

1. Mara Eisch-Schweitzer

This week's readings focused on the idea of 'mechanisms'. The apparent 'sibling rivalry' characterized by the 'Science Wars' between the natural and social sciences seems to be about this idea of social science mechanisms and how they measure up to the 'laws' of the natural sciences. Hedstrom and Swedberg depict three types of social mechanisms. An exploration of Type 1 (situational) mechanisms and Type 2 (action-formulation) mechanisms, which involve the individual, is essentially to open the black box to explore the causal relationship between *input* and *output* effects. Further, if I understand this correctly, mechanisms are not variables, but there are variables in the form of characteristics or concepts that impact mechanisms.

I have three points to make from the readings. First, Hedstrom and Swedberg express a concern that exploration into the black box for causal mechanisms have, to the detriment of the social sciences, been too often descriptive and less often explanatory. I am assuming descriptive means a description of the actions of individuals and the variables that influence the action. **[The term "influence" here introduces an ambiguity in your formulation, for this could be interpreted as meaning "explains" or "causes," and thus is more like a mechanism.]** Much like anatomy is descriptive of the body systems, organs, tissues and cells and the variables that influence their actions. (Recognizing that these actions are not conscious choice actions simply mechanical like actions). I also assume explanatory means an explanation **of the causal mechanisms** **[The issue is not so much finding an explanation of the mechanisms, but explaining something by invoking mechanisms. Rational choice is a mechanism that can be used to explain certain actions, but this does not mean that one actually tries to explain rationality as such.]** much like physiology explains the functioning of body systems, organs, tissues and cells. However, unlike anatomy and physiology, the social sciences do not have separate categories in which to place descriptive vs explanatory knowledge. To learn anatomy is not to learn physiology; anatomy can be learned without an understanding of physiology but physiology requires an understanding of anatomy. (Another example might be arithmetic and mathematics.) It seems that it would be beneficial to differentiate more clearly the categories of descriptive and explanatory scientific knowledge of the social sciences. The predictive nature of mechanisms would fall into the explanatory category but not the descriptive category. Challenges to methodology and content would be limited to specific categories of the social sciences rather than to the whole of the social sciences.

Second, there seems to be an assumption that mechanisms can be law-like only if they apply to all action. **[To be law-like it is not necessary that it apply to all action, but simply that where it applies it leads to law-like generalization of the form When A**

occurs, B occurs because of mechanism X.] The subject matter of the social sciences is human action and the work of the social sciences is to describe and explain human action. Technically, then the social sciences have but one subject – humans, and mechanisms must account for all of them; if A, then B. And yet in the natural sciences have multiple subjects and their laws can follow, if A, then sometimes C, D, or B. For example in biology all plant life needs water to survive, but it can be a little water, a lot of water, or they live in water, or salt water, or fresh water. This may be overly simplistic but it seems to make the point. There are also exceptions to natural laws. Gravity, an often cited natural law, has exceptions – not everything falls down. **[The law of gravity does not say that “everything falls down” it says that everything attracts everything in a particular manner – the manner is the law – and that a small object that is close to a huge object will “fall” towards it unless it is impeded by some other force.]** A helium filled balloon rises against gravity, this exception is explained in the relationship between helium’s properties to gravity. **[This is not an exception to the law of gravity – it is precisely what the law of gravity predicts given mass, densities, etc.]** And, the law of hydrostatic pressure has exceptions in the presence of colloid-osmotic pressure. Mechanisms of social sciences do not seem accorded the same level of ‘if A then sometimes C, D or B’, nor the opportunity to make exceptions. **[Mechanisms in social science do obey the “sometimes true” proposition – that is precisely why they can be explanatory but not predictive in a simple manner. I am not sure I understand your point here.]**

Finally, the sibling rivalry over mechanisms vs laws seems to be analogous to comparing apples and oranges, or worse yet, apples and cauliflower. Both are types of plant vegetation but each requires distinctly different context and methodology of production and offer distinctly different nutritive value. The natural and social sciences are both types of science and yet each requires distinctly different context and methodology for production of knowledge and offer distinctly different knowledge and value for understanding reality. I don’t understand the need for the social sciences to create its image in the likeness of the natural sciences – as if to be restricted to the natural science’s ideology of the social sciences. As Flyvbjerg suggests in *Making Social Science Matter* (2001), it may be the commitment of the social sciences to the analytical reasoning of the natural sciences that actually thwarts the development of the value-rationality of the social sciences.

Coda (additional response sent later): In this week’s readings, mechanisms, as defined by Stinchcombe, is “a bit of theory about subunits of a larger structure that has independent validity at its level...” (Hedstrom and Swedberg, p. 298). Sorensen (p.240) presents a case for mechanisms and not variables as the subunits, and power, of explanatory theory. I would like to follow in class with a discussion around the role of variables and mechanism paralleled with descriptive vs explanatory theory. It would seem unlikely that variables can produce explanatory power, but they are not in and of themselves useless to the understanding of mechanisms.

2. Matt Desmond

What is at stake in van den Berg's critique of Habermas, Bourdieu, Giddens, and Alexander (talk about a tall task!)? He sates that his main contention is that the aforementioned theorists (except for Habermas, who is framed as 'one of us in disguise') have manufactured little causal explanatory mechanisms for social phenomena to account for how "structure is converted by individuals into social action or how such social actions aggregate to constitute social structures" (p. 205). However, the article seems to be just as much about (and here I find van den Berg especially whinny) difficult language, an unfair allocation of fame, the underdeveloped idea between consuming versus producing mechanisms, or the "globalization" of social theory. To the last point on theoretical "globalization," this does not seem to me to be a problem at all—although van den Berg rams this hammer down as a concluding point as to why 'Grand Theorizing' has failed—since the relationship between structure and agency is a difficult query, and we should except our most prominent social theorists to be grappling with it. **[There is also the complaint that these theorists make banal statements that they misrepresent as profound new formulations, so that when they do talk about mechanisms they really have nothing new to say.]**

If we take the definition of a mechanism as a "plausible hypothesis, or set of hypotheses, that could be the explanation of some social phenomenon, the explanation being in terms of interactions between individuals and other individuals, or between individuals and some social aggregate" (Schelling, p. 32-3), then we can easily locate mechanisms in the above mentioned theorists' work. Take Habermas, is not the idea laid out in *Legitimation Crisis* that in modern capitalist societies the intrusion of the state (he calls it the 'political system') into the market (the 'economic system') a mechanism explaining why legitimation crises emerge? **[To count as a mechanism-explanation, in van den Berg's account, one would have to elaborate the idea of "intrusion into the market" in a way that works through the actions and purposes of individuals]** Or take Bourdieu, is not the idea of correspondences between homologous social positionings and positionings in the university field as argued in *Homo Academicus* a causal mechanism for why certain faculty members are scholars of law and not chemistry or art? **[Two comments here: 1) van den Berg does admit that Bourdieu sometimes has mechanisms, but just that they are generally nothing very special and they remain unexplicated; 2) "correspondences between homologous social positionings and positionings in the university field" does not yet seem exactly like a "mechanism" in van den Berg's sense. A correspondence is a descriptive claim about a correlation. To be a mechanism you would need to show how it works, what is the process involving human action by which this generates the purported outcomes.]**

In my eyes, the presence or absence of a mechanism is not what is at stake here, as van den Berg asserts; rather, his 'dog in the fight' is the *style* of theorizing, which is precisely what the Hedstrom and Swedberg claim is the drive behind this text (p. 25). He wants grand theory to be a bit more humble, a bit more mid-range that is, and a bit more rational choice. This is why he picks a selectively shortsighted and rigid fight with the four theorists in his article. For example, he critiques Bourdieu's notion of habitus mostly for

being unoriginal since it aligns with the “globalized” theoretical agenda confronting the structure and agency problem; however, he fails to mention how the habitus makes a distinctive epistemological break with rational choice theorists by turning our attention from the mind to the body (this is why Bourdieu does not use the word ‘ideology’ for example). Unlike James Coleman’s conception of “strategy,” where the actor follows calculated goals, the strategy of the habitus is both conscious and unconscious and moves within its respective fields in with a sort of predictionary inertia, much like a versed tennis player on the court. It advances a dispositional, not rational, theory of action, and this is what distinguishes Bourdieu from many other thinkers, including most theorists in this text. **[van den Berg’s critique here is that habitus is really nothing other than ingrained habit – a perfectly fine, but conventional concept – and that it in no way “resolves” the dualism of structure and agency. I don’t think van den Berg insists on hyper-rational action as the only explanatory mechanism. Boudon comes close to that, but not van den Berg. What he is saying here is that there is no transcendence of the structure/agency dualism in Bourdieu in spite of his proclamations that there is, and that the mechanisms in the theory are conventional, but dressed up in a obscure formulation.]** Now, if you want to go picking a fight over the rationality of actors or lack thereof or level of theorizing that is most meaningful, I welcome such ideas, but framing the argument in terms the absence of mechanisms smokescreens the underlying arguments within this article and is, I think, a largely empty claim.

3. Matt Dimick

My first question is on the relationship between mechanisms and methodological individualism. Hedstrom and Swedberg tie mechanisms to methodological individualism since “[m]echanism-based explanations usually invoke some form of ‘causal agent’” (p. 11) and in the social sciences “the elementary ‘causal agents’ are always individual actors” (ibid.). Elster, however, contrasts his present position on mechanisms with his earlier work, saying that in the latter the search for mechanisms was “more or less synonymous with the reductionist strategy in science” (p. 47), that in the earlier work “the antonym of a mechanism is a *black box*” (ibid.), and that the reductionist, anti-black box strategy is “closely connected” to methodological individualism (ibid.). In Elster’s revised position (I am unsure whether he means to reject or complement his earlier approach) is to contrast mechanisms with laws and deterministic accounts. **[I think this is a complementary position rather than an alternative]** Boudon more forcefully rejects the connection between mechanisms and methodological individualism since the latter “does not guarantee the absence of black boxes” (p. 173). **[Boudon’s object of attack is really utilitarianism rather than methodological individualism more broadly understood. He insists that reasons are of central important in explanations of action, but feels that utilitarianism provides an unsatisfactory theory of reasons – thus is cognitive and normative amendments, which remain methodological individualist.]** Boudon (p. 175) provides instances such as frames, cognitive biases, and risk aversion as examples of black box, non-final explanations and Elster (p. 53) implies that cognitive dissonance is also a black box explanation since, in describing how it

operates in the world, “[s]omehow, one ‘just gravitates’” toward the dissonance reduction choice.

Restated, Boudon’s argument seems to be that reducing explanations to the individual level will not be reductionist enough (not enough to avoid black-box explanations), making the “individual” an arbitrary cut-off point. I recall that for Elster, methodological individualism is the idea that all social phenomena can be explained in ways that only involve individuals, their goals, beliefs, and actions—and “properties.” So Boudon’s argument must be that in order to explain any of these black-box psychological operations, one must resort to some sub-individual level—perhaps a resort to biology, chemistry, or physics—and that these sub-individual mechanisms would not be “properties” of the individual *per se*. One conclusion appears to be that a strategy of mechanisms does not commit one to a methodological individualist strategy. **[If your interpretation is right, then mechanisms commits oneself to a micro-reductionist strategy, but some of the reductions needed to get to a “final explanation” may be even more micro- than the individual. This would still count as a variety of methodological individualism I think.]** Also, in addition to the multiple realizability objection to methodological individualism, which says that some social phenomena may not be reducible to micro-individual level components, this discussion of mechanisms appears to raise another objection, which says that in some cases reducing explanations to the individual level will not be reductionist enough. (I wonder if this objection is a reason Elster has moved away from identifying mechanisms with the methodological individualist program?)

Boudon’s essay also raised another issue. He recommends rational-action mechanisms (of either the instrumental, normative, or cognitive kind) to sociologists on the ground that they provide final explanations. As a sociologist, I admit I prefer to resort to these kinds of explanations (rational action mechanisms), over the more, well, psychological, kind. But just because rational action mechanisms provide final explanations, I’m not sure they may always be the best or most adequate. Are there other reasons to privilege rational action mechanisms? **[It is not so obvious to me that the rational action mechanisms perform as well in providing “final” explanations as Boudon suggests, especially given all of Elster’s work on emotions and irrationalities. Boudon does give examples where false beliefs are well grounded in reasons and rationality – as in the case of scientists believing false things. But there are equally many beliefs that people hold which are false and believed for reasons of psychological processes of various sorts. Some of these are cognitive, but some are emotion driven. What this means is that it is just as necessary to explain why a particular belief is not distorted as to explain why it is – what you have is variability in the character of beliefs and need to explain that variability. Rational belief formation cannot be taken as an empirical baseline that does not itself need explanation.]**

4. Gokcen Koscuner

Questions – A Plea for Mechanisms

In “A Plea for Mechanism” Elster suggests the idea of mechanism as an intermediate between laws and descriptions. He defines mechanisms as frequently occurring and easily recognizable casual patterns that are triggered under generally unknown conditions or with indeterminate consequences.

He distinguishes between the antonym of a mechanism as a black box and as a scientific law. What does this difference imply? [I think the idea here is that the contrast with “scientific law” is about the type of explanation being proposed, whereas the contrast with “black box” is between an explanation and a nonexplanation.]

Elster considers spillover and compensation as intrapersonal mechanisms of attitude formation and suggests that monitoring different effects leads to better explanation of the action. **When looking at a social phenomenon and trying to understand why it is happening should we not also ask why certain groups of people act under spillover effect and others under compensation? [I think Elster would not object to such a question if there was any reason to believe that these mechanisms vary across groups. He sees these mechanisms as pretty universal psychological-cognitive mechanisms and I think he would be skeptical of the claim that some types of people are characterized mainly by spillover mechanisms and other types by compensation mechanisms. But there would be no a priori reason not to investigate this. To explain such a difference, of course, would itself require a specification of some mechanism of “mechanism formation” – i.e. some causal process that generates the predominance of one of these cognitive mechanisms relative to the other.]**

He also argues that actions are caused by desires and opportunities. **How about actions that are caused by neither, i.e actions caused by obligations, habits, culture etc. and what about conditions where people have both the desire and opportunity but yet do not take the agency to act. And how do we understand why people desire the things they do? [I don’t think that Elster is suggestion that actions are only caused by desires and opportunities. Certainly in his overall work he places a lot of emphasis on other kinds of psychological processes – cognitive as well as emotional, conscious and nonconscious (or what he calls “subintentional”). He would say that if an actor as a desire & opportunity and doesn’t act, then it must be because of a cognitive failure of some sort – the actor might lack information about the opportunity, or might believe that there would be undesirable side-effects of the action, or something like that.]**

5. Wayne Au

In the introduction, Hedstrom and Swedberg assert that, “In the social sciences, however, the elementary ‘causal agents’ are always individual actors, and intelligible social science explanations should always include specific references to the causes and consequences of

their actions.” (pp. 10-11). They then go on to sketch out strong and weak versions of methodological individualism, ultimately endorsing the weak version in order to incorporate “macro-level states” into the explanation of individual actions. Methodological individualism then surfaces in most of the essays of this book, sometimes in very disturbing ways (such as explicit uses of the free-market, invisible hand as a valid explanation of economic processes). This leads me to a couple of questions: **1) Do we have to “see” a mechanism at work in every single individual for it to be considered to be a valid explanation – to know that it is taking place? [I am not quite sure what you mean by “see” a mechanism. In any case, there is no need for a mechanism, in order to be valid, to be universally present. A particular kind of belief may explain certain kinds of actions without everyone holding the belief. But of course everyone has “beliefs” and beliefs are explanatory for all people. Maybe I am missing your point here.]** **2) Philosophically, doesn’t this use of methodological individualism end up mirroring aspects of subjective idealism if the causal mechanisms of observed phenomena can only be explained in terms of individual action/experience? [These sorts of explanations need not be idealist – the individual mechanisms need not simply be the ideas in the head of actors, and since there are aggregation issues through interactions of individuals and since the macro-contexts influence individual actions, there is plenty of scope here for non-subjectivist aspects of the explanation]** **3) Outside of methodological individualism, are there other, valid ways of understanding social mechanisms?** (My sense is that this book’s framing makes an argument that doesn’t leave us much room outside of methodological individualism). **[This is definitely something worth discussing in the class: can we think of mechanisms that are genuinely macro? When we say that capitalism explains imperialism, for example, this is a causal claim about the relationship between two macro-phenomena. But when we ask – well, what is the mechanism at work in this explanation? By virtue of what processes or mechanisms does capitalism generate imperialism, generally this will involve talking about strategies of capitalist firms competing with other firms and strategies of states supporting those firms, etc. And when we ask about those strategies – why they are adopted and how they generate their effects, then pretty quickly we get down to individuals and their beliefs as central parts of the analysis. But maybe not....]**

6. Eva Williams

Is Grand Theory Useful?

When Pierre Bourdieu envisioned a “Community of Scholars” it was precisely to combat this form of non-constructive attack found in Axel van den Berg’s Chapter *Is sociological theory too grand for mechanisms?* On the other hand, Bourdieu was, himself, known to summarily dismiss various scholars and their theoretical positions, so perhaps the “Community of Scholars” he had in mind was in relation to his own relational epistemology. **[Bourdieu was actually pretty dismissive of everyone who wasn’t in his own circle. There are lots of typical French stories about how he mercilessly would**

attack people in French sociology who were not part of his circle, even if they were not especially antagonistic to his brand of sociology.] (The subversion strategy, which van den Berg engages in—to dismiss grand theory as a pointless waste of time—is not surprising given his more dominated position within the field of social theory. **[Very nice point – using Bourdieu to explain van den Berg’s attack on Bourdieu!]**) That said, what do we make of this black box Bourdieu has constructed called his theory of fields? Do we dismiss it due to the absence of clear testable mechanisms spelling out specifically how the habitus is formed? Or does the need for a more detailed understanding of these mechanisms form the basis of what Bourdieu scholars need to address in their own scholarly work? It strikes me that most grand theoretical frameworks are broad-brush renderings. And while the devil is in the details, these theories remain *works-in-progress*. I’m not sure I find this as problematic as van den Berg. What we gain from grand theory, which we lack in working strictly with mechanisms, is a unifying framework through which to understand the social world. Such a framework assists the scholar to reconsider previously taken for granted, doxa, which even “systematic” work with mechanisms will likely miss. One wonders where the field would be today without grand theory. ‘Tis a bleak thought. **[I think I basically agree with you here – I am of the “let a thousand flowers bloom” school of scholarly community, and I think that there is a place for grand theory constructions. But I think van den Berg also has a point that there is a lot of puffery in these efforts. When Bourdieu claims to have transcended the structure/agency dualism, for example, I think he is pretty much just making an empty proclamation. In practice habitus reproduces that dualism as far as I can tell. Also, there is the problem identified by van den Berg and Sorenson in the book that this kind of high conceptualization theory has largely displaced other kinds of theory-building in sociology, which is probably not a good thing.]**

Is a field a mechanism?

If the market is a mechanism then so is Bourdieu’s construct of the social field. According to Gudmond Hernes (1998), “(a) mechanism is a set of interacting parts—an assembly of elements producing an effect not inherent in any one of them...not so much about ‘nuts and bolts’ as about ‘cogs and wheels’...an abstract, dynamic logic by which social scientists render understandable the reality they depict” (74). It strikes me that Bourdieu’s theory of fields fits perfectly into this definition of a mechanism, yet I’m certain most of the author’s in the text would disagree. **[The market counts as a “mechanism” in these accounts because it includes an elaborate discussion of how individual actions are formed through reasons, resources, incentives, etc. and how these are aggregated into stable patterns. Market-mechanism contain a kind of equilibrium idea that tells you why stable patterns emerge from these individual-level processes of interaction (exchange on markets). The question, then, is whether or not “fields” actually involve this kind of mechanism-elaboration, or whether fields have their effects through various black boxes. “Habitus” is meant to be a non-black box, I think, though the way it is meant to specify the dispositions of actors (basically their habits-of-action-and-thought in situations). Habitus is also a kind of equilibrium concept. But I am not sure that the exposition is clear enough for us to know precisely how this works.]**

Also drawing on Hernes' discussion of the nature of mechanisms: Does the conflict between those interested in theories of the middle-range and a systematic study of social mechanisms and those theorists proposing "Grand" theoretical frameworks, lie in the *sometimes true vs. always true* issue? **[I don't think Grand Theory is preoccupied with the "always true". Much of Grand Theory is about frameworks of concepts rather than real explanations of patterns and outcomes. Always true theories are built around general Laws, and most sociological Grand Theory doesn't attempt that.]** If not a primary source of friction, what role does this issue play if any?

7. Fabian Pfeffer

Does the stark contrast drawn between the correlational and the mechanism approach vanish when it comes to measuring up a mechanism to empirical reality? Aren't these two approaches rather complementary than mutually exclusive? [Correlational data are of fundamental importance to the study of mechanisms. One way of think about mechanisms is that they are supposed to provide explanations for correlations. Education is correlated with income. What is the mechanism that generates this correlation? Is it an investment-human-capital mechanism? A signaling mechanism? Or what?]

Hedström and Swedberg, and most of the authors in the book (most firmly Sørensen), draw a sharp contrast between "variable sociology" and a mechanisms approach to the study of social phenomena. I can agree with their assessment that most quantitative analysis in sociology has departed from meaningful theoretical explanations. Therefore, I am not questioning the fruitfulness of a mechanism approach to actually explain and not only describe social phenomena, but I want to ask how the relationship between a mechanism and empirical reality ought to be assessed concretely. I am convinced that all authors in the book would agree that theoretical ideas specifying a mechanism in the end have to be assessed on an empirical basis. The question therefore goes: how do we know whether a mechanism is correctly specified? First, by establishing consistency of the theoretical ideas about the mechanism at work with the I-O-relation, i.e. correlation under question. Mayntz (2004) notes "if social mechanisms are to explain observed phenomena or relationships, this means that the latter are logically prior: the 'what' logically precedes the 'how' question." (p. 253). Insofar, establishing a mechanism means *complementing* the correlational analysis rather than *replacing* it. Regarding mechanisms as the specification of the micro-foundation of macro-phenomena results in an additional demand concerning the 'testing' of a mechanism: For a mechanism to be more than merely a 'plausible interpretation' we will also have to look at the empirical consistency of the micro-links which are assumed. Despite the resolute rejection of the notion that "variables and not actors do the acting" (p. 16) which is assumed to be connected to 'variable sociology', I wonder how the hypothesized micro-links can be shown to be empirically consistent. Can the case be ruled out, that we find variables whose correlations could satisfactorily reflect the theoretical ideas used for specifying the micro-

links? If on the other hand this is possible, I am not sure if Sørensen's proposed mathematical models are the exclusive, most proper and most straightforward way to establish not only reasonable but empirically consistent mechanisms. **[Sorenson's point is really against the additive model. His claim is that all sociological research has implicit mathematical models, but mostly these models are "stupid models". Additive models are in general undefendable – no one really believes that the world is additive, and yet this is the standard way we do our research. The "variable analysis" accusation is basically an attack on such additive models. He is not saying we should stop measuring the variations in attributes of actors and situations, and such measures-of-variations are "variables." What he is saying is that our models should start with actors and their interactions, figure out the functional form by which the attributes of actors affect their interactions and thus the equilibrium outcomes we are interested in, and then empirically investigate these models.]** To sum up: in order to distinguish causation from correlation we need to specify a mechanism which itself reaches explanatory plausibility only by successfully 'reproducing' the correlations. Its plausibility at the end of the day also has to be assessed by testing the hypothesized links on the basis of correlations between carefully chosen variables at the micro-level. The challenge should be: Use mechanisms to explain 'statistical effects' and use statistics to test mechanisms! That might be the way to establish causality from correlations and at the same time detect potent (middle-range) theoretical mechanisms.

8. Dan Warshawsky

The authors of this week's readings pose central questions to sociological research. Although we have been indirectly studying the role of mechanisms in our discussions of critical realism and methodological realism, we will now directly interrogate the role of mechanisms in the discipline of sociology. What exactly are mechanisms, and how do they relate to description and law? How do different disciplines utilize, if at all, mechanisms as a useful analytic category? Third, how do mechanisms fit into future social science research? Will mechanisms be central or peripheral to future scholarly research in the social sciences?

As stated in Hedstrom and Swedberg's book, *Social Mechanisms: an Analytical Approach to Social Theory* and Jon Elster's "A Plea for Mechanisms," mechanisms must remain central in social science research. One of Axel van den Berg's central critiques is his disgust with Jeffrey Alexander, Pierre Bourdieu, Anthony Giddens, and Jurgen Habermas for their failing to focus on the role of mechanisms in explaining real phenomena. In response to Elster's initial questions: "Are there lawlike generalizations in the social sciences? If not, are we thrown back on mere description and narrative?" (Elster 1), he emphatically says no, and instead emphasizes the role that mechanisms can play as an intermediary between laws and description. It is the mechanism that is neither arrogantly overgeneralizing nor underanalyzing.

In the opening chapter of this week's book, the authors describe the four main components of mechanisms: action, precision, abstraction, and reduction. Among these four, I am interested in a few concepts. Last week we analyzed the role of the micro and macro and everything in between and the relationship between all of those possible connections. This week our authors pose that there are no macro level mechanisms, only macro-micro, micro-macro, and micro-micro. What, if anything, does this contribute to our discussions of methodological individualism and holism? When you combine this question with "precision," "abstraction," and reduction," I begin to wonder how the reductionist "black box" metaphor, small scope of "precision," and micro-macro discussions bring us back to a reducible methodological individualism and multiple realizability. **[These issues are closely related to the previous discussion. I think the appropriate way to respond to the claim that there are only *macro*→*micro*, *micro*→*micro*, and *micro*→*macro* mechanisms, but no *macro*→*macro* mechanisms, is to try to propose a *macro*→*macro* mechanism and see if it makes sense, see if it really does open up the cogs&wheels of the process.]**

If mechanisms are central to social science research, why have they been so underutilized in many academic disciplines?**[They are super-important in economics, and pretty important in political science, so it varies a lot across social sciences how central the attention is to mechanisms. Also, in sociology, there is actually a lot of talk about mechanisms, but it is pretty loose and unsystematic.]** Some have said that mechanisms are only thoroughly used in many biological or physical sciences, and within the social sciences, only economics frequently uses mechanisms as part of their research. **[Political science does as well – both in international relations and in things like electoral studies, lobbying research, etc.]** Although mechanisms are sometimes used in other social sciences, such as sociology, they still seem to be secondary. Is this the perspective of some of our authors, or is this truly the case? Do mechanisms take a secondary role in the social sciences, and if so, should they become more central to social science research? The authors of various book chapters and articles emphatically want us to focus on the role of mechanisms more systematically in our research. Although my hunch is that mechanisms are becoming more thoroughly utilized in sociology, I am very curious what other class members believe the role of mechanisms has been and should be in past, present, and future social science research. **[My own view is that whenever people actually claim they are explaining something they invoke mechanisms. This is more or less what commonsense tells us is needed for an explanation. In the absence of any mechanism we always feel that we may be making a mistake, and so advancing a claim about a mechanism that generates the observations we have made – especially the associations we observe – is pretty general. Where disciplines differ is in the precision and elaborateness of the mechanisms.]**

Thus, I have a couple of central questions. Clearly, the role of mechanism is important in social science research (otherwise we would probably not be studying it so much), yet the specific role it has and should play in the past, present, and future are not certain. Additionally, I would like more conversation about the relationship between mechanisms, the reductionist "black box" metaphor, discussions of the micro and macro,

and their distinguishing characteristics from methodological individualism and multiple realizability.

9. Elizabeth Holzer

I'd like to spend a bit of time figuring out how much value we should place on generality and whether a research program devoted to social mechanisms would really be superior to one that had well-developed grand theories. **[Are you sure that well developed “grand theories” actually yield more generality than well-developed theories of mechanisms? I am not so sure about this, at least if by “generality” we mean generality of our *explanations*. Grand theory may provide us with a high generality of conceptual menus for describing the world, but when it comes to actually building explanations, then I am not sure that they can avoid elaborating mechanisms.]**

The collection of articles seems to be aimed at convincing researchers to devote their energies to identifying social mechanisms rather than developing grand theories or narratives. The three approaches are presented on a scale of generality, with social mechanisms as a supposedly feasible compromise to the impossibility of making general laws and the unsatisfying specificity of narratives. I find the notion of “generality” a bit confusing—Elster, of example, writes that a “mechanism provides an explanation because it is *more general* than the phenomenon that it subsumes” (p.49). I don't quite understand the knowledge claims that Elster seems to think generality in itself has. In a case study, the object is to understand in a pretty specific manner, what's going on with a narrowly defined social phenomenon. **[But still: in every case study when an *explanation* is proposed of the particular things being studied – a change in the labor process, a social conflict, the development and dilemmas of a commune, or whatever – mechanisms are invoked which are more general than that particular case study. If a commune is running into difficulties with increasing conflicts after the original founder dies, and you want to explain this, then mechanisms like free-riding or normative erosion, or routinization of charisma, etc. are likely to be invoked to explain things. I think that is all that is meant here.]** Social statistics, on the other hand, is directed with a considerably higher degree of generality. I don't think any of the authors in this work would argue that one method to be superior to the other—but then I'm not sure what the relationship between generality and understanding actually lies.

Which leads to my second, related problem. Social mechanisms are said to be superior to grand theory because grand theory tends to generate black boxes—like “frames” and “habitus” (Boudon p. 174)—but both habitus and frames seem to me to be useful, productive concepts. Merton presents grand systems/theories as inconsistent with social mechanisms (cited by Hedstrom and Swedberg p. 6, though Elster p.49, 52 would disagree I think). I can see how discussions of social mechanisms would make a productive *addition* to the research program (and to some of the general theories out there already), but I don't see how social mechanisms are inherently superior to grand theory. **[I think the claim is that to the extent that Grand Theory crowds out efforts to build**

more systematic theories-of-mechanisms and systems of mechanisms, then it is harmful. I personally think that Grand Theory can be a source for lots of interesting insights which then need elaboration in more systematic mechanism-centered models. But there is a tension here from a sociology of knowledge point of view.]

(And I found van den Berg's claim that contemporary grand theorists like Bourdieu don't develop conceptual and methodological tools for studying social phenomena unconvincing (p. 232)).

10 . Brett Burkhardt

Elster defines mechanisms as “frequently occurring and easily recognizable causal patterns that are triggered under generally unknown conditions or with indeterminate consequences (p. 1).” Mechanisms are to be contrasted with laws, whose result is always predictable given certain initial conditions. Mechanisms thus allow us to explain but not to predict phenomena.

Despite the fact that Elster claims that mechanisms are “causal patterns”, I am not sure that he ever gets to real causality. This comes to the fore when discussing the indeterminacy of outcomes of a causal sequence. One indeterminacy example Elster provides deals with envy (23-24). Envy of one's enemies will either insulate a person from envying his friends (compensation effect) or it will lead to envy of his friends (spillover effect). A different mechanism (compensation or spillover) is posited for leading to each alternative outcome. Given a particular outcome (say, absence of friend-envy), the posited mechanism (compensation) seems quite plausible. But plausibility is not quite causality. **[I think you are collapsing the issue of the certainty with which we make a causal claim and whether or not the claim being made is about causality. I agree that Elster's formulations seem to make the explanations sometimes seem like story-telling, where we have good stories – thus the many proverbs – for potential explanations, but little way of differentiating them in practice, even after the fact.]** I don't think Elster can really show that there was any chain of causality at work in this (hypothetical) outcome if he does not base a prediction for an outcome (either friend-envy or lack of friend-envy) on a set of initial conditions (envying one's enemies).

Elster would likely respond by asserting that we must assume that these mechanisms have causal powers because the entire set of initial conditions, and therefore the entire set of relevant mechanisms, is unknown to us. If we knew all of the conditions and mechanisms in play at a given time, we could then formulate laws. Yet Elster's definition of a mechanism does involve causality. This seems to be based on the argument that a sequence of events must involve causation, and although we cannot identify all causal components of the sequence, we nonetheless can posit a mechanism; therefore, we must assume that this mechanism is causal.

But why should we assume that a mechanism has *causal* force just because we cannot observe *all* relevant causal forces? I think a better approach would be more “pragmatic.” This, I think, is the position taken in Hedstrom and Swedberg's introduction (1998). Here they write: “The choice between 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 be guided by how useful the various analytical models are likely to be for the purpose at hand (15).” Thus, we need not assume that a mechanism has causal power because we take into account the fact that it is an abstraction.

On further consideration, Elster does acknowledge this pragmatic aspect of mechanisms. Mechanisms, he says “are not desirable in themselves, only *faute de mieux* (6).” They can provide “an intelligible answer to the question, ‘Why did he do *that*?’ (10).” These are pragmatic qualities identified by Elster. My main concern or confusion, then, is the insertion of causality into his definition of a mechanism. It just seems to be unnecessary and too strong a claim if in fact mechanisms are less deterministic than laws. **[I think the issue is not so much the degree of determinism of the mechanism, but the degree of determinism in the situations in which we study mechanisms. In open-systems there are always a multitude of mechanisms operating, never just one, and therefore the postulated effects of the mechanism may be neutralized or deflected by the effects of other mechanisms. Indeed, the presence of other mechanisms may even switch off a given mechanism so it doesn’t generate effects at all. A properly specified mechanism is a real causal account of how B was produced given the initial conditions A. But sometimes you could have A with B being produced because of some other mechanism in the situation.]**

11. Mark Cooper

I am interested in the process by which investigation of mechanisms first begins by noting a strength or form of relationship between I and O, then hypothesizes and tests potential mechanisms, and finally concluding with some satisfaction that the mechanism identified is useful in explaining change within the given model. Within the third step I have two concerns. It is unclear in what sense identification of a given mechanisms actually constitutes an explanation if, “it can fairly be said that one man’s mechanism is another man’s black box.” (10) While there are certainly problems that are more likely within the realm of one discipline more than another discipline, I was confused by the statement that, “the mechanisms postulated and used by one generation are mechanisms that are to be explained and understood themselves in terms of more primitive mechanisms by the next generation.” **[The idea here is, I think, a question of continually searching for more explanatory depth – continual questions of the form “how does it work” or “why does this generate that effect”. Here’s an example:**

Step 1: sociologists notice the association of socioeconomic family background and adult income and proclaim: class origins influence earnings. Someone asks: what are the mechanisms? Why does family background have this effect? In the absence of an answer there is a black box: somehow or other there are some cogs and wheels that turn the initial conditions of family background into the outcome of income. To open that black box someone proposes that family background influences education and education influence earnings. Educational attainment is the mechanism by which background affects income.

Step 2: But why does education actually affect income? And why does family background affect education? There are now two more black boxes to unravel. The education → income black box could be unraveled through two mechanisms – signaling and human capital. Both of these invoke individual strategies, beliefs, cognitive failures, risk taking, time horizons, etc. The family → education black box can be elaborated in terms of mechanisms like aspiration formation, cultural capital formation, status expectations, risk-taking (in the manner suggested in the Boudon chapter).

Now, the question then becomes: once the black boxes in step 2 have been opened up, is there any need to open up the remaining black boxes? Once we know that education influences income because of a signaling mechanism (which works through the information imperfections of the bounded rationality of the employer) and through human capital investment (which works through the preferences and time horizons of the worker), is there any further insight to be gained by opening up the black boxes within those mechanisms? The key here is whether opening up the black box of “bounded rationality” will deepen our explanation of the family-background → income relationship? Once it seems that further black-box opening will not add anything to the original explanation, then you can stop. This is true in all sciences: there is no need to go to sub-atomic structures to explain why arsenic is poisonous. That explanation can stop at chemistry.]

The related issue is whether or not the criteria for evaluating a “useful” mechanism is at all problematic. (15) Hedström and Swedberg’s claim that the usefulness of an explanation within the model is marginally acceptable, but I am uncertain how adjudication between two explanations of the same phenomena can occur if the original models in which the mechanisms were identified were established within different parameters. Or does this mean that the two mechanisms are really not competing for the same explanatory functions since the models that inspired identification of these mechanisms were initially crafted toward the explanation of different phenomena? If this is true, how do we avoid creating models that become worlds within themselves? **[The only meaning to the idea of mechanisms “competing” with each other is if they are explaining the same phenomena. Remember, mechanism is a way of explaining an association between some initial state and an outcome. We want to explain the association of A and B and argue that mechanism M1 explains this association. But the explanation typically has the form “If A then sometimes B” – family background explains education through the mechanism of aspiration formation, but sometimes a person with a working class background develops middleclass aspirations. This means that if someone else proposes a mechanism M2 for the relationship between A and C (where C is middle class aspirations), then M1 and M2 might be compatible. But if one theory is about the A → C relation and the other about the X → Y relation there is no reason to believe that the mechanisms are in competition with each other.]**

12. Martín Santos

Adjudicating between mechanisms-based explanations

Do really “final explanations” (understood as “explanations without black boxes”) exist, as Boudon suggests? Certainly, a “black box” explanation such as “people do so because they have been socialized to do so” is unsatisfactory. However, explaining the relationship between democracy and religion in terms of a combination of the “compensation effect” and “the spillover effect” (“*if* the influence of democracy on religion is mediated by the compensation effect rather than the spillover effect, democratic societies will be religious. *If* the negative effect of democracy on desires (mediated by religion) is strong enough to offset the positive effect of democracy on opportunities, democratic citizens will behave moderately”) is still quite unsatisfactory. Regarding the relationship between politics and religion, let’s assume that the “compensation effect mechanism” is prevalent in society “A”, and the “spillover effect” mechanism is dominant in society B; a legitimate question would be, why is so? What is about these societies that lead to this different predominance in mechanisms? Would it be possible to answer this question by making recourse to *another micro-level* (as different from the “meso and macro levels distinguished by Wright) mechanism? **[You make a good case why these are not really satisfactory final explanations – the question “why?” still seems pressing. I think the test for a final explanation is whether or not our understanding of the causal relationship we are trying to unpack is changed by virtue of continuing black-box-opening. To take a trivial example: A person is hungry and wants a sandwich and so goes into a store to buy a sandwich, does so, leaves the store and eats the sandwich. We want to explain these observations. We invoke the person’s desires, capacities and opportunities (having money = capacities, having a store available = opportunities) as the mechanisms. We could then unpack the desire black box and ask how do desires lead to actions. But will this actually improve anything in the original explanation? It would only do so if it would help us sort out cases in which all of the initial conditions were satisfied in this example and yet the person did not buy the sandwich.]**

Let’s assume now, that at certain point in time, a researcher observes a switch in the predominance of these mechanisms: the spillover effect has become prevalent in society A and the compensation effect has in society B. Would it be enough in order to explain this switch to find a mechanism at the micro-level (for example, the “contrast effect”)? Would it be advisable to identify structural mechanisms at the meso and macro levels, that is to say, mechanisms not reducible to any form of social-psychological phenomenon? **[I think the illustration is not sufficiently elaborated to be clear (yet). It would be perfectly good to identify meso/macro processes if these are relevant for the on/off switching, but this would undoubtedly invite additional black-box openings. It is unlikely that the institutional factors would themselves constitute “final explanations” in the sense being demanded here, since probably those**

mechanisms would work by virtue of the way they trigger/shape beliefs, desires, propensities to act, etc.?

These concerns lead me to a further question: how to adjudicate between rival mechanisms-based explanations? Is it possible to adjudicate between frameworks that assume different models of social actor? For example, Gambetta assumes a more classic model of cost-benefit for explaining educational decisions, whereas Boudon allows for a more complex cognitive-normative model, and Elster assumes a hybrid model of cost-benefit and emotions as forces shaping decisions/actions. Would it be more powerful a model that combines a complexity of motives: cost-benefit, social emotions, norms and values? **[The more complex model in this instance is one that postulates a set of possible mechanisms. The general framework of action is really the same, but one model says you need more mechanisms to explain the variety of possible actions from initial conditions. These are entirely comparable, as is illustrated in Boudon's discussion where he accepts the utilitarian-rationality explanations as identifying real mechanisms but then argues for the need for additional mechanisms centering on cognitive processes. An even fuller theory – which would closer to law-like generalizations – would provide an account for when one mechanism or another was operative. A full account of the switching on and off of preference and belief formation mechanisms would then tell you how initial conditions get translated into outcomes.]**

In terms of the assumptions about social structure (Hernes), is it possible to adjudicate between mechanisms-oriented models that have different views of “social structure” (for example, social structure as *aggregated* reality –in Elster and even in Wright-, or social structure as a network of social relationships and practices that can not be understood simply in terms of an aggregation mechanism? **[I think it is always in principle possible to adjudicate between competing claims about mechanisms and social structures so long as there are empirical implications of the differences in question. The adjudication may be indecisive because the measurement/observation problems may swamp the theoretical differences.]**

13 Ana Cristina Collares.

My interrogation for this week focuses on the discussion presented at the beginning of the book “Social Mechanisms” about the definition of mechanisms in sociology, and on the article by Raymond Boudon, Ch. 8, “Mechanisms without black boxes”.

About the definition of social mechanisms, I identified there a tendency to associate mechanisms with the “lower” level of analysis, in a “weberian” fashion, i.e., individuals and their motivations, whether they be rational or guided by tradition, values or affections. Some authors in this book claim that an important, and sometimes neglected, dimension of sociology is the realm of explanation. Given that, I found a little

bit confusing the distinction between theory, causal correlations or associations, and mechanisms, and how all this is related to explanation in Social Sciences. Can I say that it is implicit in this view the idea that the progress of a sociological investigation follows the chain:

“finding causal associations => analyzing the mechanisms that could be responsible for the association => combining causal connections and mechanisms to produce a social theory” ? **[That seems about right. The claim here is that the kind of puzzles in sociology that have the capacity to generate advances in theory are of the form “why is A associated with B?” rather than simply “why does B vary the way it does?” The second question – I would argue – might spur the search for observations, which once made might generate the first question, but the first question is the kind of question that forces us to look for mechanisms.]**

Are there mechanisms at the upper levels of analysis (i.e., can we say, for instance, that the lack of social solidarity is the *mechanism* of suicide, and the protestant ethics is the *mechanism* of modern capitalism, or would the mechanisms be, instead, the psychological meanings developed by individual minds given lack of cohesion or religious ethics?) **[The mechanisms are the psychological meanings in these examples. The relationship between Protestantism and capitalism is a correlation. It prompts the question: how does Protestantism generate capitalism? What are the cogs and wheels that transform that initial condition into the outcome? The answer is a specification of a mechanism.]**

Boudon starts his discussion saying that explanations in the individual level have greater chance of being final explanations, because (in my interpretation) they are reduced to the lowest possible level of analysis. **[Just one little nuance here: it is not that this is the lowest possible level. It is the lowest possible level that yields anything of explanatory relevance. When no new discoveries at the lower level would affect our causal explanation at the higher level, the higher level becomes the “final” level of explanation for the explanandum in question. The argument that this is the level of the “individual” in sociology is a bet that (a) supra-individual explanations never have this character – i.e. you can always generate improvements in the explanation by moving down to the individual level, and (b) sub-individual levels will never add to properly constructed individual level mechanisms. Neither of these points are obvious.]** He says that, in order to open the black boxes of social mechanisms, the Rational Choice Model seems to be a good option because its explanations are final, “we need to ask no more question about [it]” (p.177). Nonetheless, he continues, the utilitarian view of the RCM has several weaknesses, because people do not always act based on “strong rational reasons”, and this creates several paradoxes when the Model (or the mechanism?) of rational choice is applied to empirical situations. Therefore he proposes the “Cognitivist Model” (CM) to solve for this paradoxes.

My questions about these model are, first: does the cognitivist model corresponds to a search for psychological explanations of behavior?”. If so, I do not understand how can this mechanisms be better for sociological analysis than the RCM. **[The cognitivist models are a sub-type of RCM – one that allows for a broader inventory of reasons for action than utilitarian models, which restrict belief formation and preference formation to utility maximization. The critique is just that there are more**

mechanisms at work in reason-based intentionality explanations than the utilitarian model allows.] I see this last one as being reasonable because, using Weber again, we can put ourselves in the place of rational actors and predict what their action will be, whereas we could not do the same so efficiently for actors that act according to other motives such as habit, personal dispositions, and other particular situations. **[Why not? Why can't you put yourself in the position of someone with certain habits and ask yourself how would I act? I think the problem is that you have to put a richer content into the idea of habit before it is explanatory, whereas seemingly rationality has a more universal character, but I think this is incorrect. Rationality explains action only when you put a rich content into things like information. I don't see any fundamental difference between these different elements of individual-level motivation and action.]** This makes the theories much less generalizable, and reduces the power of the theory to make predictions. **[A theory that deploys habits is no less or more predictive than a theory that deploys utility-maximization motivations. Both only make predictions where certain properties of the situation are well-defined – the content of habit in one, the nature of preferences and information in the other. Both require specification of nonobservables, since preferences cannot be observed any more than habits. What is the difference?]** It would reduce us to make sociological theories that are explanations *ex post*. Is this a reasonable goal for sociology?

Finally, how can we say that an explanation is final? The fact, as Boudon suggests, that a final explanation is one that does not require further questions does not transform this explanation into a dogma, instead of a theory? *Does our definition of a final explanation depend on our standpoints?* **[This is an interesting issue, since it suggests that there are different sorts of reasons why a scholar might say “no further questions are needed”. One reason is that the *interests* of the observer are fully satisfied by the existing answers. This is the standpoint-type account of question-stopping. Another might be that there are no possible answers to the further questions that could change the answers already established. That would be the sort of claim that Boudon is trying to make.]**

14. G.C.

Running through the essays of the Hedstrom and Swedberg book is an adherence to the principle that explanations of social phenomena are efficacious only as long as they offer up nothing that is inscrutable, or occluded within a “black box”. **[I think “efficacious” is a little strong, since I think all of these writers acknowledge that black-boxes are often needed in a provisional way. What they say is that so long as there are relevant unopened black boxes, then we need to continue our work.]**

Gambetta, Cowen, and Boudon especially want to establish a foundation for efficacious explanation upon the rationality, or failing that the meaningfulness, embodied by individuals' actions. Boudon states that, at the very least, one should be “prudent” before attributing to actions black-box causes, such as “imitation instinct” or “national spirit”, that are not even meaningful to the actor. The question might be asked, however, whether one and the same cause, or mechanism, might *become* meaningful to the actor; whether sub- and supra-intentional mechanisms might be identified by actors as have de

facto influence on the outcomes of their actions and thus enter into, or vacillate into and out of the status of understandability, rationality, or at least meaningfulness. **[But also: it isn't at all clear to me why the mechanisms that actually explain action have to be meaningful to the actors themselves. It may be implausible that a subjective mechanism isn't somehow linked to something that is meaningful, but the mechanism itself need not be, it seems to me.]**

For instance, 18th century *doux commerce* theorists, such as Smith and Mandeville, postulated unintended social benefits, such as luxury and politeness, as the result of the pure egoism of individuals; that “private vice” leads to “public virtue”. Commercialism was seen as containing the antidote to its own ills. On the other hand, Hobbes and Rousseau regarded the ungoverned social interactions of individuals (in the state of nature for Hobbes, unregulated and untransformed society for Rousseau) as truly vicious and universally inimical to the subjective aims of individuals. This must be overcome by the universal recognition of dictates of right reason (for Hobbes) or the denaturing, de-particularizing, civic education that establishes a “general” will among citizens (for Rousseau). For Hobbes, a predicament (the solitude, poverty, nastiness, brutishness, and shortness of life in the state of nature) must be overcome by epistemic means. The necessity of ceasing to be as bellicose and aggressive as possible and, instead, instituting a sovereign that will wield the collection of the quanta of force that would lead to war if they each remained, as originally, in the possession of each respective individual. In short, the problem is vainglorious egoism: a meaningful, if not rational mechanism. The solution requires recognition of the ineluctable misery attending this vainglory and thus the necessity of behaving differently. There is one supremely consequential potential supra-intentional effect of behavior, viz. war; this potential effect will need to be made to condition all behavior by epistemic means, i.e. it must be recognized for what it is, if the necessary steps can be taken. The necessary step is the social contract which *artificially* avoids the *natural* outcome.

Now, Hobbes wasn't adducing the social contract as an historical actuality. He crucially showed that the arrangement of governors and governed in already-existing society did not occur due to nature, but to artifice. This isn't a trivial distinction. In fact, he believed that, given the same conditions as he described (such as existed in contemporary North America) epistemic innovation and, through artifice, action would have to be taken. Mandeville would have individuals give in to their profligacy, society never having the need to produce from within itself any regulative and restraining force. Jameson (1992) suggests that we not conclude that capitalism is “in human nature”. Well, it is almost six and this is horribly convoluted. If I were to try to say what I have to say it is: I wonder whether *de facto* social mechanisms fade into and out of intentionality and meaningfulness on the individual level, yet remain *de facto* mechanisms; whether the flux of action within society renders black boxes in fact inevitable; whether a lot of what happens in society exists in a black box. **[I am not sure if I quite understand the core concerns you raise here (you admitted it was a bit convoluted). Mechanisms figure in attempts to explain things, especially the associations among things. They are needed if we are to avoid endlessly confusing spurious correlations with causation. The readings generally make pretty strong claims about the sort of proposed mechanisms that can accomplish this in sociology, namely mechanisms centered on individuals. And an additional claim made in some of the readings is that these**

mechanisms must be linked to subjectively meaningful mental states in the actors themselves. Are you mainly challenging this latter point?]

15. Matt Nichter

Consider the following distinctions:

- a) macro- vs. micro-level causal claims
- b) long causal chains vs. short causal chains
- c) long-range theories vs. middle-range theories **[I am not sure that long-range is the antinomy of middle range: the “range” in middle range is a scope condition – explaining some things rather than everything, explaining some range of variation rather than all possible variations. “Long-range” sounds like a temporal dimension. Perhaps you should say “wide-range” or “broad-range” here.]**

Hedstrom & Swedberg seem to treat all three distinctions as if they are logically related, while Elster appears to do the same with the last two.

But I don't see any necessary relationships between any of these distinctions. One can make causal claims about macro events that have a short sequence of events standing between them, while one can make causal claims about micro events that have lots of intervening events (which may well be 'black boxes') standing between them.

Merton's distinction between long- and middle- range theories, as H& S describe it, has to do with those theories' degree of generality: are they exceptionless laws or do they hold only in more narrowly circumscribed situations? If this is correct, then again I don't see why one can't construct both long- and short- range theories about both macro and micro events and about both long and short causal chains.

So I wasn't always sure exactly what the authors' plea for mechanisms amounted to.

At least in Hedstrom and Swedberg's case, it seems like the confusion stems from a conception of mechanisms that is quite different from Bhaskar's (despite their nod to him). If I understand him correctly, for Bhaskar a mechanism is a micro-structural feature of a macro-entity (or system of entities) in virtue of which the latter gains its (macro) causal powers. In places Hedstrom and Swedberg seem to more or less agree (e.g. diagram p. 22) but in others they treat mechanisms more like intermediate steps in a causal process (e.g. p. 9 diagram) - hence the collapse of a) into b). **[Nice point here. I like the way you framed the Bhaskar use of mechanisms as finding the micro-structures by virtue of which macro-structures have their causal powers. The Coleman-type decomposition might, however, still be**

interpreted this way: All Macro → Macro causal relations involve the three steps in the Coleman diagram. The micro→micro part is the micro-mechanism that gives the macro-structures their causal powers. Maybe.]

[H & S use what appears to be still another usage of the term 'mechanism' at the end of their introduction: "Arthur Stinchcombe...shows how [elite universities, enterprises, and nation states]...all exemplify the workings of the same mechanism: monopolistic competition." Mechanisms here appear to be neither micro-structures nor intermediate steps in a causal chain, but rather the instantiation of a general causal pattern. In fairness to H & S, I haven't read the Stinchcombe chapter yet; maybe he does explain all these phenomena in terms of the same (homologous?) micro-structures.] **[To me, if Stinchcombe is right, what he shows is that the elaboration of the micro-mechanisms validates the claim that there are indeed real causal powers operating at the macro-level, since these three cases have such radically different kinds of micro-actors with such difference preferences and purposes, and yet because of the network properties of the macro-relations, all of these macro-structures follow "monopolistic competition" dynamics.]**

I can understand why one might associate distinctions b) and c), since the longer the causal chain, the more room there is for some extraneous factor to intervene and muck up the generalizations one is trying to construct. But if macro cause A triggers lots of micro-mechanisms, each sufficient to generate macro effect B, then even if some of the micro mechanisms are thwarted along the long path from A to B, it may still be possible to construct lawlike generalizations about the relationship between A and B.

I can also see why one might associate distinction a) with distinction b), since many macro-level events are not really single 'events' at all, but rather concatenations of events, i.e. processes. (The French Revolution is usually described as an event, but of course it also consists of events.) But even when the macro phenomena under consideration are processes, one may be still able to close the temporal gap between the start and end of the process without resorting to a micro description of the steps in the process. And one may not need to close the gap at all to explain why the process occurred or what effects it had. It all depends on the case.