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# Analytical Sociology's Superfluous Revolution

(doi: 10.2383/36902)

Sociologica (ISSN 1971-8853)

Fascicolo 1, gennaio-aprile 2012

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# Analytical Sociology's Superfluous Revolution

by Omar Lizardo

doi: 10.2383/36902

This commentary is less a disquisition on Daniel Little's (generally informative and even-handed) assessment of the virtues and limitations of Analytical Sociology (henceforth AS) than it is an exercise in self-clarification. For the reader not previously exposed to the wonders of AS may find him or herself, after reading Little's article, faced with a puzzle. The puzzle is this: if Little is right, and AS does in fact boil down to the four set of points so clearly outlined, why are we talking about "Analytical Sociology" in the first place? To phrase the question in a way that is more consonant with my own experience, why is it that my bookshelf is now stocked with at least three (maybe more I should check) books that sport the phrase "Analytical Sociology" in the title, and yet the more I read about it, the less I think it is worth reading about?

The clash between what I perceive to be the highly circumscribed intellectual significance of AS and the reality of AS as an intellectual entity in the current sociological marketplace opens up a puzzle worth delving into.<sup>1</sup> I will devote the bulk of this comment to addressing this puzzle. To telegraph my ultimate conclusion: AS has an objective status (e.g. influential, prestigious, attention-getting) that is incommensurate with its intellectual significance because it represents an almost textbook example of a failed "scientific/intellectual movement" (henceforth SIM) in Frickel

<sup>1</sup> I am of course asking for the reader's indulgence. For it could certainly be the case that I am just a useless candidate for the job of providing an adequate assessment of intellectual movements in sociology. If the reader concurs with this proposition, then he or she should probably stop right here.

and Gross's sense [Frickel and Gross 2005]. Thus, while it has been able to mobilize and impressive amount of resources, garner the support of an impressive group of established and younger scholars in Europe and the United States, AS fails in its attempt to produce anything that is in fact intellectually innovative. This stodgy lack of innovation makes AS a good reflection of "good sociological practice" but makes it utterly inadequate as an advocate for a radical change in such practices. The reason for this is simple: AS is fighting a battle that does not need to be fought, because much of what it proposes is already standard practice in the mainstream. As such, we are presented a rather unique creature in the intellectual ecology of contemporary social science: the reality of an intellectual movement that has all of the external signs of a significant revolution in the field, but lacks what matters most – a credible claim to actually offer a new way of approaching sociological research. This lack of novelty renders equally incapable of arousing any meaningful reaction beyond the most perfunctory critical scrutiny.

I briefly expand on these set of points below. I do that in two parts. In the section that follows, I argue that AS meets some of the (most superficial) conditions for being a successful SIM. In the next section, I argue that in spite of its external signs of success, AS is an example of a failed SIM because it falls short of satisfying a crucial criterion: saying something that is actually intellectual innovative and capable of actually arousing resistance in the field. I close with some concluding remarks pointing to the puzzling nature of AS as a seemingly successful but ultimately failed SIM.

### **Getting Carried Away with Acronyms: AS as a Successful SIM**

From Little's article (and from the various pronouncements, textbooks, position pieces and introductory chapters written by advocates of AS), we learn that AS is not just any old entry in the theoretical marketplace. Its main advocates appear to perceive themselves as members of a full-fledged SIM, entrusted with the transcendent mission to revolutionize the practice of scientific sociology once and for all. One important question that emerges is: does AS meet the conditions for being an SIM? Frickel and Gross [2005] outline four main conditions (and two subsidiary ones that I will ignore) that define an SIM.<sup>2</sup>

First, any SIM worth their name must

<sup>2</sup> For purposes of expository convenience, the order in which I introduce these conditions is not the same as in the Frickel and Gross article.

[a]t their core [...] have a more or less coherent program for scientific or intellectual change or advance. However conceptualized and implemented, these programs involve the transformation of thoughts or research findings into ideas and knowledge that are circulated widely within the intellectual community, subjected to scrutiny and contestation, embraced by some and rejected by others, and that may emerge from the process deemed credible or true [*ibidem*, 206].

It is clear that AS at least partially meets this criterion. Its program for scientific and intellectual reform of the discipline distinguishes itself by its relative clarity, coherence, and the programmatic nature of its aims. Advocates of AS certainly see themselves as transformative figures and see AS as radically breaking with business as usual. Position pieces, review articles and other such communications discussing the merits of AS are now widely circulated within sociology, and as demonstrated by this very symposium, are now the subject of detailed scrutiny. AS has been embraced by some as a new credo, capable of serving as revolutionary force in the field. Thus, by this criterion, AS certainly counts as a successful SIM.

Frickel and Gross tell us that, in addition, SIMs must also be

constituted through organized collective action [...] The ideas of movement leaders take shape against the backdrop of their positioning in high-status intellectual networks to which they had to be admitted by someone. To become influential, those ideas then had to make their way into publication, requiring the cooperation of peer reviewers, editors, and publishers. New technologies such as instruments and specimens and new editions of books linked to the SIM need to be refined, produced, made commercially viable, and distributed to researchers. Jobs for SIM participants have to be found, conferences organized, special issues of journals edited, grant support obtained, and the like [*ibidem*, 207-208].

There is no question that AS is a successful SIM by this standard. Edited volumes have been edited. Programmatic texts have been authored (and co-authored). Review articles have reviewed literatures. Prestigious scholars have come out in public as card-carrying practitioners of the approach. Conferences have been organized. Prestigious predecessors (dead and alive) have been selected [on predecessor selection as a signature of intellectual movements see Camic 1992].<sup>3</sup> Recent journal articles published in the top sociological journals have even made somewhat ritualistic citations for the purpose of signaling that they are taking an “analytical sociology” approach. Thus, there is no question that AS has engaged in a lot of “resource mobilization” and concerted collective action to establish itself as a powerful player in

<sup>3</sup> In fact, if reading through attempts at intellectual lineage-making such as Manzo’s [Manzo 2010, 132-138] one gets the impression that analytical sociologists have actually gone overboard with this practice, resulting in a pattern of “predecessor inflation.” When one can claim both Parsons *and* Merton as intellectual predecessors (!) you know that there is something funky going on.

sociology's "strategic action field" [on the notion of strategic action fields, see Fligstein and McAdam 2011].

### **Why Books and Conferences Are Not Enough: AS as a Failed SIM**

Things get more problematic when we examine two of the other criteria definitional of an SIM. For Frickel and Gross also tell us that the core program of a possible SIM must consist of "intellectual practices that are *contentious* relative to *normative expectations* within a given scientific or intellectual domain." An intellectual movement can only count as an SIM "only if, at the time of its emergence, it significantly *challenges received wisdom or dominant ways of approaching some problem or issue and thus encounters resistance.*" This implies that, "[p]recisely because the intellectual practices recommended by SIMs are contentious, SIMs are *inherently political*" [Frickel and Gross 2005, 207, italics added].

My basic argument is that even though AS meets some of the minimal conditions for being a successful SIM (it fits the first two criteria discussed above) it is an anomalous instance of one because it summarily fails to satisfy these last two criteria. In my view, these criteria are as central (and possibly more central) as the first two. For an intellectual movement that engages in a lot of collective action, publishes edited volumes, and organizes conferences without actually having an original intellectual contribution to make, cannot be considered a successful intellectual movement.

This rather summary judgment on the lack of "originality" in AS should not be taken as a denigration of the approach. In fact, rejecting something for being "unoriginal" is a tack that is resorted to by those who confuse standards of innovation taken from fields outside of sociology for properly sociological criteria [for a wonderful example, see Goldthorpe 2007]. I happen to believe that the epistemic, theoretical and research recommendations of AS are representative of *sociological best practice*. As will argue below, they in fact represent a codification of the best that the Mertonian tradition of social research (constitutive of the core of empirical sociology in the United States) has to offer. This is a tradition of which I am admirer and one that I take as normative and foundational in my own work and that of others.

The problem is precisely the putative packaging of AS as a transformative intellectual movement, when all that is doing is explicating and codifying the *status quo ante*. No movement that takes up a banner consonant with an intellectual tradition that is constitutive of the core of *mainstream sociology* in the United States can somehow also claim to be an oppositional startup. I believe the packaging of AS as

an SIM is misguided, because it ends up making platitudes into revelations and ends up having to manufacture putative enemies where they are in fact none to be found.

This should be evident to anybody that takes the trouble to go through Daniel Little's four-step distillation of the approach. There is not a single thing that Little shows to be a basic tenet in AS, that anybody in mainstream sociology would really try to vehemently argue against. This creates the bizarre situation in which some initially non-controversial, trivial statement (e.g. individuals are the primary entities and their action are the primary activities in social mechanisms) has to be twisted into something more controversial (e.g. explanations have to bottom out in some "reductive" account of individual action), in order to generate some heat. But then, after we read a bit further and learn a little more, we find that even Peter Bearman (!) is an "analytical sociologist" and that therefore the whole panoply of network and relational mechanisms that make any explanation "sociological" is back in play. Phew! For a moment the reader might have thought that analytical sociologists were saying something controversial, but actually we find that they agree with the entirety of the membership of the American Sociological Association in claiming that "relations matter."

This fundamental lack of distinctiveness in intellectual approach, and the almost inveterate incapacity to say something that even might be misconstrued as novel, shows up in other insidious ways in AS. Consider, for instance, the theory of action and the theory of the actor espoused by AS. This, we learn, happens to be a version of *belief-desire-psychology* that Peter Hedström likes to refer as "Belief-Desire-Opportunity" theory (BDO, since we are going acronym-crazy). As Little summarizes it, the main tenet of BDO theory is that "given the desires the actor possesses, given the beliefs he/she has about the environment of choice, and given the opportunities he/she confronts, action A is a sensible way of satisfying the desires" [Little 2012, 4].

What neither Little nor Hedström tell us is that this "theory" (actually a set of platitudes that live in the realm of what Clifford Geertz called "commonsense") [Geertz 1983] also has a name among philosophers of mind and action: it is called *folk psychology*, and for good reason. For it happens to be the very same theory that everybody else uses to make sense of their own and each other's action (folk psychology thus counts as an "ethno-method" in Garfinkel's sense). Its status as a *scientific* theory of action is of course disputable [e.g. Churchland 1981], but going in that direction would take us too far afield. The basic point is that among the various action theories available in the intellectual marketplace,<sup>4</sup> this is the one that you would choose if you were purposefully trying to create the most boring theoretical system

<sup>4</sup> For God's sake people, it is not like we have no other options; see for instance, Gross 2009 and Joas 1996.

on the planet. The fact that analytical sociologists chose *the least controversial theory of action possible* (the folk theory!) bespeaks to their general aversion against saying anything that might even smell of new and controversial.

The theory of action espoused by analytical sociologists *when interpreted in the strong sense originally intended* is, predictably enough, one of the points that has in fact generated some resistance in the field. This seem to go against the claim that AS fails to satisfy the third criterion for being a successful SIM (namely, generating resistance). However, this would true if two other conditions obtained: 1) AS actually is a “tight” intellectual package that actually *required* any particular theory of action, and 2) analytical sociologists were actually committed to any strong (or even medium strength) version of DBO theory as initially stated by Hedström. The first claim is clearly not true. None of the meta-methodological recommendations offered by AS (pay attention to specifying mechanisms, “if you didn’t ‘grow it’ you didn’t explain it,” etc.) *imply* any one particular theory of action. So there is only a loose coupling between DBO theory and AS as a whole; in fact the commitment to DBO theory appears to be an idiosyncratic, path-dependent, “founder effect” (it happened to be the approach preferred by some of the senior scholars behind the movement and is clearly only peripheral to the interests of other proponents of the approach). Thus, the inherent commitment to DBO theory has been overblown by so-called “critics” of the approach. In fact, it is easy to show that analytical sociologists are actually not committed to a strong version of DBO theory at all.

If the reader is skeptical about this claim he or she only needs to consult Manzo’s “defense” of DBO theory [Manzo 2010, 150-157], which actually boils down to saying that AS actually does not really stand by any of the premises that seem to be implied in the above statement. So you thought that AS was committed to a view of desires as *preceding* action? No sir, they actually believe that desires may “arise from action rather than preceding it.” So you thought that AS was committed to a view of action that implies the *conscious* choice of means given opportunities and desires? No sir, AS can accommodate unconscious effects on action. So you thought that AS implied that an actor’s some consistency between beliefs and the world and between desires and beliefs (as required by folk psychology)? No sir, actually according to AS “beliefs and desires are not conceived as being invariably clear, transparent and consistent.”

My aim here is not to evaluate Manzo’s defense of DBO theory. In fact, I think that Manzo concedes so much during this defense that most practitioners of AS would actually not be willing to follow him that far. The point is that by the time one is done reading Manzo’s defense of DBO theory, the only reasonable conclusion is that AS actually *has no one theory of action*, or more accurately that it is such a

labile approach that it can transform itself into *any* theory of action, because the larger meta-methodological framework implies *no commitments at this level*. Note the *effect* that this has, which is to *defuse* any sort of resistance or distinctiveness that the approach might have vis a vis other potential alternative approaches in the marketplace. As Manzo makes clear, commentators such as Neil Gross are mistaken not because they criticize AS, but because they think that it would not take two seconds for an analytical sociologist to mount an argument stating that there was no difference between DBO theory and whatever alternative they are proposing (e.g. pragmatism); for instance, Manzo's main claim is that DBO theory is already a version of pragmatism. This inherent intellectual "flexibility" of AS once again defuses its claim to offer anything new. Instead, when challenged, AS simply – chameleon like – takes the shape of the counter-approach suggested critic, and then criticizes the critic for even suggesting that there was a difference worth arguing about in the first place.<sup>5</sup>

At other times, given the lack of actual intellectual distinctiveness from other approaches, and given the absence of real, live people who actually disagree with AS, the analytical sociologist is forced to invent ghostly opponents that *could* disagree with what they are saying. Enter the panoply of non-existent deductive-nomological "positivists" who ignore mechanisms and are forever lost in a maze of covariance structures, Durkhemian holists who explain "social facts" with other "social facts" and ignore lower levels of analysis, or mushy-brained and mushy-minded "theorists" who read too much Habermas and Bourdieu and end up spouting imprecise and unclear theoretical formulations. Precisely because this opposition is not a real opposition in sociology's contemporary strategic action field, one of Frickel and Gross's primary criteria for being counted as an SIM – namely that the SIM generate *actual* opposition and polarization from other *organized* contenders – will never be met.

This last fact is evident in Little's own analysis. This paper should worry analytical sociologists for a simple reason: the relative ease with which Little was able to show that a heterogeneous panoply of sociological exemplars, with nothing in common other than the fact that they were "very strong" examples of good sociological research, could be shown to actually be "doing" something very close AS without realizing it. The "differences" that Little was able to uncover (e.g. "attention

<sup>5</sup> Sometimes the attempt to generate some semblance of intellectual distinctiveness reaches unintendedly comic proportions, like when Hedström [2005, 4] grasping for something that can actually make the approach seem unique, tells us that one of the features that really differentiates AS from alternative perspectives is its emphasis on "clarity and precision." Clarity and precision? That is certainly sufficient to tell AS apart from the mass of sociological research that is unclear and imprecise (or from bad translations of French theory), but it certainly does not work well to make it different from the equally large mass of sociological research that is in fact clear and precise.

to context”) are tiny in comparison to the “commonalities” (the other major point “mesolevel” point does not really work; Peter Hedström literally wrote an article with the word “mesolevel” in the title). Nevertheless, if everybody who does “very strong work” is an analytical sociologist, then AS becomes synonymous with the term “very strong work,” and the movement fails as an intellectual innovative entry.

Another thing that should worry analytical sociologists: going by their basic meta-methodological criteria, there should exist people who meet the criteria but who (a) are not professional sociologists, and (b) do not know that what they are doing is “Analytical Sociology” (mostly because they would probably just call it “good social science”). This speaks to the point that AS happens to be an *explicitation* of practical guidelines for good research that people would be following anyways regardless of whether we had such a movement or not. Thus, Newman and Park’s *Physical Review E* paper proposing a model for the origin of transitivity and positive degree-correlations in social networks would have to count as AS, even though the paper was written by two physicists who would not know AS if it bit them in the face [Newman and Park 2003]. In fact, in my view, the analytical sociologist *par excellence* is probably Joshua M. Epstein who was doing it before it had a name [see in particular Epstein and Axtell 1996; and the papers collected in Epstein 2006].<sup>6</sup> One counter-intuitive fact that emerges from this consideration is that AS may in fact have nothing to do with “sociology” as traditionally defined! In fact, one likely future for AS is simply to merge into the now self-reflexive transdisciplinary movement known as “computational social science” [e.g. Lazer *et al.* 2009] without much intellectual loss.

## Concluding Remarks

AS is a weird animal in the current intellectual marketplace; it is a quasi-intellectual movement that sees itself as transformative and has been able mobilize a relatively impressive array of resources, generate a lot of collective action, shove itself into the attention space of the discipline and produce a large amount of literature (the job market has been tough the last few years though so I do not know about the getting jobs for students part), *even though it is a movement that manages to pack together some of the least controversial, least challenging ideas – vis a vis the sociological status quo – in recent memory*. In fact, AS can be best thought of as an *explicit codification* of the practical unconscious of Mertonian sociology, a set of principles that has formed the normative epistemic backbone of American sociology for more than

<sup>6</sup> Ironically, Epstein is a graduate (Political Science) from MIT an institution that, incidentally, lacks a Sociology department.

half a century now. Because of that, AS has failed at both producing an intellectual “splash” (e.g. generating heated resistance from others) and of distinguishing itself from the mainstream core of practices in contemporary (American-dominated) sociology. From this perspective, the future of AS is also easy to predict: it will descend back into the disciplinary unconscious from whence it came after all of its “insights” become taken for granted as “things that you do when you do solid, good, strong, work, within the constraints of puzzle-solving normal science.”

Note that this stance has the virtue of explicating something that has been rarely noted in commentary revolving around AS. If we were to accept the analytical sociologist’s version of the recent history of sociological research, then sociology would emerge as a pathological discipline because it would demonstrate a pattern of intellectual development that is the precise *opposite* of that which characterizes most other disciplines not called physics. Consider for instance the issue of “mechanismic explanation,” a putatively central revolutionary tenet in AS. As shown by Craver [2007], Bechtel [2008], and Bechtel and Richardson [2010], the recent history of science shows that the emergence of successful programs advocating mechanismic explanation (and other allied discovery strategies such as *localization* and *decomposition*, versions of which are clearly advocated by analytic Sociologists) in such fields as cellular and molecular biology, the cognitive psychology of memory and cognitive neuroscience emerged as largely *independent*, *organic*, and *unselfconscious* research strategies in these fields. What is remarkable is that these fields, as part of their natural historical development, developed strategies of research that contradicted the dominant (Hempelian) models of explanation in the philosophy of science, demonstrating that successful scientific practice has nothing to do with the philosophical reflections *on* that practice. It was only *later*, after the unabashed explanatory success of these fields that philosophers have come around to document and codify these strategies and to realize the standard Hempelian model is the exception rather than the rule.

Yet, analytical sociologists would have us believe that in sociology the meta-methodological enlightenment has come *first* (in the form of AS) and the practice has *followed* (or should follow, because apparently only the enlightened few practice it properly). A more parsimonious explanation is that sociology is just like all other scientific fields that have to deal with complex, multilevel systems: i) it has always had an unselfconscious analytical tradition, at least beginning with Merton’s post-functionalism (which explains AS’s *predecessor inflation*); ii) that tradition was transmitted as a practical paradigm for the generation of successful research programs via the standard mechanisms described by sociologists of science (e.g. invisible colleges, cross-generational apprentice relationships, etc.); iii) that tradition has been the most explanatorily successful and has come to form the core of the disciplinary mainstream;

and, finally, iv) it is only now – *ex post facto* – that a self-conscious philosophical cum meta-methodological movement (AS) has arisen to do the relevant conceptual house-keeping, assign the relevant philosophical labels and write the Whiggish histories.

AS thus emerges as a bizarre case of a movement that perceives itself to be revolutionary while peddling the conventional (Mertonian) wisdom that forms the core of good research practice in American sociology. *Precisely because AS succeeds in codifying the mainstream it fails as an SIM* (which are supposed to be transformative). The reasons for this failure are instructive. AS is a failed SIM not because it is not backed by prestigious scholars in prestigious institutions, not because it is not endowed with resources and bully pulpits, and not because it has not engaged in its own project of “predecessor selection,” but because it is a *superfluous* revolution. In fact, the only interesting thing about AS might be the empirical puzzle – from the point of view of the sociology of knowledge and ideas – that it represents: how can a movement so conventional perceive itself as so transformative? Maybe one day we will have a very strong (i.e. meeting AS-inspired criteria for good work) dissertation on this subject.

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## Analytical Sociology's Superfluous Revolution

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Abstract: Daniel Little's characterization of Analytical Sociology (AS) as essentially hard to distinguish (beyond some relatively minor points) from "sociological best practice" as traditionally conceived opens up a puzzle, since AS is usually thought of as a insurgent (and presumably novel and counter-normative) Scientific Intellectual Movement (SIM). In this comment, I address this puzzle. I argue that AS is an anomalous SIM, one that has all of the *external* characteristics of successful cases (e.g. resource mobilization), but whose intellectual content represents an explicit codification of standards of research that have formed the core of the sociological mainstream form more than two generations. AS thus fails at a key criterion defining a successful SIM, namely, saying anything that could plausibly arouse resistance or opposition from the sociological mainstream.

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*Keywords: Analytical sociology, scientific intellectual movements, theory of action, social explanation.*

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**Omar Lizardo** received his PhD from the University of Arizona in 2006 and is currently an Assistant Professor in the Department of Sociology at University of Notre Dame. His primary research interests are the sociology of culture, the sociology of knowledge, sociological theory and world systems theory. In a recently published paper he dealt with the effect of cultural tastes on the composition of social networks (*American Sociological Review*, 2006). He is currently working on an extension and elaboration of contemporary theories of culture consumption geared toward the understanding of transnational patterns of cultural trade. Additional work in progress deals with the question of the role of cultural capital in the creation of social capital and on the instrumental use of social ties.