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Response to Commentators

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It is a singular pleasure to have the opportunity to receive the critical attention of such a distinguished group of social scientists and historians. This is a very rich set of comments from which I have gained many insights, and I am pleased to have a chance to respond to a few recurring themes.

1. Are Non-analytical Sociologists Just Mistaken About The Nature Of Their Own Methods?

Part of my argument turns on examination of some recent examples of what I regard as being very good sociological research that is evidently rather distant from the strictures of analytical sociology. Karl-Dieter Opp notes that "sociological practice may be deficient," and therefore we should not take arguments based on the current practices of sociologists with too much seriousness. I have always looked at the philosophy of science as a discipline that needs to tack carefully back and forth between abstract epistemology and substantial immersion in the practices of working scientists. For better or worse, scientists implicitly define the rationality of the sciences, and philosophers should be very wary of yellow-carding their practices without strong justification. In this case I think the historical and institutional sociologists I have cited have it right: it is reasonable to attribute causal powers to at least some well-defined meso- and macro-social factors. So my reply to Opp on this particular point is that the practices of these sociologists certainly meet my own standards of methodological precision and rigor.

William Sewell is largely supportive of the positions I have argued in my article. I am happy about that, because I have great admiration for Sewell's contributions to the philosophy of social science and the philosophy of history. I often find that critically minded historians and social scientists are more insightful on issues of methodology and ontology than philosophers, because of their deep immersion with the concrete conceptual issues that arise in their research. This is true of Sewell, who has written some of the most incisive articles available on topics like temporality and causal reasoning [Sewell 2005]. But he is concerned about my treatment of the examples I have chosen from other branches of sociology. He is concerned that perhaps these examples do not actually represent alternatives to the basic AS strategy, but rather simply elliptical presentations of the same individual-based strategy. He reinforces this point by reminding readers that my approach too requires that explanations be compatible with their having microfoundations at the level of the actors. Does not this mean that Mann's explanations of fascism are incomplete until Mann has provided the full microfoundational story? I do not believe so, for reasons sketched in sections 3 and 4 below, because I argue for a weak form of the microfoundations requirement rather than a strong form. This hinges on the difference between "being compatible with there being microfoundations" and "being provided with explicit microfoundations." I support the former rather than the latter, and I think that gives a meso-level sociologist like Michael Mann full license to postulate causal powers for things like paramilitary organizations.

Sewell would prefer a meso-autonomist strategy that dispenses with individuals altogether; one according to which we can identify certain causal factors that do not need microfoundations at the level of the individual at all. His candidates for such factors fall under the large umbrella of culture: symbols, meanings, practices, rituals, traditions, and the like. I would say, however, that these items too require microfoundations. If we take the view that the obligations of *zakat* (charity) are a profound part of Muslim identity and that this element of Islam explains certain social outcomes, then I want to know how these elements of identity are conveyed to children and practitioners at the local level. What are the concrete social mechanisms of inculcation and communication through which a Bangladeshi child comes to internalize a full Muslim identity, including adherence to *zakat*? To what extent are there important differences within Bangladeshi society in the forms of identity present in Muslims – urban-rural, male-female, rich-poor? And equally interestingly – in what ways do those processes give rise to a Muslim identity in Bangladesh that is somewhat different from that in Indonesia, Morocco, or Saudi Arabia? Identities, cultures, and systems of meaning are no less embodied in the states of mind of actors than are the calculating features of rationality that underlie a market society. So the fault of AS in this sphere is not that they fail to recognize the inherent autonomy of systems of cultural meaning; it is rather that they adhere to a theory of the actor that does not give sufficient attention to the variations and contingencies that characterize actors in various social and historical contexts. I find Sewell's five axioms about cultural items to be suggestive and interesting, and I think they need to be fully confronted by an actor-centered sociology. But I do not believe they are incompatible with an actor-centered sociology. Rather, they are incompatible with a narrow rational-intentional theory of the actor. Take the independence of a code of behavior from the specific individuals who are subject to the code. It is true that one individual cannot influence the code, which is embodied in the thoughts and actions of countless others. But the reality of the code at any given time is in fact entirely dependent on those thoughts and actions (and artifacts created by previous actors).

Karl-Dieter Opp believes that the examples of good sociological research and theory I have cited do not actually invoke meso-level causes at all, because they are actor-centered. (This is the point that Sewell anticipated.) As I have said in the article in this volume, I take a strong position on social ontology (similar to that offered by AS): all social entities, forces, and processes are constituted by constellations of actors and their actions, and social entities possess causal powers and normative characteristics in virtue of the microfoundations that exist for them ranging down to the level of individual actors. I do not accept, however, that this ontological truism has a reductionist implication when it comes to explanation. As I note in the article, this is a common issue in all the special sciences, and the anti-reductionist position that denies the necessity of reducing the meso-level to the constituting micro-level. I return to this question in sections 3 and 4.

2. Are Meso-causes "Real" Causes?

Several discussants take aim at what is evidently the most controversial claim of my original essay, the idea that meso-level factors can exercise causal powers that possess relative explanatory autonomy. By this I mean that it is not necessary to trace out the pathways at the level of the individual through which some meso-level causal powers work. But the objections do not seem particularly damaging to my position. They are based on questionable distinctions between direct and indirect causes, generative versus conditioning causes, and between reducible and irreducible causes. I do not find these distinctions to be particularly important, however, when it comes to identifying causal powers anywhere in the sciences; so they are not the basis of convincing arguments against meso-level social causes either.

Opp argues that meso-level factors cannot have *direct* causal influences, even though they may have *indirect* causal influences. However, it seems to me that the distinction between "direct" and "indirect" is entirely pragmatic when it comes to causal relations. Is the effect of the rapidly falling steel hammer on the walnut "direct" or "indirect"? I would say that it is a direct effect; but of course the micro-physics of the collision between steel and nut shell will involve molecules, chemical forces, and micro-level material properties. So if a direct cause is one for which there is no lower level of causal activity into which the higher level causal power can be decomposed, then there are no direct causes of any sort. Rather than a seemingly logically definitive distinction between "indirect" and "direct," I would prefer to put the point in terms of something like this: "such-and-such causal claim needs further sub-analysis for the purposes of the current explanation" and "such-and-such causal claim needs no further sub-analysis for the purposes of the current explanation." This is plainly a pragmatic distinction.

Opp also suggests that analytical sociology asserts that there are only correlations among macro variables, not causal relationships. This is a much stronger assertion about social causation than I would have attributed to analytical sociology. If so, then surely analytical sociology is in serious straits when it comes to pretty normal statements such as these: "The financial crisis of 2008 caused a sudden increase in the US unemployment rate." In Opp's formulation we would be forced to say something like this: that financial crises are correlated with increases in the unemployment rate. There are two things wrong with this paraphrase. First, it denies the plain causal relevance of the financial crisis to subsequent unemployment in 2008 and following; and second, it asserts a regularity between macro states that is unlikely to hold in any strong and generalizable sense. For similar reasons, I am unconvinced by Opp's view that macro-causal claims could only be justified if there were strong lawlike correlations among them. This is much of the point of the mechanisms approach: we can discover causal relations among factors even when strong laws are unavailable. This is one of Jon Elster's central justifications for taking the causal-mechanisms approach in the first place.

Filippo Barbera is an important expert on analytical sociology, so it is significant to me that he too is unpersuaded by my view that meso social entities (structures) can have causal powers. But it turns out that his criticism too is quite limited and depends on a distinction between "causally generative" and "causally relevant." He holds that social structures cannot be causally generative – only actions by individuals can generate change – and therefore structures cannot be said to have causal powers.

In fact, this seems to be true by definition in Barbera's article. He concedes, however, that structures can be causally relevant to outcomes by constraining actions. Social structural and relational factors influence causal outcomes that are, however, generated by the actions of individual actors.

I am unconvinced at several levels. First, the distinction itself is puzzling to me (and not clarified in the Glennan piece to which Barbera refers) [Glennan 2002]. Why would one assume that "real" causes are limited to "generating" causes rather than "conditioning" causes? If limiting the supply of currency leads to a rise in commodity prices, why should we accept the conclusion that this circumstance is not really causal because it is conditioning rather than generative? So even if the distinction between "generating" and "conditioning" causes is a legitimate one, it doesn't serve to reduce the causal status of the latter kind of circumstance.

Second, Barbera's assumptions about what characteristics a "generating" cause must have are implausible to me. He stipulates that only actions can "generate" social effects. There seems to be a fundamentalist thrust to the distinction that sounds a lot like Hobbes: only objects bumping objects can be said to "generate" new outcomes. And this does imply, of course, that intangible social structures cannot generate outcomes. In the current context, it implies the kind of reductionism to the level of individuals that I am arguing against here.

I would be perfectly happy to say that:

> Installing the "ROAD ENDS AHEAD" sign on a highway < generates > a new traffic flow.

Barbera would say, I guess, that the signal is causally relevant but not generative, whereas I would not hesitate to say that it generates the new pattern of behavior. It does not do so by serving as one action in a series of actions. Rather, it sets a piece of information that gets incorporated into many actors's actions. It seems arbitrary to insist that such an influence is not "generative." So I do not think the distinction is a valid one. Constraining actions is in fact a way of generating new patterns of behavior.

This is a simple bit of highway management. But it validly models the action of a social structure that puts up a prohibition or inhibition of a certain kind of behavior. When the Department of Education creates a new regulation requiring that graduating seniors pass Algebra I, this causes a systemic change in the curriculum from elementary school on.

Or in the example of Michael Mann's complex causal explanations of the occurrence of fascism in Europe: to say that the complex structural and dynamic features associated with World War I constituted a partial cause of the rise of fascism seems entirely believable to me. And Mann does, of course, make the case by disaggregating some of those features and showing how they lead to various strands of belief, activism, and action at a range of levels of actors.

Barbera's concluding observations seem much more sympathetic to the central thrust of my view of meso-level causation than the body of his comment: that meso-level (structural and relational) properties causally influence meso-level outcomes, and often the microfoundations for these influences are "trivial". His remaining concern, then, is simply whether these relations are "generative" or merely influential. And I am not yet persuaded that this is a compelling distinction.

So I will make my position even more explicit, to allow readers to answer for themselves whether they find it plausible. First, meso-level social structures like paramilitary organizations or regulatory agencies can *cause* a variety of meso-level outcomes (erosion of police control of the streets, increase/decrease of chemical plant accidents). Second, meso-level structures are *constituted by* individuals occupying roles within those structures whose behavior is governed by the rules and processes of the structure. These represent one level of the microfoundations of the structure or organization. The fact that there are microfoundations for the causal powers of the organization is supportive of the causal claim, not undermining of the claim; it does not imply that causation at the meso-level is less real.

3. Is The Microfoundations Requirement Incompatible With Meso-causation?

Several commentators allege that my commitment to microfoundations – which is unwavering – vitiates my ability to claim relative explanatory autonomy for the meso level. Gianluca Manzo does not like my distinction between weak and strong microfoundations (henceforth MF), and others think that commitment to MF means explanations have to proceed through explicit discoveries of the MF pathways.

My position is intended to exactly parallel physicalism in cognitive science. Almost all of us are committed to the idea that all cognitive processes are somehow or other embodied and carried out by the central nervous system. But we are not obliged to actually perform that reduction in offering a hypothesis and explanation at the level of cognitive systems.

Even more prosaically: we believe that the properties of metals depend upon the quantum properties of subatomic particles. Does anyone seriously believe that civil engineers are not giving real causal explanations of bridge failures when they refer to properties like tensile strength, compression indices, and mechanisms like metal fatigue? We can observe and measure the metal's properties without being forced to provide a quantum mechanical deduction.

Filippo Barbera writes that "Little's examples actually confirm that meso-level mechanisms work only through micro-level processes," a sentiment repeated by Manzo as well. Yes, and I likewise confirm that cognitive processes work only through neural events, and material properties work only through quantum physics. But I do not accept that this demonstrates that the higher level cannot be treated as having real causal properties. It does have those properties; and in insisting on the availability of microfoundations we simply reaffirm the point that somehow or other those properties are embodied in the lower level elements. This is not a new idea; it was contained in Jerry Fodor's "Special Sciences" article years ago [Fodor 1980]. If the argument is generally a bad one then we are forced to undo a lot of work in cognitive science. If it is generally compelling but inapplicable to social entities then we need to know why that is so in this special case of a special science.

To be clear, I too believe that there is a burden of proof that must be met in asserting a causal power or disposition for a social entity – something like "the entity demonstrates an empirical regularity in behaving in such and such a way" or "we have good theoretical reasons for believing that X social arrangements will have Y effects." Moreover, some macro concepts in sociology are likely cast at too high a level to admit of such accounts. That is why I favor "meso" social entities as the bearers of social powers. As new institutionalists demonstrate all the time, one property regime elicits very different collective behavior from its highly similar cousin. And this gives the relevant causal stability criterion. Good examples include Robert Ellickson's analysis of Shasta County and liability norms and Charles Perrow's analysis of the safety characteristics of technology regulatory organizations [Ellickson 1991; Perrow 1999]. In each case the micro foundations are easy to provide. What is more challenging is to show how these social causal properties interact in cases to create outcomes we want to explain.

The best reason I am aware of to doubt stable causal powers for social entities is founded on the point that organizations and institutions are too plastic to possess enduring causal properties over time. I have made this argument myself on occasion. But researchers like Kathleen Thelen [2005] demonstrate that there are in fact some institutional complexes that do possess the requisite stability. The safety organization literature does so as well.

4. Is The Weak Microfoundations Requirement Flawed?

Gianluca Manzo does not like the distinction I rely on between a strong and a weak version of the microfoundations requirement. With respect, I continue to believe the distinction is crucial. Consider these two formulations of physicalism in the philosophy of mind:

- Any statement about mental properties must be compatible with there being a physical embodiment of those properties;
- Any statement about mental properties must be decomposed into the physical properties that embody those properties.

The first is an acknowledgement of a high-level ontological principle about what mental stuff consists of; the second is a constricting and unnecessary limitation of the scope of the cognitive sciences. The first is a weak principle that sets an ontological framework around our reasoning about mental processes; the second is a strong reductionist principle that places a narrow methodological restriction on the content of theories in the cognitive sciences.

So how does this work within the social sciences? First, on ontological grounds, we cannot do without the microfoundations requirement. It is analogous to physicalism in the field of cognitive science: whatever mechanisms the cognitive scientist postulates at the level of abstract cognitive processing, there must be an embodiment of those mechanisms at the neurophysiological level. Strong MF requires that statements made at the higher level should be immediately disaggregated into statements at the lower level. Weak MF requires only that we keep the ontological constraint in mind as we formulate hypotheses about the meso and macro levels. We must make a reasonable case for there being microfoundations for our claims, but we are not obliged to provide the microfoundations. The strong interpretation conforms to the logic of Coleman's boat; the weak interpretation does not. To require, as Manzo seems to want to do, that any sociologist who is inclined to attribute causal and explanatory power to things like ideologies, religious systems, organizations, or periods of civil strife must immediately take off his/her sociologist's hat and take on the uniform of the aggregation-dynamics modeler, is unreasonably reductionist. In short, Manzo's complaints about strong and weak microfoundationalism seem to be equally valid or invalid when reformulated as complaints about physicalism. I believe they are invalid.

I am somewhat surprised at Manzo's complaint in his penultimate paragraph that my account of how organizations can have causal powers is self-refuting, on the grounds that my stories all proceed through the situational behaviors of individuals. Of course that's what the story needs to look like, given that organizations are constituted by individual actors in social locations. So if we want to illustrate how a meso-level structure might have a persistent causal power, we need to tell a microfoundational story. Likewise, if we wanted to help a beginning physics student come to a better understanding of the conductivity of silver, we would tell him or her a molecular-level story. This does not refute the fact that conductivity is a stable meso-level physical property. And the parallel strategy for organizations does not refute the fact that "regulatory agencies of such-and-so design cause higher levels of accidents than regulatory agencies of a different design," which is a causal claim about a meso-level structure. My point in this part of the paper is simply to show how a concrete set of causal properties at the meso-level can be given microfoundations.

Manzo also finds it odd that I refer to the AS program as "reductionist" in a specific sense: it requires that we explain features of the macro-social level by demonstrating how they derive from features of the micro-level situation of individuals. This strategy is reductionist in a straightforward sense; it requires that we "reduce" characteristics of the macro-level to characteristics of the micro-level. If the programme of analytical sociology is willing to be non-reductionist, then it must be willing to attribute "relatively autonomous" causal powers to macro- or meso-level entities – which Manzo himself visibly is unwilling to do. So "reductionist" seems like an appropriate description of the explanatory methods advocated by AS.

So I continue to believe both things: that statements about social entities and powers must be compatible there being microfoundations for these properties and powers; and that it is theoretically possible that some social structures have properties and powers that are relatively autonomous, in the sense that we can allude to those properties and powers in explanations without being obliged to demonstrate their microfoundations.

5. Is AS Chauvinistic?

I am happy to read in Peter Bearman's comments that he too recognizes the somewhat "chauvinistic" tendency in the AS field, as well as his own rejection of that tendency. Bearman's unstinting recognition of examples of great sociology that do not conform to the central explanatory model of analytical sociology is both generous and pluralistic at its core. I also agree with Bearman's comment that sociology involves more than explanation, and that good descriptive studies are also important contributions to sociological knowledge.

Christofer Edling shares my concern that AS is sometimes presented as a "master plan" for sociology, and like me, he advocates a more pluralistic view of the methods and theories of good sociology. What he defends in AS is the commitment expressed by almost all of its practitioners to bring theory and empirical research closer together. So Edling favors pluralism, with a strong commitment to keeping theoretical reasoning in pace with empirical research.

Edling also notes something a bit paradoxical about AS that is pertinent to my reading of the non-AS sociologists. The advocates of AS express a primary interest in social rather than individual phenomena; and yet their explanations often "get stuck at the micro-level of individual action." Edling explains this fact by providing a rationale for the schematic "desire-belief-opportunity" theory of action. The abstraction of this theory of action is what offers AS the possibility of modeling macro-level outcomes: like economists modeling price behavior, analytical sociologists can model simple social situations by representing the actors as being similar in their modes of action and choice. My response to this point is that sociologists need to be more attentive to the theories of the actor that they presuppose; computational tractability is certainly a desirable feature of a model of social activity, but we still have to have a theory of the ways the actors actually behave if we expect the model to conform to the social aggregate.

So Edling and Bearman (as well as Reed, Santoro, and Sewell) acknowledge the sometimes tendency of AS advocates to suggest that AS needs to be the model for all sociological theorizing. However, Gianluca Manzo believes that I am uncharitable to AS in arguing that it is presented as a general model for how good, scientific sociology should be conducted. I do not think I am making a caricature of AS by quoting statements from the introduction of a defining recent collection in the field [Demeulenaere 2011]. Further, I read *Dissecting the Social* in much the same vein: advocating for a "bottom to top" strategy of explanation for all social explanation [Hedström 2005]. Recall, for example, Hedström's dismissal of Bourdieu on the grounds that sociology requires analytical clarity above all else. Bourdieu, in Hedström's assessment, is not clear; therefore his work does not support good sociology (Santoro makes this point about the dismissal of Bourdieu by AS in greater detail in his paper.)

So it seems evident to me that at least some key advocates of AS do in fact believe that AS can and should serve as a template for good sociological explanation, and that research that does not conform is defective in some basic way. But that is not the important point. Rather, the important point is the reaffirmation of the validity of multiple approaches to sociological research, theory, and explanation – which I refer to as "theoretical pluralism" – and it appears that Manzo agrees with this point and seems to agree that advocates of AS should do so as well. So perhaps we can agree that AS should not be presented as a general template for sociological research, and work within a more pluralistic understanding of sociological theory and reasoning.

One place where Manzo and I do disagree in this area has to do with the assessment of sociological research generally. He finds that much published work is pretty weak ("fugitive frameworks"), whereas I believe there are very good examples of recent sociological research that are in fact helping to extend the theoretical and methodological frontiers of the discipline, and are doing so in ways that are intellectually rigorous. This was the point of my extended discussion of a range of examples.

So I continue to believe that it is worth affirming the kind of theoretical and methodological pluralism I have asserted here. And that in turn entails rejecting the idea that AS provides a general template for all sociological research projects.

6. Not Pluralistic Enough?

Isaac Reed poses several important questions. One is whether social mechanisms are ubiquitous across all social phenomena that we would like to explain, or are instead just a subset of explanatory facts about the social world. My response is similar, I suspect, to the one that AS practitioners would offer as well. We identify "mechanisms" when we are interested in the questions of why and how a certain social outcome came about. Explanation is commonly an effort to discover causes; and social mechanisms are the ways that causes work in the social world. What were the concrete processes that conveyed a given social setting from the pre- to the post-change state of affairs? Here is an example from Reed's own area of study: given a set of religious, political, ideological, and economic circumstances in colonial Massachusetts, what were the concrete processes that took place and culminated in the Salem witch trials? These are the mechanisms that we are looking for in trying to explain why and how the witch trials came to occur. We might want to say that those more or less stable initial conditions of ideology and politics were themselves causes of the witch trials, in the sense that the heavy rain that prevailed on Thursday evening was a cause of the truck crash late in the evening; but the mechanisms approach requires us to provide an account of how those standing conditions disaggregate into specific pathways of causal influence culminating in the event or outcome we are examining.

In the case of the Salem witch trials, one might hypothesize a social mechanism of mimesis, through which villagers absorb and retransmit the utterances and behaviors of their neighbors. It is then up to the investigator to see whether there is concrete empirical evidence of this pattern of inter-personal behavior in the historical case. Reed places heavy emphasis on the role of "context" in good sociology, as I do in my own contribution. I do not think, however, that this point is inconsistent with the search for mechanisms; rather, it seems to imply that we need to recognize that mechanisms like "mimesis" may actually play out differently in different social and cultural settings. Agency and mechanisms are context-sensitive.

Reed also asks the question of how I conceive of the "purposive actor", and whether this concept is intended to play a cornerstone for the social sciences. I do in fact believe that the social sciences need to be "actor-centered" in important respects. This is one reason why I am philosophically attracted to many of the assumptions of analytical sociology. Rather than following this view to the conclusion that we need to ground our explanations in theories of the rational, purposive individual, however, I take the conclusion that we need to have better and more nuanced theories of the actor. This means recognizing that "purposive rationality" is itself a cultural reality, inflected differently in different historical and social settings. It means that we can and should take very seriously the theories of the actor currently being constructed within the "new pragmatism," including habit, creativity, and imitation in our toolbox for interpreting individuals and their actions. Moreover, we need to look here as well for the microfoundations – the concrete social institutions and processes through which individuals come to embody one form of agency or another in specific settings. So I suggest that not too much weight should be placed on "purposive" as opposed to "intentional," "habitual," "imitative," or "impulsive." In each description the actor *does* something, generated by a set of conscious and perhaps unconscious mental processes and contents. A theory of the actor is intended to provide an account of the states of mentality on the basis of which actors do things, recognizing that these states may work differently in different cultural communities.

Reed's argument that my notion that we understand pretty well how organizations function depends on a sparser theory of the actor than I have just described is perhaps correct. A collective farm that was populated by agents who embodied Chairman Mao's ideal of "socialist man" would have functioning characteristics very different from those observed – no "easy riders," lots of earnest Stakhanovites. So standard organizational analysis of the tendencies towards low productivity in collective agriculture is dependent on something like a purposive agent theory of the actor. This does not mean, however, that we could not have reasonably good understandings of "organizations" under differently realized structures of agency. This seems to be part of the work that Andreas Glaeser [2011] is doing in *Political Epistemics: The Secret Police, the Opposition, and the End of East German Socialism*. Glaeser tries to understand how organizations like the Stasi functioned in a setting in which participants's understandings and motivations were changing rapidly.

7. Is There Any "There" There In AS?

Two response articles, those by Lizardo and Santoro, make more radical objections to AS than I was inclined to do. Though I do not identify myself as an advocate of AS, I will consider their objections to the programme as a whole that they express.

Lizardo and Santoro both take a step back into the sociology of science or the sociology of knowledge in order to formulate their critiques of analytic sociology. Lizardo refers to the idea of a "scientific/intellectual movement" from Frickel and Gross [2005]. Lizardo's chief criticism of AS is that it does not advance any premises or insights that he would regard as genuinely innovative. Rather, it is a restatement of *status quo ante* assumptions about sociology, largely (in his assessment) derivative from Robert Merton. As he writes in his conclusion, "AS can be best thought of as an explicit codification of the practical unconscious of Mertonian sociology." (Interestingly, Santoro's careful unpacking of the intellectual ancestry of AS shows that this is not an adequate treatment of the threads of sociological theory that contributed to current formulations of the AS approach.)

Lizardo's criticism sounds quite a bit like those of Imre Lakatos and Lakatos's idea of a "degenerating problem shift" within a scientific research programme [Lakatos 1974]. Is this a fair assessment of AS? It does not seem so to me. The Demeulenaere volume is a good exemplar of research being done under the general framework of analytic sociology [Demeulenaere 2011]. This volume presents a fairly wide range of empirical research that is being conducted within the broad framework of assumptions associated with AS. The researchers represented in this volume by no means demonstrate a slavish adherence to every line of the AS catechism; but they are close enough to warrant classifying them together within the research programme of AS. And they demonstrate just the kind of innovation and empirical progress that Lizardo seems to find lacking in AS in his own survey of the field. So it seems to me that Lizardo is too dismissive of AS as a programme within contemporary sociology.

I would also comment that Lizardo misconstrues my treatment of the instances of good social-science practice that I highlight – Mann, Brenner, Steinmetz, etc. I do *not* regard these as "really" being analytical sociologists without knowing it. Rather, I regard them as sociologists who pay attention to meso-level structures, social and historical context, and a much wider range of theories of the actor than AS scholars have themselves countenanced. They do not fall within the ambit of analytical sociology. But I also believe that these researchers reflect in their work core a body of commitments to an actor-centered social ontology that I commend – the need for a theory of the actor, a recognition of the meta-level fact that there must be a connection between social structures and the individuals who constitute them, and a recognition of the value of studying the linkages that exist between higher-level structures and on-the-ground forms of structured human agency. These are meta-level beliefs about social ontology that, contrary to Lizardo, are not universally acknowledged in mainstream sociology. Examples of researchers who do not incorporate these premises range from empiricist practitioners of the "variables paradigm" that Andrew Abbott criticizes to the practitioners of structuralist Marxism or more extreme versions of world-systems theory. Moreover, Lizardo seems to be mistaken in thinking that "meso-level" explanations are accepted by AS practitioners; AS scholars do not want to assign relatively autonomous causal powers to meso or macro social entities. That is the whole point of my critique of the "Coleman's boat" model of social explanation. AS practitioners seem to espouse a vertical model of explanation in which meso-level characteristics need to be explained as the aggregate consequence of micro-level individual activities. Surely the fact that "mesolevel" occurs in a title is not evidence of anything!

So I would disagree with Lizardo in his assessment that AS is just a paraphrase of "good sociological practice"; in fact, there are many good practitioners of contemporary sociology, including those whom I discuss, who do not espouse or adhere to the strictures of AS. So AS is more distinctive and more innovative in its methodological program than Lizardo admits.

Marco Santoro's essay is highly illuminating and will be of great interest to readers of this symposium. Santoro's treatment of AS as a "would-be theory-group", in the sense used in current discussions of the sociology of knowledge and the sociology of science [Mullins 1973], is detailed and valuable. His efforts to locate the emergence and development of AS within specific research institutions and through networks of specific leading scholars are exemplary.

Santoro's crucial critical point, it seems to me, is that sociology as a discipline is much more theoretically pluralistic than the orientation of AS as currently formulated can accept. It includes descriptive work (a point that Bearman makes as well), and it includes analysis of social phenomena at a range of levels of aggregation (my central point in my own essay). Moreover, Santoro demonstrates that there are elements of sociological theory outside of the current AS toolbox – for example, concepts subordinate to the idea of "culture", including frames, codes, stories, scripts, logics, and symbols – that have been carefully and clearly developed in spite of their not mapping easily onto the individual-centered social ontology favored by current AS theorists. Finally, Santoro demonstrates that AS has an intellectual heritage that is broader than its current practitioners recognize, including Pareto and leaders of the Chicago School. Santoro's account is an important and valuable contribution to the discussion of where analytical sociology fits into the larger development of sociology in the past century.

Santoro's criticisms of the corpus of analytical sociology are more focused than Lizardo's, but they are more telling in my opinion. First, Santoro is sharper in his critique of how AS scholars treat the rest of sociology than I was in my piece. He argues that AS scholars have consistently undervalued the work of social scientists like Geertz, Giddens, and Bourdieu, on the grounds that their theories are analytically unclear. And he believes this judgment comes from not having taken the effort to read them seriously. He writes, "I do not know how the scholars who write on behalf of AS are really knowledgeable about those research strands. My sensation is that an approximate knowledge and an impressive reading is enough for many to disregard them." This seems like a valid criticism of AS to me as well. Sociologists like George Steinmetz have in fact taken the trouble to work through Bourdieu's writings in detail; and Steinmetz is able to assign specific and rigorous meanings to many of Bourdieu's central concepts.

Santoro extends this critique in his discussion of the metaphor of "toolkits" in the AS literature. He argues that the toolkit metaphor is indeed a useful one, but that it is underutilized by AS researchers. "Instead of a well-assorted box, we find in it only a few selected tools, to which it is asked to do a very heavy work." Essentially, the criticism is that a few tools from rational choice theory and game theory are privileged, to the disregard of other important conceptual tools advanced within the "culture" end of the sociological spectrum. Here Santoro mentions "norms, beliefs, values, expressive symbols, meanings, frames, skills, rituals, worldviews, codes, scripts, logics, signs, stories, boundaries." I agree with this criticism as well. Goffman's treatments of frames and scripts add a great deal to our understanding of how socially situated actors behave, and much more can be done with these ideas as building blocks of a theory of action than has occurred to date.

The most penetrating critique that Santoro puts forward, however, is also a very important one. It is the view that AS has turned out to be a highly Eurocentric approach to sociology. He points out that the core practitioners are Northern European, and the whole movement has thrived in elite universities in Europe and North America. The AS framework has not been successful in incorporating the inclusion or the insights of social scientists from Asia, Africa, or Latin America. This is a serious intellectual shortcoming. Sociology needs to extend itself as a collaborative enterprise that incorporates the theories and perspectives of globally situated researchers, and AS has not succeeded in this respect.

One of the basic organizing premises of the sociology of science is that there are meaningful differences in the conduct of a given area of science across separate communities, all the way down. There is no pure language and method of science into which diverse research traditions ought to be translated. Rather, there are complex webs of assumptions about ontology, evidence, observation, theory, method, and reasoning; and there are highly significant differences in the institutions through which scientific activities are undertaken and young scientists are trained. And global differences in the ways in which sociological research is conceptualized and conducted are genuinely important.

There is in fact sociological research underway that attempts to document the gaps that exist in the epistemological and methodological practices of sociologists in different parts of the world. Gabriel Abend attempts to take the measure of a particularly profound form of difference that might be postulated within the domain of world sociology: the idea that different national traditions of sociology may embody different epistemological frameworks that make their results genuinely different. Abend offers an empirical analysis of the degree to which the academic disciplines of Mexican and U.S. sociology embody significantly different assumptions when it comes to articulating the role and relationships between "evidence" and "theory." "[The] main argument is that the discourses of Mexican and U.S. sociologies are consistently underlain by significantly different epistemological assumptions" [Abend 2006, 2].

I agree with Santoro that it is an *intellectual* deficiency of analytical sociology that it has not succeeded in incorporating the involvement and perspectives of non-European sociologists into its research networks. It is also almost certain to me that once it begins to do so, the framework of thinking about theory and methodology will begin to change dramatically as non-European researchers inject their own theoretical and empirical priorities into the discussions. This is a form of pluralism that is inherently enrichening to a theoretical discipline like sociology. What would be most unfortunate is if the framework did *not* change and instead functioned as a regulative statement about how "good" sociology ought to be conducted, whether in China, Mexico, or Uganda.

I am grateful to all these distinguished scholars for taking the time and effort to seriously address the position I've argued for here. That is a real pleasure for me and I thank them all.

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Responses to Commentators

Abstract: The programme of analytical sociology brings sharp focus to fundamental issues in sociology as a scientific discipline. Its practitioners offer a clear paradigm of how sociological explanations ought to proceed, from individual actors to social outcomes. This essay considers limitations of the approach as a general framework for all sociological research, however. The essay surveys recent high-quality research by a number of sociologists and other social scientists and demonstrates that their explanations often do not conform to the aggregative model advocated by AS. Additional philosophical arguments are provided for justifying the point that we can attribute relative explanatory autonomy to meso-level social structures. The essay offers a brief description of an approach to social ontology referred to as "methodological localism."

Keywords: Analytical sociology, methodological individualism, methodological localism, philosophy of sociology, philosophy of social science, reductionism.

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