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# The Whole and the Parts. Or: Is Analytical Sociology Analytical Enough about Sociology, and Itself?

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# The Whole and the Parts

## Or: Is Analytical Sociology Analytical Enough about Sociology, and Itself?

by Marco Santoro

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For the believer there is but one good course; all others are bad. The Mohammedan will not take oath upon the Gospels, nor the Christian upon the Koran. But those who have no faith whatever will take their oath upon either Koran or Gospels – or, as a favour to our humanitarians, on the *Social Contract* of Rousseau; nor even would they scruple to swear on the *Decameron* of Boccaccio, were it only to see the grimace Senator Berenger would make and the brethren of that gentleman's persuasion. We are by no means asserting that sociologies derived from certain dogmatic principles are useless; just as we in no sense deny utility to the geometries of Lobachevski or Riemann. We simply ask of such sociologies that they use premises and reasonings which are as clear and exact as possible. "Humanitarian" sociologies we have to satiety – they are about the only ones that are being published nowadays. Of metaphysical sociologies (with which are to be classed all positive and humanitarian sociologies) we suffer no dearth. Christian, Catholic, and similar sociologies we have to some small extent. Without disparagement of any of those estimable sociologies, we here venture to expound a sociology that is purely experimental, after the fashion of chemistry, physics, and other such sciences [Pareto 1935, I, 6].

Daniel Little's paper does a good job in presenting the main tenets of analytical sociology (henceforth AS) in clear and engaging ways, focusing his criticism on one of its more characteristic and apparently central points: the claim that good sociological explanation of whatever social fact has to be based on detailed micro foundations, which AS's representatives read as individuals and their inter-actions. It is only individuals who act and relate to each others, it is argued inside the AS camp, and therefore it is only individuals who may bring about social outcomes – whatever their size, form, and structure. Examining a few contemporary works of American sociology

which admittedly do not recur to micro foundations to build persuasive sociological explanations, Little argues that AS would do better if its proponents admitted that also meso-level entities, such as organizations and other collectives, may produce social outcomes and therefore be the locus where to look for explanatory mechanisms.

His arguments look persuasive to me, and the works he chooses as exemplar of alternative explanatory strategies are in many cases (even if not all) among my favourite pieces of sociological research. As not a believer in – neither a practitioner of – AS, this agreement comes as no surprise to me. More surprising (to me) is that even after reading and enjoying Little's paper I feel still a bit unsatisfied in my quest for an understanding of AS, of what I experience as its limits, but also of what I feel as its appealing force. I suspect there is something more in AS, that needs some further thought.

To resume what I will say in the following pages: I think AS has indeed good things to offer to contemporary sociologists, such as a healthy plea for clarity in definition and coherence in argumentation, and a sustained reflection on the role and modes of causal accountability in the social sciences. AS has contributed and is contributing to address and clarify serious issues in sociological theory, making a case for the elaboration of a set of tools and strategies which enforce the toolbox of sociologists, while making more visible the faults and limits of others. But it also holds a rather narrow and idiosyncratic conception of sociology's tasks, and of what sociology has been, presently is, and will probably be in the future. I suggest that this is paradoxically a consequence of AS's rather partial, selective, unsystematic and not enough analytical vision of the discipline, of its history, of its constituent parts, of its actors, and of the diversified tools it can provide. This selectivity is apparent in the way AS makes sense of its own history, and in the way in which it presents itself.

### **Is AS Simply An Intellectual Strategy, Or Is It More?**

First of all, I would go deeper in the presentation of AS, focusing on its being – or better, on its self-presenting as – more than an approach or a strategy. Looking at the subject with the eye of a sociologist of knowledge (or ideas, if you prefer), and making use of the available evidence, it makes sense to define AS a rising school which is breaking through the contemporary international sociological field armed with the typical instruments of a (would-be) collective of thought, i.e. intellectual and organizational leaders, programmatic statements, research and training centres, reference- and textbooks, critical assessment and so on. Like Parsonsian functionalism,

symbolic interactionism, ethnomethodology, Marxism, etc., AS would be a school, or – to use a more elaborated sociological term – a theory-group [see tab. 1 for a stylized and not exhaustive description; on the concept of theory-group see Mullins 1973].

Like the Chicago School [e.g. Becker 1999; Abbott 1999], AS could indeed exist more in the minds of observers than of its alleged practitioners, more as a myth than as a real social entity, i.e. a shared collective identity with relatively well defined boundaries and interactivities. There are cues suggesting this could be the case, as I will say. However, there is also some evidence showing that AS exponents think and work as if they were encompassing a true school, a collective enterprise. “The whole is greater than the sum of the parts,” claims a recent review essay written in praise of AS [Manzo 2011]. Leaving for now aside the puzzle of the place of a *Kollektivebegriff* in a paradigm which argues only individuals are foundational in sociological analysis and is radically against holism in all its variants, I temporarily accept this claim as it is given and from this I move on in my discussion. I am aware that my table is contributing to give an appearance of substance or reality to something that may still exist more as a wishful thinking than in concrete practices and feelings, but I am ready to run the risk.

First, according to which criteria may AS be qualified as a theory-group? That is, how can we detect a clear-bounded network of scholars who recognize themselves as parts of a collective? Tab. 1 gives some cues, but they come from a selective and external reconstruction and modelling. An analysis of co-citation and communication networks would be the best strategy, of course, for assessing the degree of cohesiveness and boundedness of the movement, its working as a school [of thought as well as of activities: on this distinction see Gilmore 1988, Becker 1999]<sup>1</sup>. But this is something yet to be done. While we wait for this evidence, we can refer to more qualitative, impressive, but also living cues.

<sup>1</sup> The use of the distinction between « school of thought » and « school of activity » exemplifies well one of the mechanisms Merton identified in his sociological analysis of science, i.e. the « Matthew effect. » It happens in fact that even if originally proposed and elaborated by sociologist Samuel Gilmore, when he was a PhD student of H.S. Becker, people routinely quote the concept referring it to Becker who made some use of it in his work. Interestingly, Becker himself works hard in order to have the concept acknowledged as Gilmore's and not as his own, so contributing to the weakening of the effect. I underline this point, as it introduces an element of reflexivity in the working of a supposed mechanism – whose concrete functioning could be accounted for through a variety of cultural processes and factors, e.g. a certain style of master-student relations, a given conception of what academic work is, an historically grounded epistemic culture, and so on.

TAB. 1. *Social and intellectual properties of Analytical Sociology as a (potential) theory-group*

Intellectual leaders	P. Hedstrom R. Boudon J. Elster
Social-organizational leaders	P. Hedstrom P. Bearman
Research centres	Oxford University/Nuffield College (till 2012) GEMASS-CNRS (Paris) Columbia University
Training centres for students	Oxford University/Nuffield College Université de Paris-Sorbonne Universitat Autònoma de Barcelona University of Turin, etc.
Basic tenets	structural individualism + explanation through mechanisms + strong commitment to computational modelling
Success	T. Schelling, <i>Micromotives and Macrobehaviors</i> (1978) J. Elster, <i>Nuts and Bolts for the Social Sciences</i> (1989) P. Hedstrom and R. Swedberg (eds.) <i>Social Mechanisms</i> (1998)
Program statements	P. Hedstrom and R. Swedberg, “Social Mechanisms: An Introductory Essay” (1996-1998) P. Hedstrom, <i>Dissecting the Social</i> (2005) P. Hedstrom and P. Bearman, “What is Analytical Sociology all about?” (2009)
Exemples of secondary work	P. Hedstrom and P. Bearman, eds. <i>The Oxford Handbook of Analytical Sociology</i> (2009) P. Demeulenaere, ed. <i>Analytical Sociology and Social Mechanisms</i> (2011)
Exemples of critical work	from inside: G. Manzo, “Analytical Sociology and Its Critics” (2010); P. Hedstrom and P. Ylikoski, “Causal Mechanisms in the Social Sciences” (2010) from outside: Z. Norkus, “Mechanisms as Miracle Makers?” (2005); N. Gross, “A Pragmatist Theory of Social Mechanisms” (2009)
Textbooks	F. Barbera, <i>Meccanismi sociali</i> (2004); P. Hedstrom, <i>Dissecting the Social</i> (2005); M. Schmid, <i>Die Logik mechanistischer Erklärungen</i> (2006)

NB: this is a merely illustrative table, without any claim to exhaustiveness, and to be confirmed by further research. Things are also rapidly changing: i.e. Oxford will probably stop to be a relevant research centre, as the main scholars attached to AS (such as Hedstrom and Gambetta) have left or are leaving for other institutions.

The beginning of AS as a theory-group is well captured in the following reminiscence offered by one of its first and foremost proponents, the Italian sociologist Diego Gambetta, who apparently chose this term to give a name, and a potential distinctive identity, to the content of a new cycle of seminars he was organizing at the University of Oxford in 1995:

The idea to introduce a new disciplinary term, something one should never entertain lightly, came as a result of the utterly depressing state of sociology in Britain and elsewhere, and the wish to *mark semantically a distinction* with the work we were doing and were interested in. The choice of “analytical” was derived from “analytical Marxism,” and meant to signal similar properties as essential to the sociological practice. First of all, an understanding of the quintessential task of sociology as that of aiming to produce rigorous explanations of social phenomena. Next, a close attention to the micro foundations of social phenomena – that is the importance of identifying the set of desires, beliefs and opportunities which sustains agents’ behaviour, which jointly produce social phenomena. Third, the importance of using the micro foundations as the building block for constructing middle range theories capable of generating potentially testable hypothesis. Fourth, a close attention to the coherence of arguments and clarity of definitions and prose [quoted in Manzo 2010, 138; italics mine].

This is a highly informative and authoritative witness that deserves some comments. The proposed similarity between “analytical Marxism” (AM) and “analytical sociology” (AS) – the former being the inspirer and model for the latter – sheds light on some overtones which may remain hidden to the contemporary reader even if they keep working in the backstage. As it is well known, AM is a trans-disciplinary research program, started in the second half of the 1970s, which attempts to combine an interest in some of the central themes of the Marxist tradition with a “resolute use of analytical tools more commonly associated with ‘bourgeois’ social science and philosophy” [van Parijs 1994, 202]. Briefly, it is the attempt to rethink the traditional issues addressed in Marx’s work using not the originally Marxian logics (i.e. Hegelian dialectics) but “the most appropriate forms of standard analytical thought” as “conventional conceptual analysis, formal logic and mathematics, econometric methods and the other tools of statistical and historical research” [*ibidem*]. Incidentally, the merging of the Marxist tradition of social thought with the characteristic techniques of Anglo-American analytical philosophy which is at the core of AM may account for the strange alliance that has given form and substance also to AS after 1995: a mix of scholars firmly located in the left, social-democratic camp (e.g. Elster, Hedstrom, Gambetta) with others with typically liberal if not conservative orientations (e.g. Boudon). It also suggests one of the intellectual springs behind the rising of AS: the hope to revitalize and give new weapons to a brand of social research aimed at

informing and driving progressive social policies, if not even sustaining leftist politics [for a discussion of the political overtones and implications of J.A. Goldthorpe's vision of sociology see Savage 2000]. Even if this is a relevant point for a sociological account of AS as a political-intellectual movement, I will not elaborate on it in the following.

In this context, it would be more relevant to tell something about what appears to be the crucial, contingent condition for the rise of AS, that is, the perception of an "utterly depressing state of sociology in Britain and elsewhere," to be temporally located (roughly) in the early 1900s. As it is well known, the trope of a crisis of (Western) sociology dates back to the 1960s [Gouldner 1970 being its Ur-text]. In a certain sense, we could claim AS to be one of the many responses to that crisis, and especially to its perceived medium-term effects. Among such effects, three of them deserve special attention: the quest for a new theoretical platform after the demise of Parsonsian functionalism; the rise of qualitative, ethnographic-like social research; and the formation and success of Cultural Studies (henceforth CS). Many cues suggest that these two latter intellectual movements have strongly contributed to the production of a perception of a "depressing state" of sociology, especially in Britain where CS was born and started growing to a sort of hegemonic position inside the academy, especially in the Anglo-American world [on the early history of CS in its relations with Marxism see Dworkin 1997; on CS in its global dimensions see Miller 2001]. The two movements were closely related, indeed (think at Paul Willis as a case in point.) But it was the transformation of social theory into a post-structuralist and anti-modernist (or post-modern) program to generate the greatest troubles, especially to social-democratic, reformist social scientists. The 1970s and the 1980s have also been the years of the rise of such social scientists as Clifford Geertz, Anthony Giddens, and Pierre Bourdieu to the status of reference points for younger practitioners. Indeed, we can suppose these three scholars to be among the ones responsible, in the eyes of Gambetta and the likes, for this "depressing state" of sociology as a social science more in general. Bourdieu's social theory in particular is one of the more frequent target for many of the foremost representative of AS [e.g. Elster 1989; Gambetta 1989; van den Berg 1998; Hedstrom 2005; Goldthorpe 2007]. If the criticism against postmodernism has been a recurrent and distinctive feature of the movement, then interpretive social and cultural theory has been another common target. 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 are enough for many to disregard them. As a case in point, I would recall here the repeated insistence with which proponents of AS quote Bourdieu's concept of habitus as an epitome of the kind of confused and

not-analytical sociology they disapprove [see for all Hedstrom 2005, 4]. However, nowhere but in one chapter of the early manifesto [van den Berg 1998], we may find in the reference texts of AS some evidence of a direct and updated engagement with Bourdieu's sociological theory and the huge literature developed around and upon it – including the concept of habitus. Quoting a definition and simply claiming “this is obscure” does not seem very analytical, after all. What precisely looks unclear, obscure, puzzling in the concept – which indeed refers to a general social mechanism? The surprise increases when we consider that AS is partly a spin-off of analytical Marxism: what has been possible to do with Marx, it seems, should be possible and probably easier to do also with Bourdieu, whose mode of thought is after all relatively close to the Anglo-American standard, as it is shown – among other things – by the success encountered by his work among American sociologists in the last three decades [e.g. Zavisca and Sallaz 2008; Lamont 2012]. The suspicion to be dealing with a very common mechanism of intellectual distinction and academic competition [a double mechanism, by the way, studied by Bourdieu himself, e.g. Bourdieu 1984], with the accompanying devices of negation or censorship, is indeed strong.<sup>2</sup>

In fact, we could not understand nor explain the formation of AS as an academic/intellectual movement without locating it in the larger field of the social sciences it has been developing within between the 1980s and the 1990s. We should look at this larger field also to understand AS's research program, and the criticism to which it has been subjected since its inception – something that arguably occurred between 1995 (the year of the aforementioned Oxford cycle of seminars) and 1996 [the year of the Stockholm conference organized by Peter Hedstrom and Richard Swedberg, from which Hedstrom and Swedberg 1998 was drawn; for an early criticism see Abbott 2007, originally presented at the 1996 conference but not included in the proceedings]. This development is sociologically accountable.

As Tab. 2 summarizes, the stages through which AS has been evolving in the last fifteen to twenty years fit relatively well, at a first sight, with those identified by Mullins in his analysis of the dynamics of theory-groups' formation: a normal stage

<sup>2</sup> Instead of discarding the concept of habitus, AS practitioners would do better to analyze it and decompose it in its constituent parts in order to make it clearer – even if it looks already clear enough to the many scholars who make use of it in their work. In Google Scholar (consulted in May 2012), there are 68.900 texts quoting the words “habitus” and “Bourdieu;” of these 20.200 have been published since 2005. This is not the place where to develop the notion of habitus as a social mechanism. But I would like at least to quote a recent definition of mechanism: “A structure performing a function in virtue of its component parts, component operations and their organization. The orchestrated functioning is responsible for one or more phenomena” [Bechtel and Abrahamsen 2005]. The readers who are familiar with Bourdieu's classic definition of habitus will immediately notice a few enlightening similarities.



characterized by a low degree of organization and little coordination; a network stage during which people previously spread in various place start to meet and communicate in patterned ways; a cluster stage, when key figures and their students and colleagues find ways to institutionalize their meetings and reinforce one another's interests. It does not seem AS to have attained the specialty stage, when "the students become successful themselves, and both they and others are hired away from their original location" [Mullins 1973, 24]; in this stage, journals and positions specifically devoted to the new ideas are established: the theory has become a recognized specialty inside the relevant field.

TAB. 2. *The development of AS as a (potential) theory-group, since 1995*

Years	Developmental Stages	Events
Up to 1995	Normal Stage	Oxford seminars (1995)
1996-2002	Network Stage	Stockholm Conference (1996); publication of the proceedings of the conference (1998); foundation of the European Academy of Sociology (2000); Hedstrom goes to Oxford (2002)
2002-2012	Cluster Stage	Publications of first book-texts about AS (Barbera 2004; Hedstrom 2005; Schmidt 2006); First conference on AS and Social Mechanisms, Oxford 2007; Foundation at Oxford of ENAS (European Network of Analytical Sociologists); First Conference on AS and Institutional Design, Universitat Autònoma de Barcelona, March 2007; other conferences in Paris, Turin, Barcelona by ENAS; Foundation of INAS, International Network of Analytical Sociologists, 2011; First Meeting of INAS, New York 2012.
?	Specialty stage	--

Finally, in tab. 3 I have collected the names of the most quoted (or referred) authors in four reference texts of AS. The four lists show a good degree of consistence – a cue to the formation of something like a canon – but also some individual trajectories: some authors disappear, e.g. Bourdieu (the object of sustained criticism but also attention in the 1998 book), Peter Blau and Otis D. Duncan, while others make their appearance and rise in visibility, e.g. Peter Bearman, Thomas Fararo, Duncan Watts, and the late Roger Gould. The growing centrality of Hedstrom emerges clearly

from these data. Curiously enough for a school that took inspiration from analytic Marxism, Marx remains relatively at the margins.

TAB. 3. *Top Ten most quoted scholars in selected reference books, 1998-2011*

	Hedstrom and Swedberg 1998	Hedstrom 2005	Hedstrom and Bearman 2009	Demeleuneare 2011
1	Coleman	Coleman	Hedstrom	Hedstrom
2	Elster	Schelling Elster	Elster	Elster
3	Merton Boudon	Boudon	Merton	Boudon
4		Weber	Coleman Watts	Fararo
5	Bourdieu	Merton	Bearman	Coleman
6	Duncan, O.D.	Blau, P. Hedstrom	Gould, R.	Homans
7	Schelling Hedstrom Tversky, A. Blau, P.	Katz, E. Goldthorpe	Granovetter Fehr, E.	Bearman
8			Gigerenzer, G.	Parsons
9				Marx(ian)
10				Bunge Schelling

A few remarks are worthwhile, at this point, about AS as a would-be theory-group in the sociological field.

### How Much Sociological Is AS?

A first puzzling feature we could notice in AS is that among its foremost representatives we find scholars who are not institutionally and professionally sociologists. Two of the most quoted and also most active, Elster and Schelling, are respectively a philosopher-turned-political scientist and a (Nobel-prized) economist. Bunge is a highly respected philosopher of science. A couple of the top ten authors are psychologists (the late Tversky and Gigerenzer). Ernst Fehr is an experimental economist. Granovetter and Gould are well-known fruits of Harrison White's structural sociology (which Mullins, himself of the school, described in his 1973 book as "the structuralists"). I am not sure about their self-identification as analytical sociologists. Richard Swedberg, who contributed greatly to the launch of a mechanism-based sociology, does not identify himself in the world of AS (personal communication). The

same can maybe be said of an organizational leader as Peter Bearman, himself coming from White's structuralism/network theory (see his Comment, in this issue). Many other scholars can be added to the list, I suspect. This is not unknown to AS's most committed representatives. As a recent collective book admits, "[not] all the contributors consider themselves to be part of a single movement; nor that this movement is a perfectly unified school united by common and consistent beliefs" [Demeulenaere 2011, 3].

For an approach or a movement or a theory-group describing itself as "analytical sociology" (AS), it is a matter of identity to be analytical in its assessment of social things. A second, more serious weakness a critical observer may remark to AS is therefore that its analyticity seems to fail exactly in assessing what sociology is about and how it is composed. This brings us back to the four points Gambetta lists in his account of the rise of AS at Oxford – points that we could find in other programmatic statements and critical assessments as pillars or tenets of the "school" [e.g. Hedstrom 2005; Hedstrom and Bearman 2009; Manzo 2010].

Among them, the most important is apparently the fourth: clarity and rigor as essential ingredients of a good sociology. This is a common claim among scholars participating to the movement: as Elster puts it, the quest for clarity and explicitness is a sort of obsession. Indeed, obsession apart, this is something few sociologists would disagree with – at least in principle. Even sociologists attracted by postmodernism would dislike the charge of incoherence, and would admit a certain lack of clarity of definition and writing only when epistemologically justifiable, e.g. (they would argue) in case of concepts so deeply embedded in history that any clear-cut definition would be an unacceptable simplification and deformation. Incidentally, this is also the strategy Weber followed in his celebrated study on the spirit of capitalism: the definition of the object is the end, and not the start of the analysis. If this is the case, it is not at this programmatic level we have to look for any originality and specificity of AS as a theory-group, but need to focus on the concrete practices of research and writing. Given that all scholars like to be coherent, what makes them able to match this objective is the real point.

According to AS, the real point is the explanatory strategy, which has to be grounded on a double constraint: detailing the generative mechanism, and referring to individuals in its modelling. In fact, these are the most committing pillars of AS – more important, in fact, than the quest for clarity and rigor. Or better, they are the necessary conditions for doing a rigorous and clearly articulated sociology. They are also the less consensual, as they ask for the acceptance and adoption of a certain frame of mind (comprising a series of options) that is not so common among current sociologists. It is clearly upon these points that we have to focus our critical reading.

First, there is the statement about the task of sociology. This issue is not so pacific as it seems. Many AS practitioners notwithstanding, sociology has never been, is not, and presumably will never be uniquely an explanatory endeavour. Explanation is not *the* task of sociology but one of its various tasks, together with description – a necessary condition for elaborating sound explanations, but also an end in itself [see e.g. Rueschemeyer 2009] – and interpretative criticism. Sociology is not only a scientific, but also a humanistic discipline [e.g. Zetterberg 1965; on sociology as part of the humanities see Berger 1963 and Lepenies 1988]. This double identity is constitutive of the discipline, as a historically based human enterprise, since its inception.

I do not know exactly why scholars committed to AS believe that sociology has to be explanatory in order to be worthwhile, or to be sociology at all. What I know, is that rather than a fair analytical dissection and clarification of existing theoretical traditions and approaches, what we find in the texts, and also the subtexts, of AS's proponents is too often an embarrassingly biased reconstruction of sociology's various genres [Boudon 2002 is a well-known case in point], or a relatively generic assessment of what is wrong with the rest of sociology considered as a whole – to be contrasted with the only self-claimed scientific and promising way to do sociology (i.e. AS).

Indeed, the rest of sociology is a very large, and highly diversified field. It is large and diversified enough to make such claims symbolic tokens – beliefs useful for drawing and policing boundaries – more than an acceptable ground for comparatively evaluating the strengths and weaknesses of the various approaches. This will not turn out to be surprising to sociologists of science: AS suffers as a social thing (an intellectual movement, a theory-group) of some common human faults – viz. intellectual competition, invidious comparison, indifference to certain topics, idiosyncratic distaste for certain styles of thought. These are common passions, which seems however to strongly compromise AS – more strongly than in the case of other movements less committed to objectivity – in its constitutive claim to analytic rigor.

Second, there is the mechanistic option. This is, in my view, the most promising and satisfying pillar. We need to know what happens in the black box. A simple statistical association tells nothing about the working of the social world. A generative mechanism is what a sociologist looks for if he/she is engaged in an explanatory work. It is true – as Little himself recalls – that the concept of mechanism is far from being clear (indeed, a paradoxical situation for a school obsessed by the quest of clarity), and even the notion of middle-range theory is not very elaborated. But if there is a merit in AS, then that is the aspiration to provide an explanatory mechanism, a persuasive story, in order to account for any social fact sociologists are interested in. However, the concept of the mechanism is something AS shares with a lot of

other intellectual streams, not only in the social sciences: AS is far from having the monopoly of this conceptual device [see also Gross 2009]. I will come back later on this point.

What AS adds to the notion of mechanism is indeed a strong individualist commitment. Even the more open-minded statements available in the AS literature insist on this individualist option: “Analytical sociology explains by detailing the mechanisms through which social facts are brought about, and these mechanisms invariably refer to individuals’ actions and the relations that link actors to one another” [Hedstrom and Bearman 2009, 4]. True: instead of a purely methodological individualist stance, AS has a strong preference for what is now known as structural individualism [Udehn 2001], a stance which attributes substantial explanatory relevance also to the social structures in which individuals are embedded (much less to cultural structures, however, notwithstanding actors’ embeddedness in a cultural world and not only in a social structure) [Zukin and DiMaggio 1990]. But for AS, mechanisms work as explanatory devices only when they mobilize individuals’ properties and intentions. In his paper, Daniel Little argues this is not necessarily the case, and refers to a series of recent scholarly work to sustain his claim [including, on the epistemological level, Jepperson and Meyer 2011]. A different strategy would be to look at the history of sociology, and to search for alternative uses of the same mechanism concept.

### **Where Does AS (Really) Come From? Some Remarks On The Genealogy of “Analytical Sociology” and The Early Use of The Idea of “Social Mechanism”**

As shown also in table 3, Merton, Coleman, Homans and Parsons (the latter for his plea for an analytically framed sociology, but not for the kind of sociological work he practiced which was all but explanatory), are the recurrent names we find in the historical accounts of AS, more recently supplemented by the “discovery” of the French writer and historian Alexis de Tocqueville as a forerunner of analytical sociology [Cherkaoui 2005; Edling 2009; Elster 2009]. The liberal overtones of AS can only be strengthened by this recent addition, which, at the same time, contributes to increase its intellectual legitimacy. After all, Tocqueville is one of the most praised intellectuals of the Nineteenth century, and his name can only positively affect the reputation of AS. In an attempt to expand and deepen the genealogy of AS, also the names of less known, and less renowned, scholars as Lowell Julliard Carr and Gert H. Mueller have been recently added to the pantheon of potential precursors, while immediately taking distance from their legacies [Manzo 2010].

However, if we look at the past as historians of ideas and not as committed and interested genealogists, we can discover that AS has many more roots, precursors and potential sources than the current movement actually recognizes. The *explicit* aspiration to an analytical mode of practicing *sociology* dates back at least to Vilfredo Pareto, whose programmatic statement in his magnum opus *Trattato di Sociologia generale* (translated in English under the title *Mind and Society*) is worth quoting: “In these volumes I am reasoning objectively, analytically, according to the logico-experimental method. In no way am I called upon to make known such sentiments as I may happen to cherish, and the objective judgment I pass upon one aspect of a thing in no sense implies a similar judgment on the thing considered synthetically as a whole” [Pareto 1935, I, 39]. Indeed, if there is a founding father of sociology whom the label of “analytical” could be easily associated to, then this is the Italian economist turned sociologist Pareto. His influence on a whole array of scholars who are recognized by AS as its sources is also well documented, and well known to historians of sociology [e.g. Heyl 1968]. Suffice to say, Pareto was a central reference for American sociologists working or studying at Harvard in the 1930s – the same circle where both Parsons (who identified his own research program as “analytical sociology” since the 1930s) [see e.g. Burger 1977] and Merton were sowing the seeds of their brilliant careers. We may suppose that Parsons originally developed his analytical orientation partly from his closeness to the philosopher Alfred Whitehead on one side, and to the naturalist and epistemologist Lawrence Henderson – the real expert of Pareto at Harvard – on the other. Henderson was a chemical biologist keen on social theory, who taught for many years a seminar on Pareto and a course on sociology (paradoxically titled “concrete sociology,” indeed), attended by then-young scholars as George Homans, Talcott Parsons, Robert K. Merton, and Bernard Barber [see Henderson 1970]. Both Merton and Parsons were strongly affected by this experience. Homans himself started his academic career with a book on Pareto. As Merton is considered one of the founding fathers of analytical sociology qua “mechanismic sociology” (i.e. sociology as the quest for social mechanisms), and Parsons the first scholar to label his own research program as “analytical sociology,” it would be interesting to know what Paretian sociology may have contributed to the subsequent development of AS, and what has been lost in the meanwhile. Given the centrality of Parsons in the genealogy of an “analytical sociology,” it is not so strange that the label has been sometimes used to identify also an intellectual tradition derived from Parsons’ action theory, i.e. Garfinkelian ethnomethodology [McHugh 1968] – an intellectual tradition, however, which is really hard to be referred to by contemporary analytical sociologists, notwithstanding the fact that it has already generated what looks like the most immediate antecedent of a mechanismic micro-macro sociology

in the American context, i.e. Randall Collins' research program on interaction ritual chains [Collins 1981a; Collins 2004]. Sure, Collins is not kind towards rational action theory and exchange models, that have been at the core of AS since its inception. However, it is surprising how little this contribution to analytical sociology, grounded as it is on micro-translation and the search for mechanisms [interaction ritual chains are explicitly identified by Collins as a mechanism: see Collins 1981a, 985], has been noticed by current AS practitioners.

It would be interesting also to know more about other scholars who figure in the genealogy of AS – e.g. Lowell Juilliard Carr, the first to use the label AS as a book title, in 1955, and Gerd Mueller, a German sociologist who has worked for almost four decades on what himself called “analytical sociology” [see Manzo 2010]. Readers may be interested to know that Carr was a student of Charles Horton Cooley (with whom he co-authored a very fortunate textbook on sociology in 1933). The influence of this apprenticeship is still clear in the 1955 book, where there is no reference to Parsons or Merton, while Cooley is referred to one hundred times. Cooley, the theorist of the “looking-glass self” and one of the forerunners of symbolic interactionism, is not exactly the kind of author contemporary analytical sociologists would read, however [an author clearly more attuned to the pragmatist re-reading of the concept of mechanism recently proposed by Gross 2009].<sup>3</sup>

The Chicago tradition has much to do also with the other pillar of AS, that is the quest for social mechanisms. As it is well known also to AS practitioners and genealogists, the term “mechanism” has been indeed widely used in the sociological literature since its inception (it occurs also in Comte)<sup>4</sup>, even if in very generic ways and rarely conceived as an explanatory tool. However, it is possible to identify some crucial places where the notion looks more strategic than in others, and Chicago is among them. Already in 1905, for instance, Albion Small included the term “social mechanism” in a list of relevant sociological categories [Small 1905, 402]. The word, and notion, of mechanism, both in its behavioural and cognitive version, figures prominently also in the work of G.H. Mead – including the titles of a few papers of his [e.g. Mead 1912].<sup>5</sup> Not surprisingly, we may find a relatively consistent use of the concept

<sup>3</sup> Gert H. Mueller (1922-2011) is also a good case in point for enlightening the various sources an “analytical sociology” may be grounded upon: in his case, it is Husserlian phenomenology to provide strong foundations (a foundation which works for ethnomethodology too, of course). See Muller, n.d.

<sup>4</sup> Words and concepts are not the same, of course [a point vigorously made by the German school of *Begriffsgeschichte*, e.g. Kosellek 2004]. In the following I will focus mainly on the word « mechanism » as a more reliable and « objective » index than the concept of mechanism, notoriously polysemic [see e.g. Mahoney 2001].

<sup>5</sup> An early use of the notion of mechanism which anticipates the sense in which it is proposed inside AS is apparent in the work of the social psychologist Floyd H. Allport: see in particular his

(even if not necessarily the word) as an explanatory device also in Herbert Blumer and Everett C. Hughes, the two scholars who greatly contributed to the development of symbolic interactionism as a sociological paradigm [e.g. Hughes 1962, 6].

We could maybe locate here one of the roots of the use Howard S. Becker has been making of the word of mechanism, even “social mechanism,” since the 1950s [see an illuminating example in Becker 1960, which has been completely neglected by AS so far], as well as – I would underline this point – his familiarity with Florian Znaniecki’s and Alfred Lindsmith’s method of *analytic* induction, and with Everett Hughes’s plea for rigorous conceptual analysis [Becker 1998]. Incidentally, Richard Swedberg has been a student of Hughes as well [see Swedberg 2012 for an account of the current intellectual track of one of the original proponents of a mechanism-based sociology]. To be sure, there are good cues (including Howie Becker’s witness) indicating that Thomas Schelling’s work contributed to this early use of the concept in sociological analysis as a theoretical device and not simply as a descriptive category. This has to be read as a piece of evidence not only of a compatibility between game theory and symbolic interactionism the readers of Goffman are already well aware of [see Goffman 1961; Goffman 1969], but also of the fact that a mechanistic approach has a larger and less monolithic history *inside sociology* than contemporary analytical sociologists claim. The relationship between Schelling and Goffman was far from being unidirectional, grounded as it was on a dense exchange of insights and ideas (*pace* Boudon’s curious re-reading of Goffman’s work as an example of “expressive sociology,” as such, not scientific nor analytical)<sup>6</sup>. In sum, if Columbia has been pivotal in introducing the idea of “social mechanism” in the sociological toolbox *via* Merton and his middle-range theories, Chicago has also contributed to its early diffusion and implementation, moving from a different set of cognitive interests, presuppositions, and methodological motives.

While not Chicagoan in a strict sense, but strongly influenced by pragmatism as well as the European critical tradition of sociological analysis, even C. Wright Mills found the notion of “mechanism” appealing, and made strategic and consistent use of the word in his first influential articles in the sociology of knowledge, as the following excerpt clearly shows:

AJS’ article on the “group fallacy,” defined by the same author as “the error of substituting the group as a whole as a principle of explanation in place of the individuals in the group” [Allport 1924]. On this author, his individualistic experimental psychological early program, and his relations with the Chicagoan tradition for social psychology, see Greenwood 2004.

<sup>6</sup> In 1966, Goffman spent a sabbatical year at the Harvard Center for International Affairs, at the invitation of Thomas Schelling. For Schelling’s early reference to Goffman’s work see Schelling 1958. On the similarities if not convergences between Goffman’s and Schelling’s work on strategic interaction, see Collins 1981b; Mannini 1992; Burns 1992.



We cannot “functionalize” reflection in social terms by postulating a “collective subject”; nor can we avoid the fact that there is no “group mind” by conveniently using implicit conceptions of “collective subjects.” We can socially functionalize a given thinker’s production *only when we have made explicit, and systematically applied, a sound hypothesis of the specific sociopsychologic mechanisms* by which cultural determinants are operative. Without a thorough-going social theory of mind, there is real danger that research in the sociology of knowledge may become a set of mere historical enumerations and a calling of names. Only with such construction can we gain a clear and dynamic conception of the relations imputed between a thinker and his social context. Until we build a set of theoretically substantial hypotheses of socio-psychological nature, our research is likely to remain frustrated and our larger theoretical claims feeble [Mills 1939, 672; italics mine].

As a pragmatist who worked for a while at Columbia, at the Bureau for Applied Social Science run by Lazarsfeld and Merton, maintaining close yet difficult relationships with both [see Sterne 2005], Wright Mills is a good case in point to shed light on the complex intertwining of intellectual traditions which have been making American sociology. As the quoted excerpt shows, the quest for an explicit and systematic identification of explanatory mechanisms referred to individuals (at the sociopsychological level) was already among Mills’s priorities well before his arrival at Columbia in 1945.

Pareto, Cooley, Small, Garfinkel, Znaniecki, Mead, Becker, even Husserl: what this very brief excursion into the genealogy of AS may suggest is that there is more in the past that can be rescued or reworked than the current scholarship about AS seems to know or willing to accept. At least, there is some room for exploring other routes to an “analytical sociology” than the commonly accepted one (typically referring to Weber’s supposed methodological individualism, Merton’s plea for the search of causal mechanisms, Homans’ exchange theory, Coleman’s mathematical sociology, Anglo-American analytical philosophy, etc.). The misrecognition of the Chicago tradition in the official genealogy of AS is surprising also because if there is an historical alternative to the variable-based sociology AS has been criticizing since its inception, then this is exactly the research program which has its champions in G.H. Mead, R. Park, E.C. Hughes, F. Znaniecki, and of course Herbert Blumer [the author of a classical criticism to variable analysis: see Blumer 1956].

However, the most interesting discovery an historian of social thought could make while looking for the early use of both the concept and the word of mechanism is the following: for what I’ve been able to see, Robert K. Merton, who is considered by AS proponents as their foremost forerunner [e.g. Hedstrom and Udehn 2009; and recall also the reference to the “theories of the middle range” in Gambetta’s witness, a clear reference to the Columbia-based scholar], has used the term for the first time

while presenting Durkheim's work on the social division of labour to an American readership. In this case, the notion is fugitively quoted to refer to "recent field studies," e.g. Malinowski's ethnographical work on the Trobriand, which demonstrated the presence in "savage societies" of some corpus of restitutive law "kept in force by social mechanisms" [Merton 1934, 324]. Here the young Merton was directly following Malinowski, who explicitly used the term of social mechanism to make sense of the existence and working of economic and juridical obligations also in primitive societies [Malinowski 1926]. This makes anthropologist Malinowski a direct source, from a historical-intellectual point of view, of the mechanistic idea AS claims to have inherited on its turn from Merton (and the reader would recall, Malinowski had been a teacher of Parsons while the latter was studying in London in the early 1920s). Two years later, Merton uses again the notion of mechanism in his review (published on the recently founded *American Sociological Review*) of an important book by Italian statistician and sociologist Corrado Gini, with whom Merton was collaborating in 1936 as a teaching assistant at Harvard [see Merton 1936]. Also in this case, the concept is not Merton's but taken directly from the referred author. What is more interesting, the use Gini made of the word and concept of mechanism in his book *Prime linee di patologia economica* – an impressive work of economic sociology which is still waiting to be acknowledged in international sociology – was totally anti-individualistic. Indeed, in Gini's theoretical system the individual, micro level is almost invisible: mechanisms are integral parts, or components, of systems (conceived as biochemical organisms) whose existence in dynamic equilibrium is contingent upon the working of a series of forces and processes that were explicitly labelled *mechanisms* by Gini. Interestingly, Gini founded on biochemistry his own research program (which he called "neo-organicism"): this is the kind of theoretical background Merton had to cope with when he met Gini in 1935, working as his TA for a semester.<sup>7</sup> Of course, the concept of mechanism employed by Gini is *different* from the concept of mechanism we find in AS, but we can imagine that Gini's version somehow affected the way in which Merton conceived mechanisms, at least for chronological reasons. 1936 is the same year in which Merton wrote and published the first of his landmark articles on social action and social structure, i.e. *The Unanticipated Consequences of Purposive Social Action*. How much did Gini influence Merton on this issue? That is an issue whose discussion we have to defer to another occasion. But the point is that in the genealogy of the notion of mechanism which is at the centre of contemporary AS we have good reasons to hypothetically add such an apparently odd sociological

<sup>7</sup> On Gini and his relationship with Merton, also with respect to the concept of mechanism and biological theory, see Santoro forthcoming.

theory as Gini's neo-organicism. The idea that Gini could have been a source for the use of the concept of mechanism in sociological theory fits well also with the fact that Pareto has been a source for the quest for "analiticity" in sociology, as Gini built his social theory also on the shoulders of Pareto.

## Toolkits

Jon Elster has strongly contributed to the introduction of the idea of a toolkit or toolbox in the vocabulary of the philosophy of science. He has advocated a methodological pluralism which is certainly to be praised, and which the metaphor of a toolbox tries to convey. However, as Little himself has observed, in the end "Elster is guilty of the sort of over-generalisation about social science method that his tool-box metaphor would reject" [Little 1992]. Instead of a well-assorted box, we find in it only a few selected tools, which are asked to do a very heavy work. Also the conceptual toolkit of AS is surprisingly relatively ungarnished, and fails to include many "conceptual devices" other sociological approaches have (analytically) elaborated in order to grasp social reality. Consider the concept of "culture," one of the first categories proposed by social scientists. Sociologists and anthropologists have worked hard in the last three decades to distinguish – inside this very general and admittedly vague notion – a series of analytic components, such as norms, beliefs, values, expressive symbols, meanings, frames, skills, rituals, worldviews, codes, scripts, logics, signs, stories, boundaries, etc. [see DiMaggio 1990; DiMaggio 1997; Zerubavel 1997; Sewell 2005; Alexander 2003]. A cultural sociology has developed, which comprises a larger and larger part of sociologists in the US, the UK, France, and the rest of the world. A cultural history is born from the ashes of the old social history. Cultural anthropology has renewed itself through Geertzian and post-Geertzian insights. However, and for reasons that are not immediately clear, AS seems to ignore or at least to neglect all of this work and reduces the cultural stuff – or better, the culture-as-toolkit, as Swidler [1986] has famously named it – to conceptually old categories as norms and beliefs, further classified in various subtypes but highly simplified in their internal semiotic structure [e.g. Hedstrom and Bearman 2009]. In the meanwhile, it loses sight of the more sophisticated and varied elements which are – according to current sociologists of culture and cultural anthropologists and historians – worth of analysis and consideration in our grasp of social life in its constitutive diversity, e.g. the aforementioned frames, codes, stories, scripts, logics, symbols etc. (of which there is no trace in the already cited *Oxford Handbook of Analytical Sociology*, at least judging from the detailed subject index). These ele-

ments are not only diversified, but also organized in a sort of architectonics with different degree of deepness in the texture of social life (see table 4). Norms, beliefs and preferences – the elements AS focuses upon – are located at the more superficial level. Sure, among the “cogs and wheels” we find heuristics and emotions alongside norms and beliefs. Whereas a cultural history and cultural sociology of emotions have developed in the last twenty years, enlarging our knowledge of the variety of feelings and emotional expressions humans are able to produce, within AS emotions are still surprisingly considered as anthropological universals (to be explored with the aid of philosophy and cognitive psychology) that variously interfere with rationality.

TAB. 4. *The architectonics of culture . Adapted and updated from DiMaggio (1990)*

Level	Types of cultural elements	Representative authors
I	norms, beliefs, attitudes (including preferences), values	Parsons, Weber
II	strategies, <i>habitus</i> , logics, <i>scripts</i> , rituals, codes, narratives, performances	Bourdieu, Garfinkel, Swidler, Friedland and Alford, Alexander
III	rules of relevance governing the (performative/strategic/ecc.) social uses of cultural <i>scripts</i> , institutional logics	Garfinkel, Friedland and Alford
IV	systems of classifications, boundaries	Schwartz, Zerubavel

Still, there is a whole *theory of action as practice* which has been developed at the intersection of sociology, philosophy and anthropology, to account for deeply rooted and usually highly codified way of doing things [e.g. Schatzky *et al.* 2001; Ortner 2005]. But this theory and the pivotal concept of practice have no room in the AS repertoire. Why does this happen, exactly? Probably for a supposedly obscure nature of practice – as evinced in the typical disdain for one of the concept through which practice has been managed, i.e. *habitus*. I have already expressed some serious doubts about the analytical soundness of such a disdain. I would add that practice theory has gained a status, and an analytical development, and neglecting it could only further contribute to the unproductive seclusion of camps in the sociological field. The conditions for avoiding such a scenario actually exist: “Although some analytical sociologists are rational-choice theorists, most are not,” claim Hedstrom and Bearman [2009, 22]. However, there can be no doubt whatsoever that RAT has

been and still is the favourite theory of action within AS: therefore, AS practitioners most likely assess any alternative concept or theory through such a mindset. Is social action always and necessarily the consequence of decision or choice made at the individual level according to a specific calculus? No, of course, and the more honest AS practitioners now admit it and look for ways to analytically differentiate among diverse kinds of actions and their environments – something that cognitive and cultural sociologists have been already doing [DiMaggio 2002]. True, habit and creativity may find some place in AS (as suggested by Manzo [2010] in his reply to Gross [2009]), but we can doubt this place to be the most fertile and profitable where to work on these crucial and consequential aspects of social action – especially considering that there is a huge and highly influential work already done elsewhere on both habit and creativity. Rationally speaking, it seems wise to benefit from this work and to cumulate knowledge, instead of starting again or searching tricks to restore what was originally excluded from the model.

We could see in this relatively weak understanding of culture an aspect of the Mertonian legacy AS is so proud of. Indeed, on this point AS scores even worse than Merton, as the diagram in figure 1 shows: and the late Merton has obviously had less time to realize the wider consequences of the cultural turn in the social sciences, especially in terms of conceptual development and clarification. Albeit Merton's model of culture is not really sophisticated, and less focused on the *internal* structure of culture than on its social consequences, in the general analytical apparatus elaborated by Merton in his many works there is room for *cultural structures* along with social structures as conditioning or better shaping factors of individual choices [see Crothers 1987]. Those choices are not considered as isolated acts but as “embedded within broader strategies,” i.e. social practices, which can even be conceived – when highly patterned and recurrent – as ways of life, or lifestyles [Crothers 2004, 29; for a recent, wide assessment of Mertonian sociology and its legacy see Calhoun 2010, especially the chapters by Fuchs Epstein and Zelizer on Merton as a cultural theorist]. As figure 1 shows, the Mertonian theoretical model is much more complex than the now familiar Boudon-Coleman diagram, also known as the Coleman boat (notwithstanding Coleman, on his turn, drew it from psychologist David McClelland).

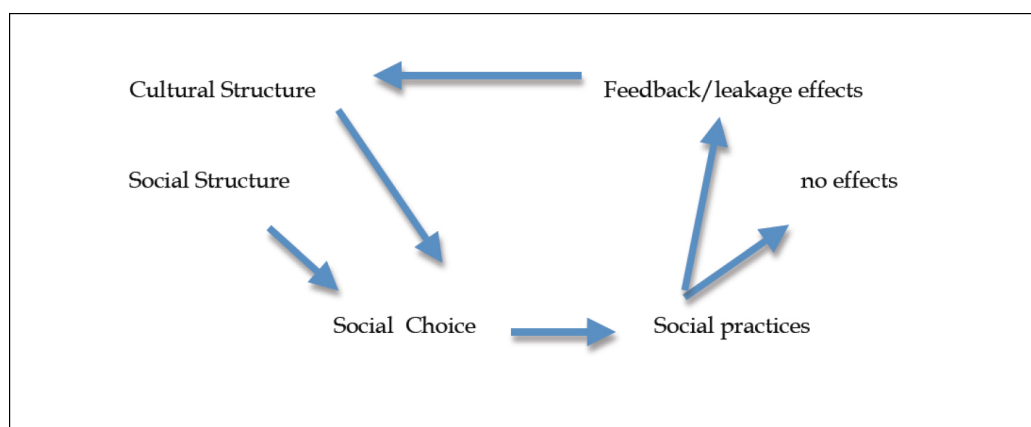


FIG. 1. Merton's general analytical model.

Source: Crothers 1987.

In order to make my point less abstract, let me consider one of the most praised work among AS practitioners, Peter Bearman *et al.*'s analysis of the structure of romantic love among adolescents [Bearman, Moody, and Stovell 2004]. This is indeed a very sophisticated social network analysis, an elegant exemplar of structural sociology that integrates network theory with both individual reductionism and simulation analysis. What the authors discovered, after observation and collection of relational data among the students of an American high-school, is a configuration of romantic and sexual relationships in form of a spanning tree. This is a puzzling social fact asking for an explanation. The authors find the latter in the hypothesis, empirically tested through a series of sophisticated simulations and comparisons with the actual data, of a simple norm governing the dating world: avoid four-cycle relationship partners, i.e., from the point of view of a boy, to have a partnership with his prior girlfriend's current boyfriend's prior girlfriend. But what exactly is the statute of this norm? We are said it is not in the consciousness of actors, who choose partners according to a series of vague and not really consistent motivations, knowing however one thing, it seems: that some partners produce loss of status. It is not necessary to be sociologists to know it. However, sociologists may find legitimately interesting to explain not just why, when aggregated, the individual choices generate a spanning-tree structure (the macro social fact to be explained through a mechanism referred to individuals with their actions and relations), but why some kinds of partners – and not other – produce loss of status, which scripts govern the adolescents' search for a partner, how and when and to whom they account for their choices, which meanings i.e. which symbolic repertoires they use or negotiate or even create to make sense of this complex set of choices [on adolescent culture see e.g. Fine 2001]. The norm itself looks very similar to the constitutive rules of social order ethnomethodologists are profession-

ally looking for, trying to make sense phenomenologically of their existence and their working. I am not sure all this can be done following the principles and making use of the tools offered by AS – especially the simulation and computational ones. If it was possible, it would mean that the identified mechanism asks for a further explanation (presumably starting that *regressio ad infinitum* that some critics, discussing the idea of social mechanism, have already pointed to); if it was not possible, it would mean that AS needs to be at least complemented with other intellectual strategies in order to account for its same research objects.

Of course, there are some reasons for this simplification: only when we have agreed upon a relatively small but consistent set of well defined concepts can we analytically work with them and through them for building models, doing simulations, etc. which are the prerequisite for that rigor and clearness AS asks for. Concepts are available to this “purification” in different degrees. The concept of meaning (and meaning system) is far more difficult to model than that of belief, especially if the modelling we are interested in is agent-based and grounded on an implicit cleavage between the subject and the world [Wuthnow 1981]. The price of AS’s merits can indeed be high when, for gaining rigor, we drastically reduce – and without the possibility to check exactly what we are losing – the symbolic texture of social life to a set of manageable elements which more than others are susceptible to modelling. With the further risk to mistake – as Bourdieu would say – the model of reality with the reality of the model, a risk AS practitioners may run (e.g. Gambetta’s renowned analysis of the mafia). A certain amount of reification is indeed at the core of AS in as much as it is grounded on the firm assumption that individuals are real things (realer than other social things, e.g. organizations or social groups), and we have always to move from them and their properties to make sense of social life. As Georg Simmel observed at the beginning of the Twentieth century:

[I]f we examine “individuals” more closely, we realize that they are by no means such ultimate elements or “atoms” of the human world. For the unit denoted by the concept “individual” [...] is not an object of cognition at all, but only of experience [...] What we know about man *scientifically* is only single characteristics [...] Only by isolating and grasping them and by reducing them to increasingly simple, covert and remote elements do we approach what is really “ultimate,” that is, what is real in the rigorous sense of the world. This “real” alone must form the basis for any higher intellectual synthesis [...] But what is more, even *these* so-called elements are highly synthetic phenomena [...] Thus, a conception that considers only “individuals” as “real” lets what *should* be considered real get out of hand. It is perfectly arbitrary to stop the reduction, which leads to ultimately real elements, at the individual. For this reduction is interminable. In it, the individual appears as a composite of

single qualities, and destinies, forces and historical derivations, which in comparison to the individual himself have the same character of elementary realities as do the individuals in comparison to society (Simmel 1950, 6-7).

Also “individuals” then are indeed synthetic models, abstractions of abstractions whose unity is grounded on experience and not cognition, models which are more or less visible according to the degree of focalization on social life we are looking for; we can circumscribe agency to them only on philosophical tenets that cannot look but naïve in the Twenty-first century [see Fuchs 2001].<sup>8</sup> At minimum, AS has to take explicit and sustained position with respect to such currently available and diffused intellectual stances as phenomenology, structuralism, post-structuralism, system theory, institutionalism, actor-network-theory, and other kinds of (so called) holistic theory, not for simply dismissing them but for analytically showing when and why these stances do not work as platforms for generating valuable explanations of social facts. Maybe, also for assessing and deciding when and under which conditions their insights could be included in the AS’s framework.

### **The Rest, the West, and the Best (of the World)**

We are approaching the final point I would like to rise in this paper. Let me express it in a very drastic and extreme form: AS is one of the most Eurocentric program in contemporary sociology. Its main proponents come from North Europe (Scandinavia), its main followers are French, Italian (on their turn mainly located in France, the UK and the US), German, Catalan, and North-American (typically located in very prestigious universities). All the authors quoted in its reference books are Western, and of an intellectual brand that a political philosopher would immediately label “liberal.” The fate of Marx is enlightening: for a sociological program which took inspiration from analytic Marxism, it looks revealing to notice how little Marx is referred to in AS. But this is clearly a minor issue. More seriously, it is necessary to note that in a period in which sociology is becoming a really global enterprise, and alternative discourses and even canons are emerging in Asia as well as in Africa, in Latin America, and in Australia [e.g. Alatas 2006; Connell 2007; Patel 2010], here we have a collective made of all white scholars, coming from and located in metropolitan

<sup>8</sup> Still, individuals are embodied entities, indeed – and as bodies and not only minds they socially exist, as many sociologists, anthropologists, feminist theorists, philosophers have been telling us for many years. Embodiment is not a particularly valued property in AS – notwithstanding the discovery of the body in recent social theory, also with reference to the theory of practice [Turner 1984]. Individuals act as embodied agents – and the body is a crucial medium and a (constraining) resource of their agency.



areas<sup>9</sup>, almost all male (only very recently women have entered the field), and – what is more important – deeply committed to typical Western values and/or beliefs as individualism and rationality. As Meyer and Jepperson have argued [Meyer and Jepperson 2000; see also Jepperson and Meyer 2011], individual agents are cultural construction of the modern Western world. Individuals are not considered everywhere as those unitary and stable essences endowed with (rational) agency that AS practitioners take for granted. Cultural psychological research teaches us that the degree of agency and “entitativity” [Campbell 1958] granted to individuals change in time and space, i.e. culturally, and that agency in some cultures may be equally conferred to both individuals and social groups [Kashima *et al.* 2005].

This has also methodological implications. As Susan H. Rudolph discovered a few years ago, for example, doing survey research in an Indian village does not necessarily make sense, as individuals, before answering to questionnaires, want to have a meeting with the whole the community, in order to know what to reply. In her own words: “Responding to a survey question became a matter of collective deliberation, a veritable seminar. The experience instructed us that in village India the individual was not the unit of opinion. Indeed, the singular, private, and personal were alien to the life worlds of Indian towns and villages” [Rudolph 2005, 5]. Paradoxically enough for a movement that does not recognize a great role to cultural elements, one of the strongest ontological foundations of the intellectual strategy it recommend could ask to be acknowledged as culturally grounded, and variable. This is a point that the expansion of sociological creativity beyond the boundaries of the western world – not as a tool in the hand of a colonizing army, but as a tool for self-knowledge and self-government directly commanded by local scholars – asks to urgently address. This is the challenge that such a privileged intellectual movement as AS could not afford to neglect, even if only to help “mistaken” or “disoriented” colleagues to find their (right) way.

Waiting for this, we may guess that AS – still intended as a rigorous and coherent disciplinary endeavour, capable of constructing clearly defined conceptual systems and through these building (middle range) theories, or models, which can generate potentially testable hypothesis about the working of the social world, wherever and whenever – will develop in the future into a wider, more variegated field than what the current AS looks like, with lesser constraints at the level of ontological presuppositions and epistemological commitments but with the same aspiration to rigor and

<sup>9</sup> I know only one exception to this rule, the French (but North-African) sociologist Mohamed Charkaoui, author of many studies in the tradition of AS (strongly influenced by Boudon), who studied in Paris and there made his career. For a collection of his shortest works see Charkaoui 2005.

clarity. Something like this is already occurring – it seems – through the diffusion of AS beyond the narrow European borders and into the larger and less polarized field of US sociology, where sociologists attracted by the quest for a more rigorous and formalized (i.e. model-generative) kind of sociology are often critics of RAT in any of its variants (strong, weak, middle, and so on) and therefore eager to build their explanatory models on other theories of action and other conceptions of the actor. (The theory of the habitus is a precious even if perfectible step in this direction. But it is obviously not alone.) We may guess this expansion is a condition for the movement to succeed as a truly international if not global theory-group.

A genuine “explanatory pluralism” is what sociologists may really need in the present conditions of the discipline, also in light of current developments in the philosophy of science [see e.g. Marchionni 2008]. If we acknowledge that methods always presuppose and enact ontological and epistemological options, then we can imagine this is after all what Bearman and Hedstrom were foretelling in concluding their introduction to the *Handbook*: “That at present most analytical sociologists arise from the social-network and/or mathematical- and computational-modeling traditions, while not accidental, does not mean that future work will be tied as tightly to these methods” [Hedstrom and Bearman 2009, 21; for a different guess, see Manzo 2010; Manzo 2011]. It is our common task, and interest, to work for this widening of scope and pluralisation of strategies, and make it happen.

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## The Whole and the Parts

### Or: Is Analytical Sociology Analytical Enough about Sociology, and Itself?

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**Abstract:** The article offers a critical description of “analytical sociology” as a would-be theory-group, showing the kind of institutional and rhetorical devices scholars have been using for imposing a new brand of sociological theory in the field of sociology. The main argument is that AS looks paradoxically poor in analytical terms when confronted with what should be the first test of any sociological theory, i.e. itself, as a social and cultural object. Three main points are highlighted: 1) the biased and highly selective reconstruction of the standard genealogy of AS, which neglects whole paradigms of social research equally committed to analysis and the search for mechanisms; 2) the use of the notion of toolkit as a metaphor for both a working research approach and an extremely limited set of tools (with the paradoxical neglect of the toolkit of culture, and the series of analytical distinctions social and cultural theorists have developed in the last decades for making valuable and usable the concept of culture); 3) the apparent eurocentrism of AS, and its problematic relation with both an increasingly globalized sociological discipline and the current search for alternative (to Western) discourses in the social sciences.

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*Keywords:* Mechanism, theory-group, toolkit, eurocentrism, analytical sociology.

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