubt that our understanding of institutional change is still at an early stage, and there are many ways in which that research programme can be developed. In Hall (2010), I argue that we need to know more about three issues. First, how are 'critical junctures' generated? Second, how do the processes of gradual institutional change that occur between critical junctures condition what happens in those conjunctures? Third, if institutions ultimately depend on social coalitions, how are those social coalitions themselves structured by institutions? Progress is being made on each of these issues (cf. Streeck and Thelen, 2005; Morrison, 2011).

Do we need new methodologies for tackling such questions? This is an issue that deserves more attention. As Rohlfing observes, if we think the timing of an event or the sequence in which it occurs relative to other events conditions its effects, the problem of finding comparable cases in which to assess the impact of such events becomes more difficult (Pierson, 2000b). Thus, there are arguments in favour of seeking new tools for the purposes of such enquiry. Some have found them in QCA and the fuzzy set approaches linked to it, which have been highly revealing about many kinds of historical cases, and especially useful for integrating small-*N* and medium-*N* research designs (Ragin, 2000). They draw attention, for example, to cases of equifinality, in which a specific political outcome is generated, not by a single set of conditions, but by different conditions in different cases. We now understand that there is more than one route to a developed welfare state or a stable democracy. In principle, such techniques can also be applied to the problem of establishing the relevance of timing or sequence to the impact of specific events, by allowing scholars to canvass a wide range of cases in which variation on the relevant temporal dimensions is observable.

On the other hand, many issues of interest to comparative historical enquiry can be illuminated using methods already available, such as comparative case research, statistical analysis and systematic process analysis (Hall, 2003). With process analysis and comparative methods, for instance, Jacobs (2011) has shown why the extension of old-age pensions in Germany, Britain and the United States followed path-dependent processes. We need better theories even more than new methods if we are to develop better understandings of how processes such as sequencing and path dependence work.

However, it is important to note that methodological innovation often follows from theoretical innovation. The efforts of Capoccia and Ziblatt (2010) to define a new approach to the study of democratisation illustrate this point nicely. They develop a new theoretical perspective on democratisation, which emphasises the ways in which episodes of reform cumulate over the long term, and then explore the methodological implications of this perspective. They find new ways in which existing methods can be applied, arguing, for instance, for statistical analysis at the micro level, and then identify some new problems for which our methods may have to be refined, such as the problem of understanding how the actions taken during specific episodes aggregate into more durable outcomes over time.

I conclude with the question posed by Derek Beach about whether it makes sense to use procedures akin to Bayesian likelihood analysis to draw inferences within cases, but standard comparative methods to draw inferences across cases. There is real value in seeing process tracing as a technique akin to Bayesian inference, in which new information is used to update a set of theoretical priors.

The useful point here is that those priors can tell us how informative any particular observation is, thereby helping the analyst specify the kinds of observations to seek in the case and how to judge the import of each of those observations for the theory (Bennett, 2008; Van Evera, 1997). This is a distinct improvement on the standard advice simply to multiply our observations.

The caveat, of course, is that we should not simply change our theories loosely to fit the data as it emerges, lest we subvert the confrontation between theory and observation at the heart of sound enquiry. Instead, successive observations should be seen as a stream of data that gradually enhances or erodes the credibility of a theory. That is why I have some qualms about the 'analytic narratives' advocated by Bates *et al* (1998) (cf. Elster, 2000). Using that method, the analyst alters her theory in successive iterations to fit the emerging observations. Thus, when this method is used, the element of constraint required to discipline the Lakatosian 'confrontation' turns heavily on the brittleness of the relevant theory, that is, there must be some point at which discrepant observations lead, not to amendment, but to the conclusion that the theory is false. For Bates and his collaborators this approach makes some sense because they are interested in testing a specific set of rational choice theories. But I am sceptical about whether those theories are brittle enough to provide the requisite constraint, and that problem is then magnified if they are not tested against rival theories.

These considerations highlight a general problem to which the field has not yet devoted enough attention, namely the issue of how to integrate 'inductive' and 'deductive' elements of enquiry. If the former refers to a process in which propositions emerge from empirical observation, the latter refers to a process in which propositions are deduced from more basic premises. The traditional positivist argues, of course, for a sharp separation between the two. Theories are formulated in advance via processes of deduction and then tested against observations. If inspection of the initial observations leads to some revision in the theory based on induction, it is often said that the theory can then be accepted as valid only if it is retested against a new set of cases (King *et al*, 1994).

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In practice, however, political scientists often refine their theories in the light of initial observations, without retesting them against new sets of cases. This is a common feature of statistical analysis, where scholars often re-specify their estimations to explain the variance better, in effect refining their theory in the light of early research results, without retesting it on new cases. But the practice is also a frequent one in case study research. Indeed, there are arguments for it, as yet underappreciated by the literature on methods. A little induction can often vastly improve a deductive theory, saving time and resources on what would otherwise be useless tests. To what extent, however, can induction be used to improve deduction before the scientific validity of one's research is damaged? Are there techniques for using induction to refine a theory that retain the requisite tension between theory and observations? These issues deserve more attention from the field.

In much the same way, we need to think more deeply about how best to compare cases that have been the object of process

tracing and how to select cases when process tracing and comparison will both be used in the enquiry. The standard view, which Beach sensibly endorses, is that, even when we apply process tracing within the cases, we should apply the standard comparative method across cases, looking for co-variance in the dependent and key explanatory variables. But process tracing gives us more information about cases than the comparative method typically exploits, and we should ask whether that information can also be used for purposes of cross-case comparison.

If process tracing reveals important causal mechanisms within the cases, for instance, we should be using comparison across the cases, not only to look for co-variance in dependent and explanatory variables, but to look also for cross-case variation in how those mechanisms work. Findings from the comparative enquiry could then be used to assess some key components of the causal theory and, where discrepancies across cases are found, the adequacy of the theory and of the process analysis conducted in specific cases scrutinised. As Mahoney (1999) points out, this is what Skocpol (1979) does in her comparative study of *States and Social Revolutions*, and it is reflected in the approach Thelen (2004) takes to the evolution of institutions for skill formation. For the most part, however, their theories emphasise the commonalities in processes found across the cases. We might imagine going further to see if our theories can be refined to account, in systematic terms, for case-specific variations in what is otherwise a common causal process, as Lipset *et al* (1956) do, for instance, in their classic study of *Union Democracy*. In short, the use of process tracing in small-*N* studies has implications for how best to conduct and exploit comparison across cases that are yet to be explored.

The interest that political scientists are now showing in process tracing and causal mechanisms is having a profound impact on the discipline. Process tracing is now used more reflectively and more frequently in small-*N* studies. As a result, 'qualitative' enquiry has been revitalised, and many scholars are being drawn to mixed-method research designs, as scepticism rises about whether statistical analysis alone can reveal causal relationships. These developments have inspired, in turn, a new interest in critical realism, which now leavens the positivist dough out of which most enquiries in Political Science have been baked. Because the strengths and weaknesses of process analysis and statistical analysis are often mirror opposites, these mixed methods may well yield better causal inferences. The caveat, of course, is that combining one imperfect technique for drawing causal inferences with another imperfect technique does not yield perfect causal inferences. But, given how difficult it is to arrive at any certain knowledge of the social and political worlds, there is real value in cultivating a methodological pluralism. In doing so, the field has had to confront some of the methodological issues raised in this symposium, but, out of such debates, a stronger social science is likely to emerge.

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