

***More challenges for social mechanisms:  
Contribution to the Persson-Chuang discussion***  
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The extended discussion stimulated by Johannes Persson's (2012a) critique of Elster's definition of causal mechanisms in this forum is a valuable one. Anyone interested in causal mechanisms theory will be appreciative of the thoughtful contributions offered by Persson (2012b) and Kimberly Chuang (2012).

The core of Persson's critique is that Elster appears to define a causal mechanism in a way that incorporates the idea of unknown triggers or indeterminate outcomes as a part of the definition. Persson finds fault with this approach on the ground that it implies that when we gain more knowledge about how a mechanism M works, M is no longer a mechanism (because its trigger or outcomes are no longer unknown). This is deeply counterintuitive: more knowledge produces less explanatory power.

Kimberly Chuang thinks there is a flaw in Persson's argument against Elster. She provides a careful exegesis of what Elster might have meant with his original specification of causal mechanisms as well as a sustained argument intended to show that Persson's arguments fail. Her argument is complex, but it comes down to the claim that the examples Persson provides are actually specifications of particular (local) triggers rather than general triggers. So the item remains a causal mechanism after all — against Persson's conclusion. This is the thrust of her claim that "Persson's argument inappropriately overlooks the difference between applications of mechanisms rather than mechanisms themselves" (2012: 2).

For what it's worth, I think that Persson's criticism is still a valid one. I do not believe that Elster had fully thought through the implications of the definition he offered, and I do believe that Persson's argument highlights a significant problem with that definition. As indicated in my discussion of Persson in my earlier contribution to this debate, I believe that the issue is remedied by offering a realistic rather than epistemic definition of a causal mechanism. (Persson prefers "ontic" to "realist", but fundamentally we are in agreement.) Chuang's dissection of the logic of Type A and Type B mechanisms is skillful and useful and substantially more explicit than Elster's formulations were. However, this elaboration of the language of "unknown triggers, indeterminate consequences" does not make it any more philosophically compelling why these features of our knowledge of a causal process should be built into our definition of a causal connection (mechanism). When philosophers and scientists attribute causal powers to things, their intention is to convey that the power exists in the world, not in the framework of knowledge. And so it should be with attributions of causal mechanisms.

**Why causal mechanisms?**

In what remains of this brief contribution I would like to direct attention back to a more fundamental question and then introduce a new problem with causal mechanisms. The

fundamental question is this: why are we interested in social causal mechanisms in the first place? In engaging in a debate over the appropriate definition of an X, one of our guiding goals should be to clarify the important issues that led us to want a conception of an X at all. In this case, what were the driving motivations for introducing the idea of a causal mechanism into discussions of social explanation in the first place?

One such reason is the difficulties raised for the social sciences by the covering law model of explanation, with its collateral requirement of explanatory laws of a domain of phenomena. Social phenomena are heterogeneous and conjunctural, and outcomes rarely conform to simple regularities. There is no reason whatsoever to expect to find a unified theoretical system that permits unified explanations of all social phenomena. At the same time, we think there are compelling and empirically defensible answers to questions like “Why did the anti-Muslim violence in Gujarat in 2002 occur?” and “Why does ethnic violence occur in India?”. But we recognize that there are no governing regularities when it comes to ethnic violence or other such complex social processes. So if the explanation of Gujarat and the occurrence of ethnic violence elsewhere in India must wait upon the discovery of such regularities, then we are forced to conclude that no satisfactory explanation exists. (Here is a [lecture](#) by Atul Kohli at the Asia Society on the causal roots of sectarian conflict in India that shows how a skillful social scientist attempts to account for some of the causes of ethnic violence.)

A second reason for turning to the idea of a causal mechanism is a rough-and-ready causal realism that many of us share about how social events come about. We do think that there are real causal relations in the social world, where one set of circumstances brings about another set of circumstances according to some kind of necessity deriving from the nature of the social constituents. We think that the causal language that social scientists and historians use when they explain why social events or structures emerge makes sense. So it is very natural to ask, what were some of the salient mechanisms and processes in play in the causation of these events? How is it that certain events and conditions made violence inevitable or likely in the circumstances? How does the outbreak of ethnic violence work? From what does the necessity linking events in time and place derive? The idea of a causal mechanism was introduced — certainly this was true in my own case in writing about the idea in *Varieties of Social Explanation* (Little 1991) — as a way of capturing the idea that there are real underlying causal processes in the social world which it is the business of the social sciences to uncover.

So choosing the language of causal mechanisms is one way of expressing causal realism about the social world. But there are other metaphors that could have been chosen. “Mechanism” is an appealing concept because we have fairly clear ideas in mind of how mechanical processes work. So when we ask “what were the social mechanisms that were operative in Gujarat?”, there is a fairly intuitive meaning that is conveyed to the reader. But we could have chosen to pursue the metaphors of causal powers or causal processes as well, or even something more fluid and relational along the lines of what Andrew Abbott recommends in his [critique](#) of causal mechanisms (Abbott 2007).

The difficulty of formulating a clear conception of a social mechanism that doesn't raise more problems than it solves suggests the possibility that we might be well advised to begin to look beyond mechanisms to a more satisfactory set of concepts about the underpinnings of social causation. (John Dupré (2012) raises exactly this point in the case of the philosophy of biology.)

### **A new problem**

Here is a problem about causal mechanisms that has not yet received much attention. This issue derives from the condition of "regularity" that many definitions of a causal mechanism invoke. A mechanism is often defined as a real causal relation among several parts, supported by a strong regularity governing the interactions of those parts. Stuart Glennan's definition plainly makes this conjunction: "A mechanism for a behavior is a complex system that produces that behavior by the interaction of a number of parts, where the interactions between parts can be characterized by direct, invariant, change-relating generalizations" (Glennan 2002: S344). McAdam, Tarrow, and Tilly share this assumption as well: "Mechanisms are a delimited class of events that alter relations among specified sets of elements in identical or closely similar ways over a variety of situations" (McAdam, Tarrow, and Tilly 2001: 24). "A produces B" constitutes a CM if the real properties of A work to bring about B and if A is regularly associated with B. (I refer to these as pocket-sized regularities.) The basic idea here is that once the mechanism is triggered, it leads more or less inexorably to its result.

The first part concerns the idea of real causal efficacy. Here is Rom Harré, excerpted by Mahoney:

The structures, states and inner constitutions from which the phenomena of nature flow ... the permanent or enduring conditions under which a certain kind of phenomenon will occur.

The inner constitutions, structures, powers, encompassing systems, and so on, of which natural generative mechanisms are constituted, and of which the connection between cause and effect usually consists." (Harré 1970: 101-102, 104)

The second criterion expresses the idea that trigger and outcome are linked by a strong regularity or law. Here is Stuart Glennan:

A mechanism underlying a behavior is a complex system which produces that behavior by the interaction of a number of parts according to direct causal laws. (Glennan 1996)

The problem is that these two criteria are likely to pull apart in many circumstances. There may be a real causal push from A to B, but it may result in a relatively low incidence of A followed by B. In this circumstance it is true that A is part of the cause of

B, but it is not true that  $A \rightarrow B$  represents a causal mechanism. And in fact we do refer to mechanisms where the likelihood of the consequent is low even when the antecedent occurs; for example, “the mechanism of George’s becoming ill is his close exposure to Alice who had the contagious illness.” It may be that the likelihood of catching the flu is still low following exposure; but there is no other way of getting the flu.

This would entail that it is not the case that all causal linkages are transmitted by causal mechanisms, and we would be forced to refer to causal powers or processes instead. Further, the question of whether a given linkage represents a mechanism or not would depend on the relatively boring facts about the world that push up or down the probability of the linkage — not some fundamental ontological distinction between mechanisms and other causal connections.

This seems to suggest that we have a difficult choice. Either we can drop the “regularity” criterion or we can acknowledge that “causal mechanism” does not capture all reasonably direct causal connections. If we take the second route, we are ill advised to define causation in terms of causal mechanisms per se; instead, we need a more comprehensive concept of “real causal structures and processes”, of which a subset are causal mechanisms.

It may be that considerations surrounding this issue are impossible to resolve without dropping the requirement that a mechanism corresponds to some sort of regularity. However, another possibility is to weaken the regularity criterion so that it encompasses causal relevance rather than high probability of succession. The causal relevance test offered by Wesley Salmon (Salmon 1984) would handle this approach for a mechanism like “contagion through exposure” along these lines:  $\text{illness} \mid \text{exposure} > \text{illness} \mid \sim \text{exposure}$ . Would this suffice as a corrective to the current definition? We might reframe the definition along these lines:

- $A \Rightarrow B$  represents a causal mechanism if
- (i) the real properties of A produce B
  - (ii)  $B \mid A > B \mid \sim A$

But if we take this approach, we lose the ability to infer from cause to effect through the mechanism. In alternative language, we move from thinking of mechanisms as sufficient causes to necessary conditions (or rather their probabilistic equivalents).

In short, it appears that the continuing conjunction of regularities with causal mechanisms poses yet another difficult challenge for the theory of causal mechanisms. What we can probably agree about is that for any level of phenomena for which explanation is possible at all, there must be a level of entities, constituents, and causal powers that interact causally to bring about specific macro outcomes. The discovery of those causally connected processes is a central task of explanatory inquiry, and these findings can reasonably be interpreted realistically. What is not determinate is whether we should best

describe these intricately interconnected sequences as mechanisms, processes, causal interactions, or something else.

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## References

- Abbott, Andrew. 2007. Mechanisms and relations. *Sociologica* 2: 1-22.
- Chuang, Kimberly. 2012. In defense of Elster's mechanisms. *Social Epistemology Review and Reply Collective* 1 (9): 1-19.
- Dupré, John. 2012. *Processes of life: essays in the philosophy of biology*. Oxford; New York: Oxford University Press.
- Glennan, Stuart. 1996. Mechanisms and the nature of causation. *Erkenntnis* 44: 49-71.
- Glennan, Stuart. 2002. Rethinking mechanistic explanation. *Philosophy of Science Supplement* 69 (3): S342-S353.
- Harré, Rom. 1970. *The principles of scientific thinking*. Chicago: University of Chicago Press.
- Little, Daniel. 1991. *Varieties of social explanation: An introduction to the philosophy of social science*. Boulder, Colorado: Westview Press.
- McAdam, Doug, Sidney G. Tarrow, and Charles Tilly. 2001. *Dynamics of contention* (Cambridge Studies in Contentious Politics). New York: Cambridge University Press.
- Persson, Johannes. 2012b. Social laws should be conceived as a special case of mechanisms: A reply to Daniel Little. *Social Epistemology Review and Reply Collective* 1 (7): 12-14.
- Persson, Johannes. 2012a. Mechanistic explanation in social contexts: Elster and the problem of local scientific growth. *Social Epistemology* 26 (1): 105-114.
- Salmon, Wesley C. 1984. *Scientific explanation and the causal structure of the world*. Princeton: Princeton University Press.