B.J.Pol.S. **38,** 161–179 *Copyright © 2007 Cambridge University Press*
doi:10.1017/S0007123408000082 *Printed in the United Kingdom*

Review Article: The Mechanismic Worldview: Thinking Inside the Box

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A widespread turn towards mechanism-centred explanations can be viewed across the social sciences in recent decades. This article clarifies what it might mean in practical terms to adopt a mechanismic view of causation. This simple task of definition turns out to be considerably more difficult than it might at first appear. The body of the article elucidates a series of tensions and conflicts within this ambient concept, looking closely at how influential authors have employed this ubiquitous term. It is discovered that 'mechanism' has at least nine distinct meanings as the term is used within contemporary social science: (1) the pathway or process by which an effect is produced; (2) an unobservable causal factor; (3) an easy-to-observe causal factor; (4) a context-dependent (bounded) explanation; (5) a universal (or at least highly general) explanation; (6) an explanation that presumes highly contingent phenomena; (7) an explanation built on phenomena that exhibit lawlike regularities; (8) a distinct technique of analysis (based on qualitative, case study, or process-tracing evidence); or (9) a micro-level explanation for a causal phenomenon. Some of these meanings may be combined into coherent definitions; others are obviously contradictory. It is argued, however, that only the first meaning is consistent with all contemporary usages and with contemporary practices within the social sciences; this is therefore proposed as a minimal (core) definition of the concept. The other meanings are regarded as arguments surrounding the core concept.

> [We] seek to locate and explore particular mechanisms that shape the interplay between strategic actors and that thereby generate outcomes. [We need to] focus on the mechanisms that translate such macrohistorical forces into specific political outcomes. By isolating and unpacking such mechanisms, analytic narratives thus contribute to structural accounts.
>
> Bates, Greif, Levi, Rosenthal and Weingast[1]

In recent years, social scientists have become cognizant of the extent to which implicit understandings of causation condition our understanding of the social world. This realization has generated an outpouring of work on causation and explanation in the various disciplines of the social sciences. A substantial, though little recognized, research domain has been born.[2]

* Department of Political Science, Boston University. This article began as a conversation with Jim Caporaso, who piqued my interest in the topic and offered helpful comments on an initial draft. For additional comments and suggestions I am thankful to David Dessler, Colin Elman, Bernhard Kittel, Macartan Humphreys, Jim Mahoney, David Waldner, Albert Weale and two anonymous reviewers for the *Journal*.

[1] Robert H. Bates, Avner Greif, Margaret Levi, Jean-Laurent Rosenthal and Barry Weingast, *Analytic Narratives* (Princeton, N.J.: Princeton University Press, 1998), pp. 12–13.

[2] For recent reviews, see Henry E. Brady, 'Models of Causal Inference: Going Beyond the Neyman-Rubin-Holland Theory' (paper presented at the Annual Meetings of the Political Methodology Group, Seattle, 2002); John Gerring, 'Causation: A Unified Framework for the Social Sciences', *Journal of Theoretical Politics*, 17 (2005), 163–98; Paul W. Holland, 'Statistics and Causal Inference', *Journal of the American Statistical Association*, 81 (1986), 945–60; Margaret Marini and Burton Singer, 'Causality in the Social Sciences', in Clifford Clogg, ed., *Sociological Methodology*, 18 (American Sociological Association, 1988), pp. 347–409; Vaughn R. McKim and Stephen P. Turner, eds, *Causality in Crisis? Statistical Methods and the Search for Causal Knowledge in the Social Sciences* (Notre Dame, Ind.: Notre Dame Press, 1997); Judea Pearl, *Causality: Models, Reasoning, and Inference* (Cambridge, Mass.: Harvard University Press, 2000); Fritz K. Ringer, 'Causal Analysis in Historical Reasoning', *History and Theory*, 28 (1989), 154–72; Michael E. Sobel, 'Causal Inference in the Social and

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To be sure, at the birth of the contemporary disciplines of anthropology, economics, history, political science, psychology and sociology, philosophical discussions of causation were not uncommon. For Comte, Hume, Keynes, Mill and Weber the analysis of social problems necessitated a corresponding discussion of what we might mean by calling X_1 a cause of Y. By the mid-twentieth century, however, this abstract species of discussion seems to have dropped out of social-scientific discourse. Authors presented their research question, their method and their results, taking little notice of their assumptions about causation. Physics (i.e., the exploration of the physical world) was neatly separated from metaphysics. One may speculate on the reasons for the latter's disappearance. Presumably, it was more than a little discrediting for an author to expatiate on the nature of causation while conducting an analysis of X_1 and Y. Perhaps the shrinking space, and expanding prominence, of the scientific journal article left little room for such peregrinations. Or perhaps it was deemed unnecessary, given the general agreement on such matters that seems to have obtained at the high watermark of behaviouralism.

In any case, it is a notable feature of the current methodological ferment that causation has re-entered the discourse of social science. Researchers are reading (or at least citing) philosophy of science, and this vocabulary now shapes mainstream academic debates. One hears of deterministic and probabilistic causes, of regularity theories, and – occasionally – of 'supervenience' or 'molar causes' (causes that operate at a structural level). This newfound interest in epistemology concerns the practical question of how to define and to test causal relations. Rather than simply doing it, social scientists are now thinking hard about what causation means.

Among the many extant debates, one intellectual trend stands out: the movement towards a mechanismic understanding of causation.[3] Following directly from Aristotle's

(F'note continued)

Behavioral Sciences', in Gerhard Arminger, Clifford C. Clogg and Michael E. Sobel, eds, *Handbook of Statistical Modeling for the Social and Behavioral Sciences* (New York: Plenum Press, 1995), pp. 1–38; William R. Thompson, 'A Streetcar Named Sarajevo: Catalysts, Multiple Causation Chains, and Rivalry Structures', *International Studies Quarterly*, 47 (2003), 453–74; David Waldner, 'Anti Anti-Determinism: Or What Happens When Schrodinger's Cat and Lorenz's Butterfly Meet Laplace's Demon in the Study of Political and Economic Development' (paper presented at the Annual Meetings of the American Political Science Association, Boston, 2002); Alexander Wendt, 'On Constitution and Causation in International Relations', *Review of International Studies*, 24 (1998), 101–17; Christopher Winship and Michael Sobel, 'Causal Inference in Sociological Studies', in Melissa Hardy and Alan Bryman, eds, *Handbook of Data Analysis* (London: Sage, 2004), pp. 481–503.

[3] See Peter Abell, 'Narrative Explanation: An Alternative to Variable-Centered Explanation?' *Annual Review of Sociology*, 30 (2004), 287–310; Christopher H. Achen, 'Toward a New Political Methodology: Microfoundations and ART', *Annual Review of Political Science*, 5 (2002), 423–50; Andrew Bennett, 'Beyond Hempel and Back to Hume: Causal Mechanisms and Causal Explanation' (paper presented at the Annual Meetings of the American Political Science Association, Philadelphia, 2003); Mario Bunge, 'Mechanism and Explanation', *Philosophy of the Social Sciences*, 27 (1997), 410–65; Ruth Berins Collier and Sebastian Mazzuca, 'Does History Repeat?' in Robert Goodin and Charles Tilly, eds, *The Oxford Handbook of Contextual Political Analysis* (Oxford: Oxford University Press, 2006), pp. 472–89; Tyler Cowen, 'Do Economists Use Social Mechanisms to Explain?' in Peter Hedstrom and Richard Swedberg, eds, *Social Mechanisms: An Analytical Approach to Social Theory* (Cambridge: Cambridge University Press, 1998), pp. 125–46; David Dessler, 'Beyond Correlations: Toward a Causal Theory of War', *International Studies Quarterly*, 35 (1991), 337–55; Jon Elster, 'A Plea for Mechanisms', in Peter Hedstrom and Richard Swedberg, eds, *Social Mechanisms: An Analytical Approach to Social Theory* (Cambridge: Cambridge University Press, 1998), pp. 45–73; Alexander L. George and Andrew Bennett, *Case Studies and Theory Development* (Cambridge, Mass.: MIT Press, 2005); Peter Hedstrom and Richard Swedberg, 'Social Mechanisms: An Introductory Essay', in Peter Hedstrom and Richard Swedberg, eds, *Social Mechanisms: An Analytical Approach to Social Theory* (Cambridge: Cambridge University Press, 1998), pp. 1–31; James Johnson, 'Consequences of Positivism: A Pragmatist Assessment', *Comparative Political Studies*, 39 (2006),

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notion of 'efficient' causation, contemporary writers have come to focus on the generative component of causal argumentation and demonstration.[4] Causes are understood to generate – create, change, alter, condition – outcomes. The mechanism in a causal argument is 'the agency or means by which an effect is produced or a purpose is accomplished'.[5] In model-based terminology, it is the causal pathway, process or intermediate variable by which a causal factor of theoretical interest is thought to affect an outcome. Thus: $X_1 \rightarrow X_2 \rightarrow Y$, where X_1 is the exogenous cause, X_2 the pathway(s), and Y the outcome.

This will be regarded as the core or 'minimal' definition of the term, as it is now employed in contemporary social science and philosophy. It reverberates with the Greek *mechane*, 'an instrument for lifting weights' or 'devices or contrivances for doing a thing' and *mechos*, 'the means', or 'the way by which something is expedited'.[6] And it embraces a wide set of alternative usages, as outlined in this article.[7]

It should be noted that this contemporary understanding of mechanism departs dramatically from common nineteenth-century and early twentieth-century understandings of the term, which invoked a *mechanistic* account of the world. In this context, mechanism meant 'the theory that all phenomena can be explained in terms of the principles by which machines (mechanical systems) are explained without recourse to intelligence as an operating cause or principle', or 'the theory that all phenomena (natural, biological, psychological) are physical and can be explained in terms of material changes'.[8] Evidently, to say 'mechanism' in a contemporary context does not mean that one is wedded to a mechanistic causal account modelled on Newtonian physics.

Perhaps the best entrée into contemporary usage of 'mechanism' is by looking at what it is *not*. Nowadays, writers are keen to differentiate between 'mechanistic' and 'covariational' approaches to causation. The latter derives from a certain reading of Hume's work on causation, specifically, his regularity theory, comprehending the causal

(F'note continued)
224–52; Shira Lewin, 'Economics and Psychology: Lessons for Our Own Day from the Early Twentieth Century', *Journal of Economic Literature*, 34 (1996), 1293–1323; James Mahoney, 'Beyond Correlational Analysis: Recent Innovations in Theory and Method', *Sociological Forum*, 16 (2001), 575–93; Renate Mayntz, 'Mechanisms in the Analysis of Social Macro-Phenomena', *Philosophy of the Social Sciences*, 34 (2004), 237–54; Roger Petersen, 'Mechanisms and Structures in Comparisons', in John R. Bowen and Roger Petersen, eds, *Critical Comparisons in Politics and Culture* (Cambridge: Cambridge University Press, 1999), pp. 61–77; Andreas Pickel, *The Problem of Order in the Global Age: Systems and Mechanisms* (London: Palgrave Macmillan, 2006); Wesley C. Salmon, *Scientific Explanation and the Causal Structure of the World* (Princeton, N.J.: Princeton University Press, 1984); Daniel Steel, 'Social Mechanisms and Causal Inference', *Philosophy of the Social Sciences*, 34 (2003), 55–78; Arthur L. Stinchcombe, 'The Conditions of Fruitfulness of Theorizing about Mechanisms in Social Science', *Philosophy of the Social Sciences*, 21 (1991), 367–88; Arthur L. Stinchcombe, *The Logic of Social Research* (Chicago: University of Chicago Press, 2005); Charles Tilly, 'Mechanisms in Political Processes', *Annual Review of Political Science*, 4 (2001), 21–41; Waldner, 'Anti Anti-Determinism'. For discussions of mechanism in the context of the natural science and philosophy of science, see Stuart S. Glennan, 'Mechanisms and the Nature of Causation', *Erkenntnis*, 44 (1996), 49–71; Peter Machamer, Lindley Darden and Carl Craver, 'Thinking about Mechanisms', *Philosophy of Science*, 67 (2000), 1–25; James G. Tabery, 'Synthesizing Activities and Interactions in the Concept of a Mechanism', *Philosophy of Science*, 71 (2004), 1–15.

[4] Mario Bunge, *Causality* (Cambridge, Mass.: Harvard University Press, 1959), p. 33.

[5] *Random House Webster's Unabridged Dictionary* (New York: Random House, 2006).

[6] Peter A. Angeles, *Dictionary of Philosophy* (New York: Barnes and Noble, 1981), p. 166.

[7] A minimal definition must be substitutable for all extant usages of a term within a particular language region. See John Gerring and Paul A. Barresi, 'Putting Ordinary Language to Work: A Min-Max Strategy of Concept Formation in the Social Sciences', *Journal of Theoretical Politics*, 15 (2003), 201–32; Giovanni Sartori, *Parties and Party Systems* (Cambridge: Cambridge University Press, 1976), p. 61.

[8] Angeles, *Dictionary of Philosophy*, p. 167.

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properties of a phenomenon through empirical patterns of covariation ('constant conjunction') between X_1 and Y. If X_1 and Y co-vary, either deterministically or probabilistically, then – so long as possible confounders have been controlled – one has reason to believe that they may be causally related. This view of causation is associated with the deductive-nomological model developed by the logical positivists.[9]

Causal arguments are thus identified as either covariational or mechanismic.[10] Curiously, despite the virulence with which this battle is laid out, there is little contemporary debate. Although covariational methods enlist many practitioners, they do not inspire methodological champions. In this respect, it is somewhat like that closely related empirical school known as behaviouralism, a school of social science without a well-developed philosophy. Now that Hempel and Oppenheim have departed, no active defenders of this particular brand of positivism remain.

Facing no organized resistance, the mechanismic view now dominates discussions of causality. While it is perhaps premature to proclaim the arrival of a new school, there are enough features in common to suggest the existence of a strong intellectual current stretching across most social science disciplines. The turn towards mechanisms embraces a wide variety of methodological and epistemological positions – quantitative and qualitative, experimental and non-experimental, formal and informal, nomothetic and idiographic – and finds philosophical support within the 'realist' school of philosophy of science.[11] These days, interpretivists, case-study researchers, experimentalists, neo-behaviouralists, formal modellers and epistemologists seem to agree on at least one thing (and perhaps only one thing): the traditional focus of social science has been too large, too highly aggregated and too influenced by – often spurious – associations between X_1 and Y. Greater leverage on causal questions can be gained only by scaling down. We need to get inside the box of causation.

In re-crafting the vocabulary of causation, the concept of a causal mechanism has helped draw attention to some notable deficits in the traditional approach to causation. For some writers in the positivist tradition, specifying a theory means little more than the statement of a set of predictions implied by that theory.[12] This is evidently a highly constrained view of the task of explanation. By contrast, the notion of a mechanism encourages researchers to clarify what a theory is all about, i.e., *how* X_1 might be related to Y. It also prompts us to consider the possibility that X_1 causes Y in more than one fashion, i.e., through multiple pathways. Note that in the schema introduced above, X_2 may refer to a vector of intermediate causes or causal combinations. More generally, the increasing popularity of this concept has forced scholars to devote greater care and attention to matters of

[9] E.g., Carl G. Hempel and Paul Oppenheim, 'Studies in the Logic of Explanation', *Philosophy of Science*, 15 (1948), 135–75.

[10] For Hume, this covariational relationship was assumed to be deterministic; later philosophers have usually assumed it to be probabilistic. For our purposes, the issue is extraneous. Although there are other ways of viewing the complex topic of causation (Brady, 'Models of Causal Inference'), the contrast between the generative and covariational components of causation is central to the development of a mechanism-centred view of social science. Thus, I leave other matters in abeyance.

[11] Bunge, 'Mechanism and Explanation'; Rom Harre, *The Principles of Scientific Thinking* (Chicago: University of Chicago Press, 1970); Daniel Little, 'Causal Explanation in the Social Sciences', *Southern Journal of Philosophy*, 34 (Supplement, 1995), 31–56; Daniel Little, *Microfoundations, Method, and Causation* (New Brunswick, N.J.: Transaction, 1998); Peter Manicas, *A Realist Philosophy of Science: Explanation and Understanding* (Cambridge: Cambridge University Press, 2006).

[12] Milton Friedman, 'The Methodology of Positive Economics', in *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953), pp. 3–43.

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explanation and proof. To this extent, the advent of mechanismic thinking is surely propitious.

Yet, in the face of a growing consensus, a cautionary note may be in order. Consider that wherever a great deal of excitement develops over a new term enveloping a wide range of seemingly disparate points of view, there is *prima facie* evidence that some degree of ambiguity exists on the precise definition of the key concept. That is to say, people mean different things when they invoke it. One thinks, for example, of Thomas Kuhn's introduction of the word 'paradigm' in the 1970s, which exercised a peculiar fascination over social scientists, despite its remarkable plasticity.[13] One might also ponder other key terms in the contemporary social science lexicon, e.g., 'interpretivism',[14] 'culture',[15] 'institutions',[16] 'realism',[17] and 'positivism'.[18] These terms continue to excite, though they are difficult to specify. Indeed, the history of social science methodology is characterized by perpetual terminological innovation. One may wonder whether such changes in meaning are always indicative of scientific progress, or are merely examples of 'playing musical chairs with words'.[19] Here, I think the conclusion must be: a little of both.

In what follows, I try to clarify what it might mean in practical terms – i.e., for practising social scientists – to assume a mechanismic view of causation.[20] It is an exercise in clarification, not debunking. As I have indicated, there is much to recommend a mechanism-centred approach. However, if this is the case we are first obliged to state clearly what it means to practise this form of social science. And this apparently simple task of definition turns out to be considerably more difficult than it might appear.

The goal of this short article is, first of all, to get 'inside the box' of causal mechanisms. The body of the article elucidates a series of tensions and conflicts within this well-travelled concept, looking closely at how influential authors have employed this ubiquitous term. It is discovered that 'mechanism' carries at least nine possible meanings within contemporary social science. Some of these meanings are potentially coherent; indeed,

[13] Gary Gutting, ed., *Paradigms and Revolutions: Appraisals and Applications of Thomas Kuhn's Philosophy of Science* (Notre Dame, Ind.: University of Notre Dame Press, 1980); Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, [1962] 1970); Margaret Masterman, 'The Nature of a Paradigm', in Imre Lakatos and Alan Musgrave, eds, *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970), pp. 59–89.

[14] John Gerring, 'Interpretations of Interpretivism', *Qualitative Methods: Newsletter of the American Political Science Association Organized Section on Qualitative Methods*, 1:2 (Fall 2003), 2–6.

[15] Gerring and Barresi, 'Putting Ordinary Language to Work'; James Johnson, 'How Conceptual Problems Migrate: Rational Choice, Interpretation, and the Hazards of Pluralism', *Annual Review of Political Science*, 5 (2002), 223–48; James Johnson, 'Conceptual Problems as Obstacles to Theoretical Progress in Political Science: Political Culture among the "Sociologists" ', *Journal of Theoretical Politics*, 15 (2003), 87–115.

[16] Peter A. Hall and Rosemary C. R. Taylor, 'Political Science and the Three New Institutionalisms', *Political Studies*, 44 (1996), 936–57.

[17] Richard Boyd, 'The Current Status of Scientific Realism', in Jarrett Leplin, ed., *Scientific Realism* (Berkeley: University of California Press, 1984), pp. 41–82.

[18] Jennifer Platt, 'The Social Construction of "Positivism" and its Significance in British Sociology 1950–1980', in Philip Abrams, Rosemary Deem, Janet Finch and Paul Rocket, eds, *Practice and Progress: British Sociology 1950–1980* (London: Allen & Unwin, 1981), pp. 73–87.

[19] Giovanni Sartori, 'The Tower of Babble', in Giovanni Sartori, Fred W. Riggs and Henry Teune, eds, *Tower of Babel: On the Definition and Analysis of Concepts in the Social Sciences* (Pittsburgh: International Studies Association, 1975), pp. 7–38.

[20] Note that while the style of discussion in this article is 'analytic', its focus is only peripherally on basic issues of philosophy of science and I reference work in this genre only where directly relevant to current practices in the social sciences. For earlier efforts of this nature, see Mahoney, 'Beyond Correlational Analysis', pp. 579–80; Mayntz, 'Mechanisms in the Analysis of Social Macro-Phenomena', pp. 237–54.

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they might be regarded as attributes of a single concept. Others are patently contradictory. Some of these meanings are more well established than others; but all have been adopted by prominent writers and in an explicit fashion.

It must be stressed that calling attention to disagreements and ambiguities in the usage of a key concept does not, by itself, invalidate that concept. The following passages should not be read, therefore, as an indictment. What this semantic exercise does is to underline the degree of muddiness that a key concept has introduced into our methodological discussions. Apparently, partisans of mechanisms are sometimes talking about quite different things when they invoke the same term. In this respect, the school is considerably less unified than it may have appeared.

The second goal of this article is reconstructive. I propose that the core meaning of 'mechanism' – a causal pathway or process leading from X_1 to Y – functions nicely as a general definition of the term in methodological contexts across the social sciences. Accordingly, other meanings introduced in the course of this discussion may be regarded as arguments, rather than attributes. If accepted, this small move towards semantic consensus may go a long way towards clarifying causal debates in the social sciences.

HARD-TO-OBSERVE OR EASY-TO-OBSERVE ENTITIES

An initial disagreement appears over the *observability* of causal mechanisms relative to evidence of a covariational nature. Here, it must be remembered that observability is always a matter of degrees. While the so-called Heisenberg principle decrees that no entity can be measured with perfect accuracy, it seems apparent that some entities (such as molecules) are relatively easy to observe – i.e., they can be observed more or less directly – while others (such as quarks) must be inferred. In this spirit, we shall say that phenomena are *more* or *less* observable.

For many writers, the hallmark of causal mechanisms is their unobservability, and hence their hypothetical quality.[21] The previous example, drawn from contemporary physics, illustrates that as one moves down the causal chain from small observable entities to their component parts – from molecules to atoms to subatomic particles – one's ability to see, and hence to measure, diminishes with each step.[22] In a similar fashion, many social processes are harder to 'see' as one peers into the box of causality, i.e., as the units of analysis become smaller and smaller. Perhaps this is what prompts Andrew Bennett to define mechanisms as 'ultimately unobservable physical, social, or psychological processes'.[23] 'No matter how far we push the border between the observable and the

[21] Roy Bhaskar, *The Possibility of Naturalism: A Philosophical Critique of the Contemporary Human Sciences* (Atlantic Highlands, N.J.: Humanities Press, 1979), p. 15; George and Bennett, *Case Studies and Theory Development*, p. 143; Hedstrom and Swedberg, 'Social Mechanisms', pp. 13–15; Johnson, 'Consequences of Positivism', p. 247; Mahoney, 'Beyond Correlational Analysis'; James Mahoney, 'Revisiting General Theory in Historical Sociology', *Social Forces*, 83 (2004), 459–89; Herbert Alexander Simon, 'The Meaning of Causal Ordering', in Robert K. Mertin, James S. Coleman and P. H. Rossi, eds, *Qualitative and Quantitative Social Research* (New York: Free Press, 1979), pp. 65–81 at p. 71.

[22] Steven Weinberg, *Dreams of a Final Theory: The Scientist's Search for the Fundamental Laws of Nature* (London: Vintage, 1993).

[23] Andrew Bennett, 'Beyond Hempel and Back to Hume: Causal Mechanisms and Causal Explanation' (paper presented at the Annual Meeting of the American Political Science Association, Philadelphia, 2003), app. p. 15.

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unobservable, some irreducibly unobservable aspect of causal mechanisms remains,' write George and Bennett.[24]

Yet, some writers are quite adamant in their rejection of observability as a criterion for mechanisms. Bunge declares, 'Because most mechanisms are hidden, they must be conjectured before they can actually be discovered. Consequently, no self-respecting empiricist (or positivist) can condone the very idea of a mechanism'.[25] In a similar vein, Mahoney writes: 'causal mechanisms are posited relations or processes that the researcher imagines to exist; they do not refer to any particular set of empirical conditions'.[26] And again: 'explanation by causal mechanisms requires that the analyst posit some entity, process, or structure that is treated "as if" it exists, even though at the present time scholars cannot be certain that the entity, process, or structure really does exist. [It is] not itself open to explanation'.[27] This conclusion is linked to Mahoney's view that causal mechanisms are 'ultimate' explanations, i.e., unmoved movers. It follows that,

> If the mechanism were directly observable, it would be clear that this mechanism is not actually the final mover of outcomes in the world, but rather must itself be explained. Hence, causal mechanisms that become observable because of better measurement start to lose their status as causal mechanisms and become regular variables.[28]

If mechanisms are difficult (perhaps even impossible) to observe, then it follows that any investigation into causal mechanisms is a highly deductive enterprise, resting upon what we can intuit about the world and/or about one set of events. By contrast, covariational analyses take on the flavour of a largely inductive enterprise. Indeed, it is sometimes averred that *evidence* is covariational while cogitation (logical assumptions and reconstructions of the world) are mechanismic. This is what Mahoney seems to mean when he says that, once mechanisms have been observed, they are 'regular variables'.

Yet, for other writers (or for the same writers at other times), the attraction of a mechanisms-based causal account is motivated by the prospect that micro-level relations might be *easier* to observe than macro-level relations. From this perspective, the finer pieces of the puzzle – the micro-foundations of causation – can be studied with greater clarity than the larger ambient structures. As one narrows scope, one ought to be able to measure relationships with greater precision. In contrast to the large, abstract concepts that often characterize 'structural' arguments – for example, about capitalism, geography and colonialism – arguments about causal mechanisms may be closer to the meat of things. Thus, Stinchcombe defines a causal mechanism as 'a piece of scientific reasoning which is *independently verifiable*'.[29] Elsewhere in the text quoted above, Bennett writes: 'explanation via causal mechanisms involves a commitment in principle to making our explanations and models consistent with the most continuous spatial-temporal sequences

[24] George and Bennett, *Case Studies and Theory Development*; p. 143, quoted in Bernhard Kittel, 'On the Observability of Causal Mechanisms in Macro-Level Research', *Concepts and Methods*, 2:2 (Summer 2006), 15–16, p. 15. Of course, these passages may refer to the general unobservability of *all* social phenomena, as noted above, in which case they are no more applicable to mechanisms than to covariations.

[25] Bunge, 'Mechanism and Explanation', p. 421.

[26] Mahoney, 'Beyond Correlational Analysis', p. 581.

[27] James Mahoney, 'Tentative Answers to Questions about Causal Mechanisms' (paper presented at the Annual Meetings of the American Political Science Association, Philadelphia, 2003), p. 5.

[28] Mahoney, 'Tentative Answers to Questions about Causal Mechanisms', p. 5.

[29] Stinchcombe, 'The Conditions of Fruitfulness of Theorizing about Mechanisms in Social Science', p. 367. Emphasis in original.

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we can describe at the finest level of detail that we can *observe*'.[30] Drawing on a long tradition in philosophy of science, Bernhard Kittel views mechanisms as 'processes in concrete systems', which can be 'decomposed into their components' and thereby studied.[31]

Note that for those who view causal mechanisms as the more observable component of the empirical universe, it is natural to speak of a movement 'down' from causal covariations to causal mechanisms. Whereas, for those who view causal mechanisms as the less observable component the reverse logic obtains. Mahoney, who falls in the latter tradition, writes: 'the analyst moves from a high level of abstraction (i.e., a causal mechanism) to a concrete level of abstraction (i.e., an empirical proposition)'.[32]

This debate recalls the classic and ongoing debate over whether, or to what extent, causal models must be realistic. According to one version of positivism, articulated forcefully by Friedman, models are simply logical constructs whose only purpose is to make predictions about outcomes under different (causal) conditions.[33] The constituent elements of the model itself need not – and usually will not – be observable. Those who adopt a mechanism-centred approach to causation are generally quite critical of this nonchalance. Even so, in defining causal mechanisms as unobservable, these writers would seem to have committed themselves to the same phenomenological mysticism that Friedman is commonly criticized for. Hedstrom and Swedberg are explicit on this point:

> The choice between the infinitely many analytical models that can be used for describing and analyzing a given social situation can never be guided by their truth value, because all models by their very nature distort the reality they are intended to describe. The choice must instead by guided by how useful the various analytical models are likely to be for the purpose at hand.[34]

Here is a statement that Milton Friedman in his most positivist moment could have written.

UNIVERSAL OR BOUNDED EXPLANATIONS

Another ambiguity concerns the *relative generality* of mechanismic and covariational explanations. Evidently, explanations can never be entirely particular (specific, particular) or general (universal). Yet, there are important differences of degree.

Many researchers stress the context-dependent or bounded nature of causal mechanisms.[35] The assumption is that explanations for a given causal relationship usually vary across different contexts. Thus, George and Smoke argue that deterrence failure may occur by '*fait accompli*', by 'limited probe', or by 'controlled pressure'.[36] The same outcome is motivated by different causal mechanisms. This leads to a species of typological theorizing in which causal types are distinguished not by their outcomes but rather by their causal mechanisms.[37] If one ignores 'obvious' explanatory tropes ('He crossed the road

[30] Bennett, 'Beyond Hempel and Back to Hume', app. p. 16. Emphasis added.
[31] Kittel, 'On the Observability of Causal Mechanisms in Macro-Level Research', pp. 15–16.
[32] Mahoney, 'Tentative Answers to Questions about Causal Mechanisms', pp. 6–7.
[33] Milton Friedman, 'The Methodology of Positive Economics'.
[34] Hedstrom and Swedberg, 'Social Mechanisms', pp. 1–31, at p. 15.
[35] E.g., George and Bennett, *Case Studies and Theory Development*.
[36] Alexander L. George and Richard Smoke, *Deterrence in American Foreign Policy: Theory and Practice* (New York: Columbia University Press, 1974), pp. 522–36, chap. 18, esp. p. 534.
[37] Colin Elman, 'Explanatory Typologies in Qualitative Studies of International Politics', *International Organization*, 59 (2005), 293–326.

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because he desired to do so'), causal mechanisms in the social world are usually 'idiosyncratic and singular'.[38] Stinchcombe takes note that 'quite often what we mean by "mechanisms" in social science is a causal connection *within* relatively bounded units that form coherent "parts" of a larger structure'.[39]

Others identify causal mechanisms with more or less universal causal properties.[40] The idea here is that mechanismic explanations, peering deeply into the box of causation, settle on the most fundamental building blocks of causation, and hence on those that are most general in their purview. These 'nuts and bolts' of social science rest ultimately upon simple axioms of human behaviour – rational or otherwise[41] – and may often be formalized in mathematical models.[42] If mechanisms are akin to highly abstract causal frameworks – for example, functionalism, rational choice, power theory[43] – then it is sensible to equate a causal mechanism with a universal (or at least wide-ranging) theory.

A third possibility is that causal mechanisms refer to general phenomena but that these general phenomena take on different shapes according to the local setting. Thus, generality combines with specificity – 'laws' with particular contexts. This seems to be what is implied by Elster's well-known definition of mechanisms: 'frequently occurring and easily recognizable causal patterns that are triggered under generally unknown conditions or with indeterminate consequences'.[44]

The latent disagreement between those who associate mechanisms with highly particular explanations and those who view mechanisms as universal laws (more or less) feeds into another sort of confusion about how causal inference actually occurs. The contrast here is between 'bottom-up' and 'top-down' approaches to causation. Glennan explains: 'The top-down approach explains an event by showing it to be part of a larger nomological or explanatory pattern, while the bottom-up approach explains an event by describing the network of causes that are efficacious in bringing that event about'.[45] It follows that those who view mechanisms as context-specific would be inclined to adopt a bottom-up view of how mechanisms contribute to causal analysis, associated with the work of Wesley Salmon.[46] Similarly, those who see mechanisms as an illustration of universal (or at least very wide-ranging phenomena) would be sympathetic to a top-down approach derivative of the logical positivist school.[47] Here, a causal relationship is explained by subsuming it within a more general law, the latter pertaining to the mechanism that is purportedly at work.[48]

[38] Raymond Boudon, 'Social Mechanisms without Black Boxes', in Hedstrom and Swedberg, eds, *Social Mechanisms*, pp. 172–203, at p. 172.

[39] Stinchcombe, *The Logic of Social Research*, p. 17. Emphasis in original.

[40] Cowen, 'Do Economists Use Social Mechanisms to Explain?', p. 128; Glennan, 'Mechanisms and the Nature of Causation', p. 68; Mahoney, 'Tentative Answers to Questions about Causal Mechanisms'.

[41] Jon Elster, *Nuts and Bolts for the Social Sciences* (Cambridge: Cambridge University Press, 1989).

[42] Achen, 'Toward a New Political Methodology'.

[43] Hedstrom and Swedberg, 'Social Mechanisms'; Mahoney, 'Tentative Answers to Questions about Causal Mechanisms'.

[44] Elster, 'A Plea for Mechanisms', p. 45.

[45] Stuart S. Glennan, 'Rethinking Mechanistic Explanation' (unpublished, Department of Philosophy and Religious Studies, Butler University, 2000), p. 1.

[46] Salmon, *Scientific Explanation and the Causal Structure of the World*.

[47] Hempel and Oppenheim, 'Studies in the Logic of Explanation'.

[48] E.g., Elster, *Nuts and Bolts for the Social Sciences*.

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PROBABILISTIC OR DETERMINISTIC EXPLANATIONS

A fourth issue concerns the degree of regularity to be found in causal mechanisms. Are they probabilistic or deterministic? Apparently, there is a marked divide between those who believe that causal mechanisms ought to exhibit regularity and those who believe that these interstices of causality are highly unpredictable.

Daniel Little describes a causal mechanism as 'a series of events governed by lawlike regularities that lead from the explanans to the explanandum'.[49] Mahoney presumes that a mechanism is 'sufficient to produce the outcome of interest; that is, if the mechanism actually operates, it will always produce the outcome of interest'.[50]

Others articulate the notion of mechanisms in explicitly probabilistic terms: 'Given certain conditions K, an agent will do x because of M with probability p'.[51] Elster is also impressed by the contingent nature of mechanisms, which are 'triggered under generally unknown conditions or with indeterminate consequences'.[52]

It is commonly assumed that the determinacy of a causal relationship increases as that relationship becomes narrower and more specific – i.e., as it moves from structural-level inputs and outputs to micro-mechanisms. Thus, Cook and Campbell write: 'It is probably the case that the more molar the causal assertion and the longer and more unspecified the assumed micromediational causal chain, the more fallible the causal law and the more probabilistic its supporting evidence'.[53] Yet, David Waldner points out that sometimes the movement is in the other direction.[54] Suppose that there is some irreducibly stochastic process at the sub-atomic level. This does not presuppose that all actions are probabilistic, for 'as we increase size from the microworld to the macroworld, quantum effects become negligible and new properties … emerge'.[55] Consider the natural phenomenon of ice across a body of water. While the icing of a lake may be impossible to predict at a molecular or sub-molecular level (that is, at the level of causal mechanisms), it is certainly possible to predict that if the temperature is below freezing for a given number of days, any lake will freeze over. This is a deterministic process (or it is so highly probable that it is, for all intents and purposes, deterministic). It must happen, even though the causal mechanisms are themselves probabilistic. Similar arguments might be made for molar phenomena that pertain to social behaviour, which are often phrased as necessary or sufficient (i.e., deterministic) conditions.[56]

As with previous debates, an in-between position is possible. Stuart Glennan employs the term 'law' in conjunction with causal mechanisms but prefers a non-deterministic reading of the concept.[57] The operation of causal mechanisms is highly regular, but not perfectly invariant.

[49] Daniel Little, *Varieties of Social Explanation: An Introduction to the Philosophy of Social Science* (Boulder, Col.: Westview, 1991), p. 15.

[50] Mahoney, 'Beyond Correlational Analysis', p. 580.

[51] Gudmund Gambetta, 'Concatenations of Mechanisms', in Hedstrom and Swedberg, eds, *Social Mechanisms*, pp. 102–24, at p. 102.

[52] Elster, 'A Plea for Mechanisms', p. 45.

[53] Thomas Cook and Donald Campbell, *Quasi-Experimentation: Design and Analysis Issues for Field Settings* (Boston, Mass.: Houghton Mifflin, 1979), p. 33.

[54] Waldner, 'Anti Anti-Determinism'.

[55] Waldner, 'Anti Anti-Determinism', p. 90.

[56] Gary Goertz and Harvey Starr, eds, *Necessary Conditions: Theory, Methodology and Applications* (New York: Rowman & Littlefield, 2003).

[57] Glennan, 'Mechanisms and the Nature of Causation'.

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A DISTINCT TECHNIQUE(S) OF ANALYSIS

Sometimes, it is averred that the investigation of causal mechanisms involves a different set of methodological tools than would be common, or useful, to an investigation of covariational relationships. For some writers, the investigation of causal mechanisms is linked to qualitative methods of explanation and analysis.[58] Special terms – for example, 'causal-process observations', 'process tracing', 'pattern-matching', and 'colligation' – have been coined for this endeavour.[59] Similarly, causal mechanisms are often contrasted with a 'statistical' mode of analysis, understood to apply to the covariation between X_1 and Y.[60] Finally, it appears that all writers in the mechanismic school, whether they are practitioners of qualitative or quantitative methods, share a general scepticism about the connection between standard statistical models and the social processes they are intended to explain or illuminate.[61]

It must be stressed that this species of argument is contingent upon a view of what the key concept, mechanism, means. If a mechanism is simply the causal pathway from an exogenous cause to an outcome then there is no necessary connection between the object of study and the method chosen for analysis. In the causal sequence $X_1 \rightarrow X_2 \rightarrow Y$, a mechanismic explanation is one that focuses on the relationship between X_1 and X_2, and X_2 and Y, while a covariational explanation is one that focuses simply on the distal relationship between X_1 and Y, excluding any specific reference to X_2. Notable in this interpretation is the relative nature of these relationships. X_2 is defined in terms of X_1 and Y. That which lies between the exogenous causal factor and the outcome gains this status only by reference to these stationary points. This means that any redefinition of the exogenous cause, or the outcome, changes one's definition of the causal mechanism. And it follows from this that there is no method intrinsic to covariational analysis that cannot also be applied to mechanismic analysis.

Consider the now-classic argument that geographic factors such as variable disease-propensities influenced the decisions of Europeans with respect to which areas of the world they wished to settle and what sorts of economic institutions would flourish, and these patterns of settlement and development exerted a subsequent influence on the development or non-development of property rights, and finally, through this medium, on the long-term economic development of the non-European world.[62] In this account, geography plays the role of exogenous cause, with settlement patterns and property rights as the two causal mechanisms. Now, suppose the question is re-ordered such that the development of

[58] Hedstrom and Swedberg, 'Social Mechanisms', p. 17; Stinchcombe, 'The Conditions of Fruitfulness of Theorizing about Mechanisms in Social Science'; Andrew Bennett and Colin Elman, 'Complex Causal Relations and Case Study Methods: The Example of Path Dependence', *Political Analysis*, 14 (2006), 250–67.

[59] George and Bennett, *Case Studies and Theory Development*; Henry E. Brady, 'Data-Set Observations versus Causal-Process Observations: The 2000 U.S. Presidential Election', in Henry E. Brady and David Collier, eds, *Rethinking Social Inquiry: Diverse Tools, Shared Standards* (Lanham, Md.: Rowman & Littlefield, 2004), pp. 267–72; Clayton Roberts, *The Logic of Historical Explanation* (University Park: Pennsylvania State University Press, 1996).

[60] John H. Goldthorpe, *On Sociology: Numbers, Narratives, and the Integration of Research and Theory* (Oxford: Oxford University Press, 2000), p. 149.

[61] Aage B. Sorensen, 'Theoretical Mechanisms and the Empirical Study of Social Processes', in Hedstrom and Swedberg, eds, *Social Mechanisms*, pp. 238–66.

[62] Readers will recognize an amalgamation of arguments vetted by Daron Acemoglu, Simon Johnson and James A. Robinson, 'Colonial Origins of Comparative Development: An Empirical Investigation', *American Economic Review*, 91 (2001), 1369–401; Kenneth L. Sokoloff and Stanley L. Engerman, 'Institutions, Factor Endowments, and Paths of Development in the New World', *Journal of Economic Perspectives*, 14 (2000), 217–32.

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property rights, rather than economic development itself, is regarded as the outcome to be explained, and settlement patterns rather than geography, as the exogenous cause. We have simply truncated the causal chain. Yet, in this new version we must now look for a new causal mechanism (intervening variable): by what process did settlement patterns lead to variation in property rights? Naturally, this question could also be chopped up into finer bits; this is the well-known infinite regress of causation. Similarly, the argument could be expanded – stretched out – to stretch back in time, or forward in time, thus re-situating the causal mechanism.

All of this is relatively unexceptional and likely to meet little resistance. However, the implications are quite far-reaching. If this is all that the concept of mechanism refers to, then it may be wondered whether there is anything at all that is distinctive about this mode of analysis. Note that a 'mechanism', so conceived, can be at any remove from an outcome, for the issue of proximity is purely definitional, i.e., left to the author to decide. And a mechanism may be of any sort – an event, a process, a set of events or processes, or whatever. It is simply that which lies between X_1 and Y (causally speaking). Accordingly, whatever tools may be employed in the investigation of X_1 and Y (the 'covariational' moment) may also be employed in the investigation of X_1 and X_2, or X_2 and Y (the 'mechanismic' moment). This means that tools of empirical analysis usually associated with covariation (does one variable co-vary in a predictable pattern with another variable?) are, in principle, equally amenable to the investigation of intervening variables.

As a second example, one might consider the putatively causal relationship between trade openness and welfare state development. The usual empirical finding is that more open economies (as measured by imports and exports as a share of gross domestic product (GDP)) are associated with higher social welfare spending. The question of interest concerns the causal mechanisms at work in this relationship. Why does such a robust correlation exist? One possible causal path, suggests David Cameron, is that increased trade openness leads to greater domestic economic vulnerability to external shocks (due, for instance, to changing terms of trade).[63] If so, one should find a robust correlation between annual variations in a country's terms of trade (a measure of economic vulnerability) and social welfare spending. As it happens, the correlation is not robust and this leads some commentators to doubt whether this putative causal mechanism is actually at work.[64] What is significant here is that the research design employed to investigate the original ('covariational') question – does trade enhance welfare state development? – is identical to the research design employed to test the question of causal mechanisms. Both employ a standard time-series cross-section design with country-years as units of analysis. In methodological terms, they are identical.[65]

Indeed, when one looks closely at the growing population of studies focused on causal mechanisms one finds a quite diverse array of methodological approaches, including

[63] David R. Cameron, 'The Expansion of the Public Economy: A Comparative Analysis', *American Political Science Review*, 72 (1978), 1243–61.

[64] Alberto Alesina, Edward Glaeser and Bruce Sacerdote, 'Why Doesn't the US Have a European-Style Welfare State?' *Brookings Papers on Economic Activity*, 2 (2001), 187–277.

[65] For additional examples of this nature, see Yi Feng, *Democracy, Governance, and Economic Performance: Theory and Evidence* (Cambridge, Mass.: MIT Press, 2003); Elissaios Papyrakis and Reyer Gerlagh, 'The Resource Curse Hypothesis and its Transmission Channels', *Journal of Comparative Economics*, 32 (2003), 181–93; Michael Ross, 'Does Oil Hinder Democracy?' *World Politics*, 53 (2001), 325–61.

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qualitative case studies,[66] medium-*N* Qualitative Comparative Analysis,[67] sequence analysis,[68] experimental[69] and the more traditional methods of cross-unit quantitative analysis of observational data.[70] Granted, the modal research design for mechanismic analysis is probably focused on a single example (or a few examples) of a larger phenomenon. This is because the sort of causal questions that we usually refer to as mechanismic are often difficult to approach in a large-*N* cross-case format.[71] Thus, one finds that, as a practical matter, a strong affinity exists between causal mechanisms and case study investigation – that is, investigation focused on one (or several) instances of some broader topic.[72] Usually, it is difficult to get inside the box without also narrowing the scope of investigation. One case can be taken apart – usually a time-consuming procedure – while it would be difficult to do the same for a large number of cases.

So, we are left with an equivocation. There is no necessary connection between the study of causal mechanisms and the use of particular methods. But there is a fairly strong empirical association. This means that causal mechanisms cannot be defined (minimally) as employing a particular method or set of methods, but the term is bound to carry a strong implicit methodological connotation.

A DISTINCT APPROACH TO PROVING CAUSATION

Partisans of causal mechanisms sometimes imply that causal demonstration can occur without covariational evidence (evidence of the covariation between X_1 and Y). At the same time, some empiricists seem to imply that causal demonstration can occur without a specification of mechanisms. From this perspective, the adoption of a mechanismic approach to causation appears to commit one to a distinct ontological or epistemological position. Steinmetz writes sternly: 'a [causal] law is not a constant conjunction of events but the characteristic pattern of activity, or tendency, of a mechanism'.[73]

Depending upon one's understanding of causal explanation, both positions in this debate might be defended. Causal arguments can be undertaken in exclusively mechanismic or covariational ways. Consider that some causal arguments are adduced without the benefit

[66] George and Bennett, *Case Studies and Theory Development*; Peter A. Hall, 'Aligning Ontology and Methodology in Comparative Politics', in James Mahoney and Dietrich Rueschemeyer, eds, *Comparative Historical Analysis in the Social Sciences* (Cambridge: Cambridge University Press, 2003), pp. 373–404.

[67] Charles C. Ragin, *Fuzzy-Set Social Science* (Chicago: University of Chicago Press, 2000).

[68] Andrew Abbott and Angela Tsay, 'Sequence Analysis and Optimal Matching Methods in Sociology', *Sociological Methods and Research*, 29 (2000), 3–33; Lawrence L. Wu, 'Some Comments on "Sequence Analysis and Optimal Matching Methods in Sociology: Review and Prospect" ', *Sociological Methods and Research*, 29 (2000), 41–64.

[69] James Habyarimana, Macartan Humphreys, Daniel N. Posner and Jeremy M. Weinstein, 'Why Does Ethnic Diversity Undermine Public Goods Provision? An Experimental Approach' (unpublished, Georgetown University: Institute for the Study of Labor, 2006).

[70] Macartan Humphreys, 'Natural Resources, Conflict, and Conflict Resolution: Uncovering the Mechanisms', *Journal of Conflict Resolution*, 49 (2005), 508–37; Ross, 'Does Oil Hinder Democracy?'

[71] The very fact of being labelled a causal mechanism may imply that there is something methodologically recalcitrant about the question at hand.

[72] John Gerring, 'What is a Case Study and What is it Good For?', *American Political Science Review*, 98 (2004), 341–54.

[73] George Steinmetz, 'Critical Realism and Historical Sociology', *Comparative Studies in Society and History*, 40 (1998), 170–86, pp. 177–8.

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of any observable variation on X_1 and Y. 'Counterfactual' arguments have this quality.[74] Arguably, they are purely mechanismic – although, to be sure, some covariation between X and Y must be imagined, and in this sense no argument can be purely mechanismic (*of what* would it be a mechanism?).

By the same token, the use of experimental research designs sometimes allows for the demonstration of causal relationships based solely on covariational evidence, without any identifiable causal mechanism. This was the case, for example, with penicillin, aspirin and many other medical treatments whose curative effects were well known (and not disputed) long before the mechanisms at work in these treatments could be deciphered.

Commonly, one finds that in investigating a causal question the mechanismic and covariational aspects are not equally accessible to empirical research. Sometimes, the mechanisms can be glimpsed easily, and a process-tracing style of research is warranted.[75] In other situations, arguments are clearer and more testable at the covariational level. For example, research has identified a strong and robust relationship between democracy – considered as a historical phenomenon – and subsequent economic performance, when tested within a standard cross-national regression format with fixed effects and a series of relevant controls.[76] A number of plausible causal mechanisms might be enlisted to explain this regularity. In recent work, it has been proposed that democratic regimes create four kinds of capital – physical, human, social and political – and that each of these accumulates over time, creating a 'stock' that reflects a country's regime history and, ultimately, impacts its growth performance. Regrettably, it is not easy to test any of these causal mechanisms, so they rest – for now – at the level of speculation.

Some writers in the mechanismic school appear to take the position that, without supporting evidence at the micro-foundational level, causation has not been proven. Kiser and Hechter write: 'A complete explanation also *must* specify a mechanism that describes the process by which one variable influences the other, in other words, how it is that X produces Y'.[77] Waldner continues in the same vein: 'Explanatory propositions are distinguished from non-explanatory propositions by the inclusion of causal mechanisms.'[78]

It is undeniably the case that one is less certain about a causal phenomenon whenever the mechanisms remain opaque. But it is to be wondered whether such uncertainty should prevent us from reaching causal conclusions – however qualified – where strong covariational evidence, and strong theoretical speculation, support such conclusions. The same might be said for situations in which evidence is drawn primarily at the mechanismic level.

Although the argument could be carried on at much greater length, it seems fair to say that neither causal mechanisms nor covariation is necessary – and both are, in certain rare instances, individually sufficient – for the demonstration of a causal argument.

[74] David K. Lewis, *Counterfactuals* (Oxford: Blackwell, 1973); Philip E. Tetlock and Aaron Belkin, eds, *Counterfactual Thought Experiments in World Politics* (Princeton, N.J.: Princeton University Press, 1996).

[75] George and Bennett, *Case Studies and Theory Development*.

[76] John Gerring, Philip Bond, William Barndt and Carola Moreno, 'Democracy and Growth: A Historical Perspective', *World Politics*, 57 (2005), 323–64.

[77] Edgar Kiser and Michael Hechter, 'The Role of General Theory in Comparative-Historical Sociology', *American Journal of Sociology*, 97 (1991), 1–30, p. 5. Emphasis added.

[78] David Waldner, 'Transforming Inferences into Explanations: Lessons from the Study of Mass Extinctions', in Richard Ned Lebow and Mark Irving Lichbach, eds, *Theory and Evidence in Comparative Politics and International Relations* (New York: Palgrave Macmillan, 2007), pp. 145–75.

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While this apparent resolution of the debate may provide an adequate point of departure it is not a happy point of arrival. Evidently, one wishes to construct good (persuasive) causal arguments, not merely statements that pass a minimal definitional threshold. In this light, it seems plausible to regard both of these elements of causal argument as helpful ingredients in proving a causal claim. *Ceteris paribus*, a causal argument is stronger (i.e., more convincing) if a causal mechanism can be specified and a set of covariational patterns consistent with that mechanism can be identified.[79]

Note also that as a matter of practice it is rather difficult to construct purely covariational or purely mechanismic explanatory accounts. Even in the absence of empirical evidence a counterfactual argument always presumes a pattern of covariational evidence. Similarly, a covariational pattern, if asserted as causal, presumes a (yet to be identified) causal mechanism. One might conclude on this basis that there is considerably less distance between the covariational and mechanismic accounts of causation than writers in the mechanismic tradition aver. How deep is this disagreement, really?

MICRO-FOUNDATIONS

A final view of our subject presupposes that a mechanism must refer to a cause that lies fairly close to the outcome of interest – a proximate cause, rather than a distal (structural) cause. This is linked to a broader view of explanation that seeks explanatory power in small spaces (causally speaking). In this reading, mechanisms refer to a 'style of theorizing',[80] one usually focused on individuals rather than institutions, or one that breaks down institutions into their individual-level behavioural components.[81]

A bit of intellectual history is helpful in understanding the appeal of causal mechanisms as micro-foundational causal analysis. Through most of the twentieth century, structural approaches to causation have been associated with the scientific analysis of human behaviour. This propensity may be traced back to the origins of social science – i.e., to Marx, Weber, evolutionary theory, cyclical theories (in the manner of Toynbee), racial theories and geographic theories. Indeed, most influential approaches to the explanation of human behaviour through the late-twentieth century assumed a broad, structural approach to causation (with the exception of the field of psychology, whose disciplinary bias is quite different). In the mid-to-late twentieth century, one thinks of structural-functionalism,[82] modernization theory,[83] dependency/world systems theory,[84] realist international relations theory,[85] comparative-historical

[79] Gerring, 'Causation'.

[80] Hedstrom and Swedberg, 'Social Mechanisms', p. 25.

[81] Elster, *Nuts and Bolts for the Social Sciences*.

[82] E.g., Meyer Fortes and Edward Evan Evans-Pritchard, *African Political Systems* (Oxford: Oxford University Press, 1940); Bronislaw Malinowski, *Argonauts of the Western Pacific* (Prospect Heights, Ill.: Waveland, [1922] 1984); Jean Piaget, *Structuralism* (New York: Harper & Row, 1970).

[83] Cyril Edwin Black, *The Dynamics of Modernization: A Study in Comparative History* (New York: Harper & Row, 1966); Talcott Parsons, *The Social System* (New York: The Free Press, 1951).

[84] E.g., Fernando Henrique Cardoso and Enzo Faletto, *Dependency and Development in Latin America* (Berkeley: University of California Press, 1979); Immanuel Wallerstein, *The Modern World-System: Capitalist Agriculture and the Origins of the European World Economy in the Sixteenth Century* (New York: Academic Press, 1974).

[85] E.g., Kenneth Waltz, *Theory of International Politics* (New York: McGraw-Hill, 1979).

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analysis,[86] equilibrium models of economic behaviour,[87] and cross-country regression analysis.[88] The macro focus assumed by these varied theoretical and empirical approaches was often accompanied by the implicit message that 'structures count' and, as a result, individual-level behaviour would follow more or less ineluctably from those structures.

In recent years, all this has decidedly changed. Macro is out, and micro is in. Instead of examining the behaviour of nations, economists now focus on the behaviour of individuals. Similarly, in political science many scholars have dropped down a level of analysis – from the nation-state to particular regions or communities,[89] or individual-level data. Instead of distal causes, the focus has shifted to proximate causes. The general assumption seems to be that much of our confusion over causal relations in the social world is due to what might be called a level-of-analysis problem. We have been looking for causes in all the wrong places. Specifically, we have been looking for causal relations at broad, system-wide levels where causal relations are least clear and least determinate – because so much is going on 'inside the box'. Once these inside-the-box relationships are better understood, it is averred, social science will be laid on a solid foundation and the broader causal factors at work – the structural causes, if you will – can be discerned. We should therefore begin with the trees, and proceed (at some unspecified point in the future) to the forest. Or, to choose a different metaphor, knowledge should be attained brick by brick; slowly, the shape of a larger structure will come into view.

One might question the optimism with which some contemporary writers embrace a nuts-and-bolts view of social action. Is it possible to reconstruct causation block by block? Are there basic 'building blocks' of social action? The current enthusiasm for mechanisms reminds one of a previous century's fascination with mechanistic models of causation. With respect to both, one might reply: perhaps true, but as yet unproven.

Alternatively, the current craze for micro-foundations might be viewed as a thoroughgoing rejection of grand theory. Accordingly, there is no presumption that individual building blocks will stack up to create larger theoretical frameworks. Instead, one embraces a social science founded on 'middle-range' explanations.[90] This might also be regarded as a return to the methodological individualism characteristic of mid-twentieth century behaviouralism.[91] Indeed, some writers are explicit about the connection between mechanismic and individual-level explanation. 'I take "mechanisms" to be hypothetical causal models that make sense of *individual* behavior', writes Gambetta.[92]

[86] E.g., Samuel P. Huntington, *Political Order in Changing Societies* (New Haven, Conn.: Yale University Press, 1968); Barrington Moore Jr, *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Boston, Mass.: Beacon Press, 1966); Theda Skocpol, *States and Social Revolutions: A Comparative Analysis of France, Russia, and China* (Cambridge: Cambridge University Press, 1979).

[87] E.g., Marie-Esprit-Leon Walras, *Elements of Pure Economics* (London: George Allen & Unwin, 1954).

[88] E.g., Phillips Cutright, 'Political Structure, Economic Development and National Social Security Programs', *American Journal of Sociology*, 70 (1965), 439–55; Bruce M. Russett, J. David Singer and Melvin Small, 'National Political Units in the Twentieth Century: A Standardized List', *American Political Science Review*, 62 (1968), 932–51.

[89] Richard Snyder, 'Scaling Down: The Subnational Comparative Method', *Studies in Comparative International Development*, 36 (2001), 93–110.

[90] Raymond Boudon, 'What Middle-Range Theories Are', *Contemporary Sociology*, 20 (1991), 519–52; Robert K. Merton, *Social Theory and Social Structure* (New York: The Free Press, [1949] 1968).

[91] Robert A. Dahl, 'The Behavioral Approach in Political Science: Epitaph for a Monument to a Successful Protest', *American Political Science Review*, 55 (1961), 763–72.

[92] Gambetta, 'Concatenations of Mechanisms', p. 102. Emphasis in original. See also Thomas Schelling, 'Social Mechanisms and Social Dynamics', in Hedstrom and Swedberg, eds, *Social Mechanisms*, pp. 32–44, at pp. 32–3.

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It is worth noting that while most partisans of mechanisms adopt a micro-foundational view, some are uncomfortable with its stance of methodological individualism.[93] Evidently, to focus on micro-foundations – without hope of building larger theoretical structures – presents the danger of occluding larger structures at work in conditioning social behaviour.

For all these reasons, it may be a mistake to restrict our use of the concept of causal mechanisms to work that is micro-foundational. The concept of mechanisms is helpful in many research contexts, even – perhaps especially – when the style of research is macro-structural. Moreover, given that we have another term in the lexicon – 'microfoundations' – that expresses this key idea, there is even less reason to appropriate 'mechanisms' for this semantic purpose.

RESOLVING CONCEPTUAL AMBIGUITY

I have shown that the new-found popularity of causal mechanisms hides a profundity of meanings and methodological implications. To say 'mechanism' may refer to one or more of the following: (1) the pathway or process by which an effect is produced; (2) a difficult-to-observe causal factor; (3) an easy-to-observe causal factor; (4) a context-dependent (tightly bounded) explanation; (5) a universal (i.e., highly general) explanation; (6) an explanation that presumes highly contingent phenomena; (7) an explanation built on phenomena that exhibit lawlike regularities; (8) a distinct technique of analysis (based on qualitative, case study or process-tracing evidence); or (9) a micro-level explanation for a causal phenomenon.

There are, to be sure, many additional ambiguities that I have not touched upon.[94] Consider, as one example, Arthur Stinchcombe's oft-cited definition:

> As I use the word, *mechanism* means (1) a piece of scientific reasoning which is independently verifiable and independently gives rise to theoretical reasoning, which (2) gives knowledge about a component process (generally one with units of analysis at a 'lower level') of another theory (ordinarily a theory with units at a different 'higher' level), thereby (3) increasing the suppleness, precision, complexity, elegance, or believability of the higher-level theory without excessive 'multiplication of entities' in it, (4) without doing too much violence (in the necessary

[93] Kevin D. Hoover, *Causality in Macroeconomics* (Cambridge: Cambridge University Press, 2001); Kevin D. Hoover, *The Methodology of Empirical Macroeconomics* (Cambridge: Cambridge University Press, 2001); Tilly, 'Mechanisms in Political Processes'; Waldner, 'Anti Anti-Determinism'. See discussion in Johnson, 'Consequences of Positivism', p. 247.

[94] For example, some scholars identify the study of causal mechanisms with a narrative account of social action (this is perhaps the more usual position), while others presume the construction of formal mathematical models (e.g., Cowen, 'Do Economists Use Social Mechanisms to Explain?'; Thomas Schelling, *Micromotives and Macrobehavior* (New York: W. W. Norton, 1978); Schelling, 'Social Mechanisms and Social Dynamics'; Sorensen, 'Theoretical Mechanisms and the Empirical Study of Social Processes'). Kittel, 'On the Observability of Causal Mechanisms in Macro-Level Research', points out that while the normal usage of causal mechanism is quite distinct from the manipulation theory of causation (based on formal experiments), George and Bennett occasionally veer towards the latter: 'If we were able to measure changes in the entity being acted upon after the intervention of the causal mechanism and in temporal and spatial isolation from other mechanisms, then the causal mechanism may be said to have generated the observed change in this entity' (George and Bennett, *Case Studies and Theory Development*, p. 137). This usage is virtually indistinguishable from Holland, 'Statistics and Causal Inference', or for that matter from Gary King, Robert O. Keohane and Sidney Verba, *Designing Social Inquiry: Scientific Inference in Qualitative Research* (Princeton, N.J.: Princeton University Press, 1994), pp. 76–9, as noted by Kittel, 'On the Observability of Causal Mechanisms in Macro-Level Research'.

https://doi.org/10.1017/S0007123408000082 Published online by Cambridge University Press

simplification at the lower level to make the higher-level theory go) to what we know as the main facts at the lower level.[95]

Evidently, there is much more to talk about; I have only scratched the surface of this large and complex topic.[96]

What, then, are we to make of this blooming ambiguity (which may be even greater, and more various than this brief review has indicated)?

Any analytic deconstruction of a concept offers a species of cheap debunking if read in a crude and unreflective fashion. The implication in the present case appears to be that since 'mechanism' means so many different things – often quite contradictory to one another – it means nothing at all. The logic of this slander by multiple-association is that words should mean one thing, and one thing only. Yet, while univalence is a helpful quality in many contexts it is probably an unrealistic expectation for key terms in the social science vocabulary. It may not even be helpful, given that a degree of lexical plasticity is necessary for a discipline to progress.[97] Thus, while the proliferation of meanings attached to 'mechanism' deserves notice, it does not entail that we should – or can – jettison the offending term. And it certainly does not mean that just because different authors employ the term in different ways, all such usages should be condemned.

However, for general methodological purposes it is important that a key word like mechanism be employed in a recognizable fashion. There must be some degree of consensus; otherwise, the term serves to confuse, rather than clarify, methodological issues – as has happened, arguably, over the past several decades of this term's ascendance.

I propose that the core meaning of mechanism – the pathway or process by which an effect is produced or a purpose is accomplished – provides such a consensus. My sense is that few contemporary social scientists would reject this definition out of hand and most would approve of its utility in discussions of social science research.[98] It is methodologically insightful, which is to say, there are many instances when we feel the need to clarify what, specifically, connects X_1 with Y. While the usual ('covariational') questions of causation focus on whether X_1 causes Y (is this a true causal relationship?), the mechanismic approach focuses on why X_1 might cause Y, understanding that there may be more than one causal mechanism at work. Mechanisms are about the 'why' question nested within a larger 'why' question.

Some writers would like to add further ramifications to this core definition, as outlined in previous sections of this article. I suggest that we regard these elaborations as peripheral to the core concept. They may be true in some research contexts; they may shed light on some aspects of social research; they may be true in some normative sense (as an ideal-type towards which social science practice should strive). But they are not true – yet – of linguistic practice or research practice within the social sciences generally. As such, they deserve to be regarded as arguments about mechanisms rather than definitions of mechanism. Alternatively, we may choose to regard the various meanings identified in this

[95] Stinchcombe, 'The Conditions of Fruitfulness of Theorizing about Mechanisms in Social Science', p. 367. Emphasis in original.

[96] A long list of definitions is provided in Mahoney, 'Beyond Correlational Analysis', pp. 579–80.

[97] Otherwise, all scientific progress must be achieved through new terms, a process that is sometimes more confusing than the redefinition of old terms.

[98] But see Mahoney, 'Beyond Correlational Analysis', p. 578.

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article as positions within a larger typology, in which the core concept branches out into several distinct mechanismic options, regarded as conceptual sub-types.[99]

This way of viewing things may help to restore a modicum of conceptual unity in an area where coherence is notably lacking. To clarify, this proposal is not meant to stifle the various debates that the word 'mechanism' has stimulated, but merely to preserve a core meaning upon which we can all (more or less) agree.

[99] I owe this suggestion to Jim Mahoney (personal communication). See David Collier and James E. Mahon, Jr, 'Conceptual "Stretching" Revisited: Adapting Categories in Comparative Analysis', *American Political Science Review*, 87 (1993), 845–55; David Collier and Steven Levitsky, 'Democracy with Adjectives: Conceptual Innovation in Comparative Research', *World Politics*, 49 (1997), 430–51.

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