GIANLUCA MANZO

*Analytical Sociology and Its Critics**

Abstract

In reaction to the criticisms to which analytical sociology has been subject with increasing frequency, the article attempts an overall assessment of this research program by addressing the following questions: where does contemporary analytical sociology come from? what are the differences between the ''old'' and ''new'' analytical sociology? what does analytical sociology really consist of? do the critics of analytical sociology have good reasons to be critical? Gross's 2009 ASR article is deeply discussed in order to answer the last question.

Keywords: Analytical sociology; Mechanisms; DBO theory; Agent-based models.

IT SEEMS AS if the proceedings of the Stockholm Conference (1996) edited by Hedström and Swedberg (1998a) have changed the position occupied by the concept of mechanism in contemporary sociology. Its spontaneous use, as recurrent in the history of the social sciences (Bunge 2004, p. 207), has given way to explicit, intentional and programmatic thinking about social mechanisms. Following Hedström (2005) and, more recently, Hedström and Bearman (2009a), many now agree that the research program being built on that concept can be called ''analytical sociology'' (AS, hereafter).

This research program seems to be spreading although to different extents across countries. Books, articles and symposia on AS can, for example, be found in Norway (Birkelund 2009; Osterberg 2009; Tufte 2009a, b), in Germany (Malsch 2006, Schmid 2006,

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Gianluca MANZO, GEMASS-CNRS, Université de Paris IV - Sorbonne [glmanzo@yahoo.fr].
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Soziologische Revue, 2009, 32, 4), in France (Cherkaoui 2005; Manzo 2007a, b, 2009a), in Spain (Aguiar, de Francisco and Noguera 2009), and in Italy (Barbera 2004; on this book, see Ballarino 2005, Lucchini 2007; see also *Sociologica* 2007, 2).

On institutional grounds, there are many elements to suggest that AS should be understood as a complex intellectual movement in the process of being ''crystallized'' (Barbera 2006)[1]. However, the heterogeneity and the dynamics of a movement of this kind make it difficult to evaluate.

The approach that I adopt here in attempting a first assessment of AS is to examine the criticisms to which it has been subject with increasing frequency. With the exception of Tufte's (2009b) working paper (which, however, only considers the Norwegian community), systematic analysis of these criticisms is still lacking.

At least seven objections against AS can be identified in the literature. According to the misgivings expressed: 1) AS rests on a concept, that of mechanism, which is poorly defined (Gross 2009; Bunge 2007, p. 259); 2) this concept has an ambiguous epistemological status within AS (Brante 2008, p. 276; Reiss 2007, p. 166); 3) AS action theory is simplistic (Abbott 2007a, Gross 2009, Sawyer 2007, p. 257; Pisati 2008; Lucchini 2008); 4) AS underestimates the importance of descriptive tasks (Reiss 2007, p. 164; Opp 2005, 2007; Bernardi 2007; Pisati 2007); 5) AS erroneously rejects the nomological approach to explanation (Opp 2005, pp. 174-177, 2007, pp. 117 -118; Norkus 2005, pp. 352-355; Sawyer 2007, p. 259); 6) because each mechanism entails another mechanism, AS (mechanismic) explanations are constantly incomplete (Opp 2005, p. 169; Pisati 2007, p. 7;

[1] At the international level, AS has provided itself with a network – *European Network of Analytical Sociologists* (http://nuff-caldey.nuff.ox.ac.uk/) – whose members meet once a year. The *International Institute of Sociology* (ISA) scheduled sessions on AS in 2005 and 2008. In October 2008 a follow-up to the Stockholm Conference was held in Paris. At national level, activities flourish in various countries. In Sweden, the annual conference of the *Swedish Sociological Association* organized a session on AS in 1999, 2000, 2001 and 2003. Between 1998 and 2004, P. Hedström, R. Swedberg and C. Edling coordinated several research projects at the Department of Sociology of the University of Stockholm (all of which were funded by the *Swedish Foundation for Internationalization in Higher Education*): a) an undergraduate course on AS in 1998, 1999, 2000; b) several graduate courses; c) a series of seminars entitled *Seminar on Social Mechanisms* (1999-2000); d) a working papers series on social mechanisms (1999-2003); e) an editorial series entitled *Stockholm Studies in Social Mechanisms* publishing doctoral dissertations. In Britain, D. Gambetta and P. Hedström have regularly organized at the University of Oxford a seminar entitled ''analytical sociology'' (respectively, in 1995, 2005 and 2007, and in 2004). Several research groups on AS are currently active in Norway and in Spain (see, respectively, Tufte 2009b, and Aguiar, de Francisco and Noguera 2009b).

see also Steel, 2004, pp. 61-64); 7) AS excessively emphasises agent-based models (Abbott 2007b; Sawyer 2007, p. 260; Lucchini 2007, pp. 236-240, 2008, pp. 9-12).

These criticisms provide an opportunity to examine the content and current scope of AS. Aside from the variety of points that they address, these criticisms share an implicit argument. They tend to isolate some aspects of AS and seek to demonstrate that one of two options is true: they maintain either that, on the points selected, similar proposals already exist elsewhere or that alternative and more refined options should be considered. In both cases, the approach's consistency and specificity are questioned.

The aim of this article is to assess to what extent these objections are well-grounded. To this end, it seems appropriate to adopt a perspective that is both historical and ''paradigmatic''. The problem of the specificity of AS (like any other approach) raises two separate issues. On the one hand, one should establish when some of the ideas characterizing AS have emerged. On the other hand, one should determine to what extent and in what sense AS today contains elements which distinguish it from other approaches, as well as from other traditions dealing with the concept of mechanism (Hedström and Ylikoski 2010).[2]

My main argument will be that careful analysis of the literature shows that certain pivotal ideas in contemporary AS were born at a specific point in time and were subsequently integrated together and developed in a distinct direction. According to this hypothesis, the originality of AS today consists in a combination of conceptual, epistemological, ontological and methodological options which, though not new when considered separately, define a coherent research agenda when they are studied as a whole. The distinguishing feature of AS, I shall argue, is that it comprises a syntax of explanation; that is to say, a set of constraints on how an explanation should be constructed and empirically tested.[3]

[2] At least since the early 1990s, this concept has indeed been at the centre of diverse discussions in philosophy of science (Glennan 2002; Woodward 2000), philosophy of social science (Bunge 1997, 2004; Little 2001; Manicas 2006), political science (Gerring 2008; McAdam, Tarrow and Tilly 2008; Tilly 2001; Cederman 2005), comparative historical sociology (Kiser and Hechter 1991, 1998; Mahoney 2004; Sica 2004), and economics (Epstein 2006).

[3] Demonstrating this claim requires dealing with the problem of the boundaries of AS. Here I adopt the following categories (somewhat questionable, I admit) to determine whether or not a scholar is relevant to AS: 1) authors who have specifically developed the concept of mechanism and/or intentionally expressed programmatic views about AS; 2) authors who have contributed to edited books on the concept of mechanism and/or AS; 3) authors for whom it is possible to establish institutional/intellectual links (students of, scientific collaboration with, etc.) with authors who belong to categories 1 and 2; 4) authors who are members of committees, networks and research groups on mechanisms and analytical sociology.

The article will be organized as follows. The first section addresses the question of the historical origins of contemporary AS. I shall seek to show that, although both a ''mechanismic sociology'' without an AS and an AS without a ''mechanismic sociology'' have existed for several decades, the integration of the ideas contained in these two older strands of literature has given rise to an original research program, namely contemporary AS.

The second section is devoted to the content of this research program. I shall try to identify the set of basic ''core'' principles which seem to stand at the intersection of the various contributions making up contemporary AS (which means that authors within the approach may not recognize themselves in some or other specific aspect but would not reject the whole). In this sense, aware of the ambiguity that this choice may cause, throughout this article I shall adopt the Kollektivbegriff ''analytical sociology'' (AS).

The aim of the last section is to respond to criticisms against AS. In this regard, I adopt a particular strategy. Instead of reviewing each criticism – thereby running the risk of not responding to any of them – I shall focus on Gross's (2009) article, which is a serious and constructive criticism of two pivotal components of AS: namely the definition of the concept of mechanism, and AS's microfoundations. However, to avoid giving the impression that I want to evade the other criticisms mentioned at the beginning of this introduction, I shall briefly answer them (in footnotes) at the same time as I set out the ''basic core'' of AS.[4]

1. Where does contemporary Analytical Sociology come from?

This section seeks to show that contemporary AS was born at the intersection of two sets of older ideas: the first concerning the concept of mechanism, the second linked to the concept of ''analyticity''. The junction of these two ideas gave rise to an original intellectual project which has conceived sociological analysis as the patient decomposition of the patterns of social life into the building blocks responsible for their emergence.

[4] Another profound critical analysis which would warrant especial attention is that by Abbott (2007a). Elsewhere (Manzo, 2007b), I have commented in detail on Abbott's criticism – which shares a similar pragmatist inspiration with Gross's. However, I shall later briefly discuss two arguments in Abbott's (2007b) reply to my comment.

1. A mechanismic sociology without AS

The period between the 1950s and 1970s constitutes the turning point for those seeking the origins of a conscious, intentional and programmatic mechanism-based movement in sociology. At that time, a first definition of ''mechanism'' was adumbrated, diverse methodological applications appeared, and explicit epistemological reflection began.[5]

As regards the definition, Merton (1957², p. 351; 1967, p. 43) wrote: ''mechanisms refer to structures and processes considered in terms of their functional significance for designated requirements of the social organization''; and farther ''social processes having designated consequences for designated parts of the social structure''. Although a functionalist connotation persists in these definitions, Merton used them in practice to build middle-range theories that verbally described cumulative processes of various kinds, such as the self-fulfilling prophecy or the Matthew effect (Merton 1948, 1968).

On a methodological level, however, the link between the concept of mechanism and that of model took shape within nascent mathematical sociology (Coleman 1964), where there arose the idea of studying models of social mechanisms with mathematics or numerical simulations.[6]

The first book with the expression ''social mechanisms'' in its title was a handbook on mathematical sociology (Karlsson 1958), whose author connected this concept with those of model, middle-range theory, and interdependent system.[7]

[5] My decision to identify only explicit elements does not amount to denying that the idea of mechanism is much older. A first step towards the identification of social mechanisms can already be found in Tocqueville (Cherkaoui 2005, ch. 1; Elster 2009; Edling and Hedström 2009), in Marx (Elster 1985), in Weber (Cherkaoui 2005, ch. 2) or in Durkheim (Cherkaoui 2005, ch. 3; Fararo 1989, pp. 134-137). However, such historical interpretations pose the difficult question of evaluating the extent to which one is merely re-reading past contributions through contemporary theoretical lenses. Note also that it is not easy to determine the influences exerted by other social sciences, as well as by physics and biology, on sociology in its gestation of the concept of mechanism. Linguistics may also have played an important role in the process (Cherkaoui 2005, ch. 4); yet an explicit reference to Chomsky's concept of generative grammar has been made only recently, and in economics (Epstein 2006, p. 12, 55).

[6] At that time, a confrontation between these two types of formalisms was in fact emerging more generally in the social sciences (Arrow 1951 and Simon and Newell 1956).

[7] Karlsson's (*ibid.*, p. 16) definition is as follows: ''The term, social mechanisms, is used here to refer to a model that covers only a part of a total social process, [...] It is closely related to the 'theory of the middle range' which also specializes in only certain aspects of the functioning of the group. The expression 'mechanism' is used because we want to indicate by our choice of words that we intend to study groups as interdependent system of motivations ad acts. It does not imply, of course, that this interdependence would be mechanistic in any philosophical sense.''

Fararo (1969a, p. 225), one of the most active scholars in this new field, introduced the concept of mechanism in the context of analysis of observed non-Markovian processes:

> investigators often construct a totally new process {Gt} with different states (or states interpretations) such that {Gt} is simple to work with mathematically and permits derivation of the properties of the observable process {Xt}. Such a process is here termed a generating process, since in the simplest case one has a mechanism that provides a functional transform that sends the {Gt} into {Xt} process: {Xt} = F{Gt}.

According to Fararo (1969b, p. 81, pp. 84-85), the goal of mathematical sociology is precisely ''to formulate and test specific mechanisms postulated which describe how the phenomenon occurs''. Instead of restricting ourselves to the construction of ''models of the data'', claimed Fararo, we should systematically build ''model(s) of the process generating the data''.

Sorensen (1976, pp. 71-72, 1977, pp. 965-966) made a similar point in the context of criticism against the status-attainment paradigm: it is necessary to mathematically formalize hypotheses about the mechanisms and empirically estimate parameters which reflect them (Sorensen 1979, pp. 377-383)[8].

Boudon (1979, p. 52) also accepted the concept of a ''generating theory or model'' and claimed that ''a major task for the future will be to learn how to build generating theories to explain statistical structures'' (Boudon 1976, p. 1187). According to Boudon, the axioms of such models must contain at least: ''a description of the logic to regulate the actions of the individuals'' and ''a description of the social constraints within which the logic of individual action develops'' (*ibid*., p. 60). However, unlike Fararo and Sorensen, who focused on mathematical models, Boudon studied his generating models of judicial choice, social mobility and relative deprivation by means of numerical simulations (see respectively, Davidovich and Boudon 1964; Boudon 1974; Boudon 1982, ch. 5).

This opposition between two ways of implementing a generating model also appeared in Schelling (1971) and Granovetter (1978). Apart from the differences between their models, both authors were interested in mechanisms of aggregation of interdependent choices. But while Schelling created this dynamic by iterating local rules of action and interaction (thus anticipating the modern agent-based

[8] Sorensen's (1976, pp. 71-72) criticism was that ''there is a strong emphasis on estimating the magnitude of causal effects, and little emphasis on specifying the mechanisms of the social processes by which these causal influences are brought about.''

computational approach), Granovetter studied the evolution and the equilibrium conditions of the aggregate dynamic of his model by means of difference equations.[9]

Finally, on the epistemological level, a theory of explanation focusing on the concept of mechanism, as opposed to the covering-law model, was outlined in the context of the realist approach in philosophy.

Harré (1972, p. 116, 121, pp. 136-137) explicitly linked the concept of causality with the concept of mechanism. He opposed a ''successionist theory of causality'' against a ''generative theory of causality'' according to which ''a scientific explanation of happenings, whether individual happenings or sequences of events, consists in describing the mechanism which produces them'' (p. 170). Since these mechanisms are often unobservable – Harré distinguished among ''accessible'', ''quasi-accessible'' and ''non-accessible mechanisms'' – they need to be modelled. Harré called such models ''iconic models'' (Harré and Secord 1972, p. 67).

In different terms, similar arguments were put forward by Bunge (1973, p. 92, 97), who distinguished between ''object model'' – ''a schematic representation of an object'' – and ''theoretical model'', ''a hypothetical-deductive system concerning a model object''. Depending on their degree of detail, these two types of models can assume the form of a ''black box'', a ''gray box with internal states'' or a ''translucent box equipped with a mechanism'' (*ibid.,* p. 105). Researchers should always tend towards the latter because that kind of model seeks to uncover ''the innards of things'' (*ibid.*, p. 101) or ''the modus operandi of things '' (p. 104). Bunge *(ibid.*, p. 109) termed these models ''mechanism models''.

These different strands of literature thus show that, between the 1950s and 1970s, there took shape an approach centred on the concept of mechanism. On a conceptual level, a mechanism began to be defined as a bundle of constraints and interdependent actions able to generate some macro-consequences. On a methodological level, a mechanism was conceived as an input-output function to be modelled. On an epistemological level, a relation of implication was established among the notions of mechanism, causality and explanation. However, although

[9] In the second part of his article, Schelling (1971, pp. 167-186) abandoned his approach by simulation and adopted phase diagrams to describe the equilibrium conditions at the aggregate level (SCHELLING 1973). Symmetrically, Granovetter (1978, p. 1430, note 6) substituted analytical solutions for numerical simulations when trying to complicate his initial model. The relationship between the phase diagrams outlined by Schelling and the differential equations paradigm adopted by Granovetter is evident in Granovetter and Song (1983, 1988).

each of these elements would become part of contemporary AS, none of the authors that I have mentioned used the expression AS.

2. The beginning of an AS without mechanisms

The concept of ''AS'' is not a new one. What is new is its association with the concept of ''mechanism''. For several decades, in fact, the idea of building an analytic(al) sociology or analytic(al) theory was pursued with no reference being made to the concept of mechanism.

The oldest case is *The Structure of Social Action* (1937). In the introductory chapter, Parsons enunciated what one may call the ''fundamental analytical principle'': when one wants to explain a given object, ''it shall be broken down into simpler elements which shall serve as the units of one or more theoretical system in terms of which it is to be explained'' (*ibid.*, p. 31).

According to Parsons, these elements are of two types: on the one hand, the parts or units into which the object can be decomposed (''type-part concepts'' refer to these parts); on the other, the properties which can be associated with each of these parts, and which Parsons calls ''analytical elements'' (*ibid.*, 34) (''analytical concepts'' refer to these analytical elements, see also p. 748). Parsons argued that both concepts involve an operation of abstraction (*ibid.*, p. 34). He conceived the explanation of a concrete object as analysis of the relations between the values of the analytical elements and their changes over time.[10]

Parsons called this perspective ''analytical realism'': first ''realism'' referred to the assumption that some of the concepts, in particular the analytical elements, can ''adequately 'grasp' aspects of the objective external world'' (*ibid*. p. 730); second ''analytical'' instead meant that ''these concepts correspond, not to concrete phenomena, but to elements in them which are analytically separable from other elements'' (*ibid.*, p. 730).

Parsons thus did not use the term AS, although he sometimes adopted the expressions ''generalized analytical theory'' (*ibid*., p. 601) or ''analytical theory'' (*ibid*., p. 728). However, his commentators, critics and some of his followers have employed it to denote his research program (see, respectively, Camic 1991, pp. liv-lxiv, Burger 1977; see also Parsons's (1977) reply, Bortolini 2007 and Pollini 2007).

[10] Parsons (*ibid*., p. 33, 36) used the expressions ''empirical generalization'' and ''analytical laws'' to denote the stable relations which exist, respectively, among parts and among the values of the analytical elements.

It seems that Carr (1955) was the first author to use the term AS. Although he did not give an explicit reason for doing so, the first part of his book shows that he understood AS as an exercise in conceptual classification and clarification. Carr claimed that this task was a prerequisite for field observation and beneficial to the solution of concrete social problems.[11]

This opposition between ''conceptual'' and ''concrete'' is also apparent in Katz's (1964) article, which sought to define the characteristics of ''sociologists-as-analytic-scientists'' and ''sociologists-as-applied-scientists''. According to Katz, the task of the former was ''the production of conceptual formulations'', while that of the latter was ''the harnessing of a body of existing knowledge to concrete problems'' (*ibid.*, p. 440). While applied sociologists were constantly subject to the emergency of concrete situations, sociology of analytics (p. 443) was distinguished by introspection, abstraction, explanatory ambitions, and precision in expression; its fundamental principle was ''the quest for analytic accuracy'' (p. 444).

Gert Mueller's (1967, 1969, 1989) work exhibits the greatest tension towards abstraction. In the late 1960s, he defined AS as the approach which should ''lay bare the conceptual (or categorical, or formal) framework under which empirical sociology (maps) describes social reality'' (1969, p. 86). In an unpublished paper entitled ''Analytical Sociology: An Outline'', the goal indeed became that of building a social ontology. Mueller stressed the virtues of abstraction, advocated a strict alliance between philosophy and sociology, sought to demonstrate a convergence among Husserl, Parsons and Giddens, and argued that the basis of the ''analytical theory'' is formal logic, not mathematics. The idea that ''the validity of analytical sociology is based on the coherence of its categories'' (p. 36) was defended in another unpublished paper entitled *The Logical Foundations of Analytical Sociology* (these texts are available on Mueller's website). The identification of ''analytical theory'' with a ''critical ontology revised'' received its final treatment in *Sociology and Ontology: The Analytical Foundations of Sociological Theory* (1989).[12]

[11] Carr's book received two reviews, one in *Social Forces* (1956, 34 (3)), the other in *The American Journal of Sociology* (1956, 61 (5)), both of which were rather negative in their verdicts.

[12] Mueller's book received two rather hostile reviews, in *Contemporary Sociology* (1991, 20 (1)) and in *Social Forces* (1991, 69 (3)), to which Mueller replied (*Contemporary Sociology*, 1991, 20 (5); *Social Forces*, 1992, 70 (4)). Pearce (1994) sought to carry Mueller's research program forward.

Despite their heterogeneity, these contributions thus show that the project of building an ''analytical sociology (or theory)'' was originally based on a simple idea: using abstraction and decomposition to construct clearly defined conceptual systems with which to interpret social reality. Although this idea is central to contemporary AS, none of the authors mentioned adopted the concept of mechanism which today characterizes this approach.[13]

3. The starting point of contemporary AS

Let us now consider how Gambetta justifies the introduction in 1995 of the term ''AS'' when he was creating a new cycle of seminars at the University of Oxford:

> 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 hypotheses. Fourth, a close attention to the coherence of arguments and clarity of definitions and prose (personal written communication, 28/04/2009)[14].

The fourth criterion mentioned by Gambetta shows that the concern for conceptual precision is at the heart of contemporary AS exactly as it was among those who first adopted that expression. The importance of abstraction and decomposition invoked by the ''old'' AS is also advocated by recent programmatic texts on ''new'' AS (Hedström and Swedberg 1998b, pp. 13-15, 24-25). Elster (2007, p. 455) notes: ''What one might call the analytical turn in social science in my view does not rest on the use of quantitative

[13] In regard to Parsons, for example, Fararo (2006) noted that his analyses contained implicit assumptions about social mechanisms but they did not present any ''generativity'' (Chazel 2006, pp. 363-367).

[14] By referring to ''analytical Marxism'' (for an overview, Udéhn 2001, ch. 10), Gambetta had particularly in mind Elster (1985)'s analysis of Marx's work. Incidentally, one can find in this book a statement that often recurs in Elster's writings: ''To explain is to provide a mechanism, to open up the black box and show the nuts and bolts, the cogs and wheels, the desires and beliefs that generate the aggregate outcomes'' (*ibid.*, 3).

methodology but on a near-obsessive concern with clarity and explicitness''. Hedström (2005, p. 3) explicitly accepted the concept of ''analytical realism'' coined by Parsons.[15]

However, the first three criteria stated by Gambetta show that contemporary AS is filtering the project of the ''old'' AS through the ideas associated with the first formulations of a mechanismic approach.

On the one hand, analyticity now applies to the decomposition of the internal structure of a mechanism. Conceptual decomposition is therefore a prerequisite for the construction of models whose generative capacity should be empirically testable. On the other hand, the concept of middle-range theory protects contemporary AS against excessive abstraction, the construction of a priori social ontology being the extreme manifestation of such excess (Hedström and Swedberg 1998b, pp. 5-6; Hedström and Udéhn 2009). As noted by Pawson (2000), AS indeed gives a specific content to the concept of middle-range theory; that is to say, a concatenation of mechanisms that contains at least one bundle of micro-level elements (Gambetta 1998).

However, it would be wrong to deduce that contemporary AS entirely dismisses any attempt to attain a general sociological theory. When some AS proponents express their scepticism about any general theory (Hedström and Bearman 2009b, pp. 6-7), they are questioning more the means usually adopted to achieve such a theory than its legitimacy. In this regard, according to an idea already put forward by Merton (1967, p. 68), contemporary AS suggests that we should proceed step by step by networking local models, having proven their empirical validity (Bouvier 2008). From this perspective, a mechanism can legitimately constitute the basis for a general theoretical proposition provided that it is shown to be at work behind different phenomena and, for each of them, appears under various spatial-temporal conditions involving actors with heterogeneous identities, beliefs and action logics.

II. What does contemporary AS consist of?

Let us turn to the basic core on which it seems that contemporary AS is being built. I shall argue that, although each of the elements of

[15] Some authors have even considered this ''theoretical'' component of AS to be its key feature. In his classification of sociological ''styles'', Berthelot (1996, p. 208, see also 2004) describes ''AS'' as an enterprise in conceptual clarification.

this core is not original in itself, their combination defines a research program which is specific in the context of contemporary sociology. Its most general feature is that it provides a ''syntax'' of explanation; that is to say, a set of rules on how to build and empirically test hypotheses about mechanisms underlying regularities in social life.

1. The conceptual level

Since the introduction of the concept of mechanism, it has been clear that a mechanism is not reducible to an intervening variable (in statistical terms). Rather, it is a unit with the capacity to generate the shape and intensity of the association between two or more events (see Pawson 1989, pp. 130-131). However, specifying the internal content of such a unit has proved to be much more difficult. AS now adopts a definition imported from molecular biology and neurobiology (Hedström 2005, p. 25; Hedström and Bearman 2009b, p. 5) and according to which ''mechanisms are entities and activities organized such that they are productive of regular changes from start or set-up to finish or termination conditions'' (Machamer, Darner and Craver 2000, p. 3).[16]

Within AS, norms, organizations, informal groups, social networks, interdependence structures and actors are entities of equally legitimacy and importance for building models of social mechanisms. On the other hand, reasoning, evaluation, learning, and action occupy a prominent place among the activities that one should consider. Finally, according to what is now customarily called DBO theory (Hedström 2005, ch. 3) (I shall later return to this point at length in paragraph III.b), AS focuses, among actors' properties, on desires (preferences), beliefs and opportunities.

In order to account for the genesis and the transformation of these micro-level properties, AS emphasizes the circularities that exist:

1) among desires, beliefs, and opportunities (Elster 1979, ch. 2, 1983a, 1989, 2007, ch. 3);

[16] Some commentators (Brante 2008, p. 276; Mayntz 2004, p. 239; Reiss 2007, p. 166) have noted that the status of the concept of mechanism is unclear: is it a theoretical construct or does it represent an in-world-operating-piece? The definition just mentioned shows that the most recent contributions within AS have removed this ambiguity. The definition shows that mechanisms are ''parts'' of the social world, not mere theoretical constructs. What is theoretical, by contrast, is the set of assumptions built to represent the mechanism. This point has been explicitly recognized by Hedström (2005, p. 14, note 6).

2) among desires, beliefs and emotions (Elster 1999, ch. 4, 2007, pp. 227-229);
3) among desires, beliefs, opportunities, and the interactions (or interdependencies) in which actors are embedded (Hedström 2005, ch. 3).[17]

AS accepts that actors' beliefs can be both positive and normative (Boudon 1995), and it is interested in how actors construct their beliefs within a given social context (Boudon 2001a). The instrumental conception of rationality has been extensively and severely criticized (Hedström 2005, ch. 3; Boudon 1998, 2003; Elster 1989). Other forms of rationality are often deployed, such as mimetic (Hedström 1998), evolutionary (Macy 1997; see also Macy and Flache 1995) or cognitive rationality (Boudon 1989, 1993, 1996, 1998).

It is important to stress that AS's obsession with discovering the micro-level roots of social patterns does not imply any form of reductionism. Upstream, the beliefs, desires and opportunities of each actor are constrained by a plurality of structures. Downstream, the action of each actor influences other actors' beliefs, desires and opportunities, contributing to the creation of structures which in their turn will constitute the constraints on future actions (see also Abell 1996, p. 261; Boudon 1987, p. 46; Coleman, 1993, p. 63).

AS's endeavour to systematically link structures, actions and interactions to explain the genesis of macro-regularities thus induces it to defend a weak form of methodological individualism (Hedström and Swedberg 1998b, pp. 12-13; Hedström 2005 p. 5, note 4; Hedström and Bearman 2009b, p. 8). This variant of methodological individualism, which recognizes the centrality of norms and relational structures, has been called ''structural individualism'' since the late 1970s (Wippler 1978; Raub 1982, and for a synthesis Udhén 2001, ch. 10, 12).

However, any general theory on either the links among structures, interactions and activities or the links among the internal properties of these entities is beyond the scope of the AS research program. AS only sets a constraint on how to proceed: namely, the requirement that a mechanism must be clarified whenever one mobilizes entities and/or activities between which a link is postulated to exist. AS does not exclude the possibility of inserting new entities, properties or activities

[17] The desires/beliefs/opportunities triad that AS considers to be the starting point for conceptualizing social action is thus identical to the beliefs/preferences/constraints triad that Fehr and Gintis (2007) and Gintis (2007) have proposed as a model of the actor able to unify all behavioral sciences.

(for instance, neurons below beliefs). Again, the only constraints that AS imposes are that the introduction of these new entities must be justified, their status clarified, and their link with preexisting ones specified. In short, AS contains nothing more than a principle of representation of the explanation.[18]

2. The epistemological level

The syntactic rule ''to explain an observation is to generate it'' implies that there is no causal explanation without empirical evidence of a mechanism. Thus, on an epistemological level, contemporary AS is based on the ''generative theory of causality'' proposed by Harré in the early 1970s.[19]

On these bases, AS judges as inadequate the conception of causal explanation developed by Hempel (1965, ch. 10 and 12, 1966, ch. 5), and according to which explaining is to deduce the instance to be explained from a general law, given certain conditions (Cherkaoui, 2005, ch. 4; Elster 1983b, pp. 44-48; 1999, ch. 1, 2007, ch. 2; Hedström and Swedberg 1998b, p. 8; Hedström 2005, pp. 15-20; Hedström and Udhén 2009; see also Fararo 1989, p. 44, 53, 55).

The objection does not concern the deductive character of nomological explanation. Any mechanismic explanation indeed involves a logical deduction (Hedström 2005, p. 30).[20] Nor do reservations

[18] If one accepts that AS only provides a ''syntax'' of explanation, one can avoid a tedious misunderstanding. Proposing the concept of mechanism as the basis for sociological analysis should not be equated with proposing a mechanistic conception of the social world. This misunderstanding would be tedious because it directs AS to matters which it is unable to discuss, that is to say, metaphysical matters. The structural variant of the methodological individualism advocated by AS leads to the development of theoretical models in which actors are neither completely free nor completely constrained. But the real balance between chance and determinism underlying a given macro-regularity is for AS an empirical matter which can only be solved case by case.

[19] This conception of causality has been criticized on the following grounds: to the extent that any mechanism necessarily contains some associations among entities/properties/activities which have not yet been unpacked, a final explanation would be impossible because each mechanism entails other mechanisms in an infinite regress (Opp 2005, p. 169; Pisati 2007, p. 7; Steel 2004, pp. 61-64). AS recognizes the logical paradox but points out that the decomposition of mechanisms is in practice a gradual process (Elster 1983b, p. 24; Hedström and Swedberg 1998, p. 10, note 12; Hedström 2005, p. 27, note 16). On the other hand, the criteria adopted to stop back-regressing reside in the point of view adopted in studying a given problem, as well as in disciplinary boundaries, both of which are intrinsically historical constraints (Hedström 2005, p. 27).

[20] Norkus's (2005, p. 355) criticism – which reformulates in deductive-nomological terms some of the mechanisms discussed by Elster – simply demonstrates a similarity between the covering-law model and explanation by means of mechanisms that AS itself recognizes.

amount to denying the importance of nomic knowledge for the construction of a mechanism. In this regard, if one identifies a law with a probabilistic regularity constructed inductively through statistical analysis (which is one of the options allowed by Hempel himself), AS is perfectly open to the use of ''social laws''. As I shall say later, introducing empirical regularities of this type into formal models particularly well suited to the study of social mechanisms is one of the proposals that AS makes in regard to the complex problem of how models of social mechanisms can be empirically tested (Hedström 2005, ch. 6).

Actually, AS is discontent with the covering-law model because it does not require the insertion of mechanisms into the general laws that it demands to mobilize (Hedström 2005, p. 17; among philosophers, see, for instance, Bunge 1997, pp. 411-412; 1998, p. 75).[21]

AS makes a similar objection against the probabilistic variant of the covering-law model – which Hempel (1965, ch. 10, pp. 250-251, 278, ch. 12, pp. 376-412, 1966, ch. 5, pp. 58, 59) himself called ''statistical explanation'' – especially in its elliptic form ''X because Y'' obtained when the explanans lacks certain conditions (Hempel 1965, ch. 12, p. 415; 1966, ch. 5, pp. 52, 53).

This form of the covering-law model, which lies at the core of the statistical methods in quantitative sociology, is unsatisfactory, AS argues, because the coefficient of a statistical model quantifies at best the effects of a mechanism; it cannot provide either a representation of its internal structure (and dynamic) or its relations with other mechanisms (Cherkaoui 2005, ch. 4, pp. 98-103; Hedström 2004; Hedström 2005, ch. 5; Hedström and Swedberg 1998b, pp. 9-10, 15-17; see also Goldthorpe 1999).[22]

[21] Opp's (2005, pp. 174-177, 2007, pp. 117-118) and Sawyer's (2007, p. 259) criticism thus seems misplaced. Far from denying that laws can be used in the construction of models about mechanisms, AS only questions the ''causal depth'' of the laws that are mobilized and, on the other hand, obliges consideration of the difficult problem of knowing what a law might be in the study of social phenomena. In that sense, I do not believe that, as claimed by Opp (2007, p. 122), AS's skepticism with regard to the covering-law model is ''absolutely superfluous''.

[22] This objection has distant origins (Schelling 1971, p. 147). AS argues in the same vein as Bunge (1997, p. 444): ''where does the preceding leave the so-called explanation of variables sought by many social scientists and statisticians? Strictly speaking, there just is no such thing. [...] there is no explanation of variables: there is only either an analysis of variables or a mechanism describable by certain functionally related variables''. Finally, note that the failure of statistical/econometric models applied to non-experimental data in order to detect true causal relationships is now widely recognized as far beyond the scope of AS (Abbott 1992; Fehr and Gintis 2007, p. 44; Freedman 2005; Sobel 2006; Winship and Morgan 1999).

However, this objection against the variable explanation and statistical causality should not be interpreted as an attack against the importance of empirical description:[23]

> We do not wish to suggest that quantitative empirical research is of minor importance for the sociological enterprise. Quite the contrary: Quantitative research is essential both for descriptive purposes and for testing sociological theory (Hedström and Swedberg 1998b, p. 17).

3. The ontological level

The syntactic rule ''to explain an observation is to generate it'', on which the AS concept of causality is based, does not require this approach to defend, at the ontological level, the existence of a hierarchy of levels of reality with sui generis nature.

Although AS considers mechanisms to be pieces of the real world, it only draws distinctions between levels of analysis; it does not do so between levels of reality. In this regard, the analytical realism advocated by AS differs from certain variants of contemporary realism in philosophy, such as the ''critical realism'' developed by Baskar (Hedström 2005, pp. 70-74; Pawson 2000 and Barbera 2008).

Thinking in terms of levels of analysis means that, firstly, when one is building a model of a mechanism, the attribution of a given entity/activity to a given level of analysis depends on the type of entity/activity that one finally want to generate. Paradoxical as it may seem, this means that the same entity can assume several analytical statuses within a single model. In this sense, Hedström (2005, p. 74) notes that ''notions of levels, as they are used here, are always relative to the mechanism under consideration''.[24]

[23] As Brante (2008) has already noted, it is thus incorrect to accuse AS of underestimating description (Bernardi 2007, Opp 2005, 2007, Reiss 2007). Far from neglecting the role of statistical analysis, AS simply redefines its scope (Hedström 2005, p. 21), which again is a rather old idea: ''for the believer in generative causality the existence of the statistics is but the first step in a long process of investigation which ends only when the nature of the things involved has been found out and the reasons for the statistics thus elicited'' (Harré 1972, p. 118).

[24] One of the criticisms brought by Abbott (2007b, p. 2) against AS – *i.e.* AS is based on a concept, that of level, which is inconsistent – thus seemingly fails to recognize the essential distinction between levels of reality and levels of analysis. From an ontological point of view, Abbott is certainly right to assert that ''there are no levels''. However, when one is trying to build, not a social ontology, but theoretical models which progressively help illuminate how social reality works, it seems difficult to forgo an analytical distinction between ''levels'' (by the way, Abbott does not suggest an alternative strategy).

Thinking by levels of analysis has a second important implication for the ontological status of the causal entities/activities postulated to build models of mechanisms. While each entity mobilized in the construction of a given mechanism may be relevant from a causal point of view – that is to say, it can contribute to producing a given change in a given state of the world – that entity may nevertheless not be sufficient in itself from a generative point of view – that is to say, it needs other entities belonging to other levels of analysis to make effective changes in the state of the world that one is seeking to explain. That is why Hedström and Bearman (2009b, p. 11) claim that ''the micro-to macro relationship is a parts-to-a-whole relationship rather than a cause-to-effect relationship''.

Thus, by distinguishing between ''causal relevance'' and ''generative sufficiency'', AS proposes an elegant and conceptually simple solution to the apparently unsolvable problem of what level comes first and/or matters more.

4. The methodological level

The syntactic rule ''to explain an observation is to generate it'' raises an important issue. Demonstrating that a concatenation of mechanisms A produces an outcome O is a necessary but insufficient condition for claiming that A explains O. For this purpose, one must eliminate the possibility that other concatenations of mechanisms $A_{1,[\ldots],n}$ are able to generate O in as satisfactory a manner as the concatenation originally postulated.

This problem – known in philosophy of mind as ''multiple realizability'' (for a pedagogic introduction, see Bickle 2006) – concerns the study of a mechanism once it has been theoretically designed. Studying a mechanism in its turn raises two distinct problems: first, analysis of its behavior (how does it work and what does it produce?); second, empirical validation of the mechanism (are its internal components, the process that it has triggered, as well as the outcome that it has finally generated, empirically grounded?).

AS comprises a formal as well as a qualitative approach to both phases. While the formal approach aims at integrating formal models and quantitative techniques (Hedström 2005, ch. 6; Hedström and Bearman 2009b, p. 16; Manzo 2007a), the qualitative one emphasizes natural logic and qualitative methods (Elster 2009, pp. 445-467). However, what is really at stake is finding the way to articulate these methodological options.

As far as the formal approach is concerned, AS considers statistical methods to be inappropriate for analysis of how a mechanism works. This does not mean, however, that such methods are useless. On the contrary, AS gives them an important role in testing a model of a mechanism. First, statistical analysis is necessary to compare the macro patterns generated by a model against empirical data describing the actual macro pattern of interest (Manzo 2009a). Second, statistical analysis is needed to introduce empirical regularities as inputs to formal models that explicitly represent mechanisms (Hedström 2005, ch. 6).

Among such models, AS focuses on mathematical and computational ones in particular.

Mathematical models have been spontaneously associated with a mechanismic approach since the 1960s. However, we now know that these models capture only the surface of a model mimicking a mechanism. Only a form of ''systemic causality'' has a place in formal models such as differential equations or markovian models (Doreian 1999, p. 88). Here, given some initial conditions, a set of parameters and some equations, only aggregate dynamics are studied. These equations contain some rules on transition between two high-level states, but they do not yield any fine-grained formalization of action or rules on interaction between heterogeneous low-level entities (among philosophers of sciences, see Bunge 1997, p. 423). When this happens, one has switched to a form of ''causality'' called ''algorithmic'' (Doreian 1999, p. 99). In this case, however, if the mechanisms to be represented are too complex, it may be difficult to find an appropriate mathematical representation of them; or if a representation exists, the analytical solution may be absent (Axtell 2000; Hedström 2005, pp. 87-98).

Computational methods help solve this problem (Fararo and Butts 1999, p. 35). Although they cannot produce general theorems, they can deductively generate patterns from mechanisms as complex as the theory requires. This applies in particular to the agent-based models which have recently attracted the interest of some AS scholars (for technical aspects, see Ferber 1999; for more general overviews of the literature, see Gilbert 2007, Macy and Willer 2002, Miller and Page 2007, Sawyer 2003).

This flexibility stems from the specific programming language on which the design of an agent-based model is based. The core of the program consists in a set of ''objects'', *i.e.* computational units defined by certain properties (attributes) and rules of behavior (methods or

procedures). According to the bundle of properties and rules defining an object, the latter can be used to model the behavior and interactions of a set of particles, molecules, cells, beliefs, actors, groups (of particles, molecules, etc.), organizations, etc.

There are therefore deep-lying reasons why some analytical sociologists have begun to explore this class of computational models. A structural homology exists between thinking in terms of mechanisms and building an agent-based model. As a mechanism consists of entities, their properties and their activities, so an agent-based model is made up of objects, attributes and procedures which define the behavior of the object. Hence, when one is designing an agent-based model, one is building an artificial mechanism. On the other hand, simulating an agent-based model means iterating the rules that define the objects of which it is made up and which finally enable objects to communicate with each other and to update their attributes over time. Hence when one simulates an agent-based model, one is activating in silico the process that the mechanism potentially contains.

What this process generates is exactly what AS seeks: evidence that a given entities/properties/activities triad is able to generate a given set of associations among certain aggregate events. It is precisely the generative capacity of a mechanism that an agent-based model makes it possible to implement. As noted by Epstein (2006, p. 8), ''Agent-based computational models provide demonstrations that a given micro-specification is in fact sufficient to generate a macrostructure of interest''. Such a demonstration, moreover, has a deductive character: ''every realization of an agent model is a strict deduction'' (*ibid*., p. 56).

As powerful as this technique may seem in studying how a mechanism works, it is nevertheless unable by itself to validate the empirical relevance of the mechanism. For this purpose, it is necessary to inject empirical information into both the behavior rules of the entities composing an agent-based model and the relational structures which supposedly impact on them.[25]

Hedström's above-mentioned proposal concerning empirically calibrated agent-based models identifies such empirical information with previously-highlighted statistical regularities. However, as the literature on the problem of validating an agent-based model shows

[25] It is of course possible to use agent-based models as tools for pure theoretical exploration. Within AS, Macy's work nicely illustrates such use (Centola, Macy and Willer 2005). Manzo (2009b) has also recently made outcome-range-oriented use of this technique in order to revisit the classic theoretical problem of relative deprivation.

(Fagiolo, Windrum and Moneta 2007; Moss and Edmonds 2005; Moss 2008), qualitative materials can also be used to initialize an agent-based model empirically.

This is exactly where the qualitative orientation of AS (Gambetta 1993, 2009a; Gambetta and Hamill, 2005), as well as its interest in combining survey and experimental method (Bohnet 2009; Ermisch and Gambetta 2008), can integrate more formal approaches together.

As recent trends in behavioral economics show, only a permanent exchange among formal models, laboratory experiments, survey and qualitative research enables solution to the complex problem of how models dealing with social mechanisms can be constructed and empirically tested (Fehr and Gintis 2007). The creation in 2008 of the Center for Experimental Social Sciences (CESS) at Nuffield College (Oxford University) on the initiative of D. Gambetta (with R. Duch and P. Young) demonstrates that AS is seeking to develop a ''hard'' integrative methodology of this kind.[26]

III. Do the critics of AS have good reasons to be critical?

As some footnotes have suggested, both the overall literature on AS and its historical background tend to show that many criticisms against AS have weaker bases than the critics claim. Is it also the case of the most recent attack brought against AS, namely Gross's (2009) critical analysis? [27]

A general point made in Gross's critical analysis should be welcomed without reservations: the need for better specification of the conditions under which a postulated mechanism is triggered. As shown in particular by Elster (2007, chap. 2), AS is perfectly aware that this is a central problem on which more (empirical) work is still needed.

[26] In light of the elements just discussed, it thus seems that criticizing AS for being an approach which simply, and naively, relies on only one technique, namely agent-based modeling (see for this objection, Abbott 2007b, p. 1; Lucchini 2007, pp. 236-240, 2008, pp. 9-12, Sawyer 2007, p. 260), is to caricature the approach's content rather than discuss the complexity of its methodology in depth.

[27] It should be noted that Gross never addresses his charges directly against AS as such. However, he pays so much attention to Hedström and Swedberg (1998b), and especially to Hedström (2005), that it does not seem inappropriate to take account of the broader AS context in discussing Gross's analysis.

However, Gross's criticisms, as well as his positive proposals, are in fact less able than he believes to engender a conceptualization of social mechanisms stronger than the one outlined within AS.

My argument will be three-fold: 1) Gross's synthetic definition of the concept of mechanism reintroduces, rather than dissipating, some ambiguities that AS is trying to eliminate; 2) Gross's diagnosis that the DBO theory is inadequate because it is simply a variant of rational choice theory cannot be defended on the basis of the existing literature within AS; and 3) the conceptual premises on which Gross bases his alternative conceptualization of social mechanisms – according to which habit, not intentionality, is the most useful theoretical foundation for a realistic theory of action – actually restricts, rather than expands, the bases for the analysis of social mechanisms, without this restriction being justified in light of the empirical and experimental evidence today available.

1. How AS defines the concept of mechanism

To reduce the ambiguity surrounding the concept of mechanism, Gross (2009, pp. 360-362) first considers five definitions and tries to build a synthetic one, which he terms a ''working definition''. According to this definition, a (social) mechanism is a:

> more or less general sequence or set of social events or processes analyzed at a lower order of complexity or aggregation by which – in certain circumstances – some cause X tends to bring about some effect Y in the realm of human social relations. This sequence or set may or may not be analytically reducible to the actions of individuals who enact it, may underwrite formal or substantive causal processes, and may be observed, unobserved, or in principle unobservable.

First to be noted is that AS shares Gross's concern to overcome the heterogeneity of the meanings associated with the concept of mechanism (see, for instance, the list of definitions compiled by Hedström 2005, p. 25 and Hedström and Bearman 2009b, pp. 5-6).[28] Then, recent contributions in AS tend to agree on a definition that Gross curiously does not consider:

> mechanisms can be said to consist of entities (with their properties) and the activities that these entities engage in, either by themselves or in concert with other entities. These activities bring about change, and the type of change brought about depends upon the properties of the entities and the way in which

[28] An even more comprehensive list can be found in Mahoney (2001, pp. 579-580), and, in political science, in Gerring (2008).

> the entities are organized spatially and temporally. A social mechanism, as here defined, is a concept used to describe a constellation of entities and activities that are organized such that they regularly bring about a particular type of outcome (Hedström 2005, p. 25).

Compared to this definition, it is unclear whether Gross's definition enables us to make any real progress.[29]

Firstly, since any ''event'' or ''process'' ultimately refers to entities and activities (regardless of their specific content), a definition that focuses on entities/properties/activities triads seems more accurate than a definition which refers generically to such notions as ''events'' or ''processes'' – which are at least as unclear as the concept to be defined.

Secondly, because Gross's definition tends to equate a mechanism with a process, it introduces an ambiguity which instead should be resolved: what differentiates a ''mechanism'' from a ''process''? As has been noted (Vayda *et al*. 2001), the concept of process is often itself only a cover concept: as a consequence, it appears that it cannot be usefully employed to specify what a mechanism is. As I pointed out earlier, AS by contrast suggests a criterion: a process is the dynamic side of a mechanism; that is to say, the sequence of changes triggered by the activities (and interactions) of the entities contained in the mechanism.[30]

In the second part of his article, Gross (2009, p. 368) suggests a more specific definition of the concept of mechanism, one based on the alternative theory of action inspired by the pragmatism that he defends. However, to consider the scope of this second definition, it is better first to consider the reasons which induce Gross to refer to this theory of action.

2. How AS conceives the DBO theory

According to Gross (*ibid*., p. 365), current conceptualizations of social mechanisms have a common shortcoming: they adopt a theory of action which is inadequate. This is because they use assumptions

[29] I am focusing here on the first part of Gross's definition because the second part does not require any specific comment. As Gross himself admits, the second part of the definition simply collects epistemological options on which authors often disagree. In this sense, Gross's proposal simply records disagreements without showing how they can be resolved.

[30] In that Gross considers Ch. Tilly to be one of the authors who proposed a conceptualization of social mechanisms close to his own, it is surprising that he makes this amalgam between mechanism and process. Tilly is indeed one of the rare authors who have explicitly attempted to analytically separate the two concepts (McAdam, Tarrow and Tilly 2001, ch. 3).

which by and large fall within the domain of rational choice theory or its variants such as DBO theory.

A careful reading of Gross's article shows that he detects six weaknesses in this theory of action: 1) rationality is an innate ability which is more or less homogeneously distributed in social space (p. 365); 2) beliefs and desires are clear and consistent entities (p. 365); 3) ends distinctively come before means (p. 365); 4) action is intentional (p. 365); 5) action maximizes utility (p. 367); and 6) the intimate cultural nature of the action is ignored (p. 367). On each of these points, the pragmatist approach advocated by Gross admits, by contrast: 1a) a plurality of forms of rationality whose historical origins require explanation (p. 367); 2a) a ''psychological postulate of ambiguity'' (p. 365); 3a) ''means and ends [...] are often emergent from action'' (p. 367); 4a) ''much action is habitual and typically involves no conscious weighing of means and ends'' (p. 367); 5a) ''pragmatism does not equate problem solving with maximization of utility'' (p. 367); and 6a) ''pragmatists insist that problem situations are always interpreted through cultural lenses'' (p. 367).

This diagnosis immediately raises two questions. First, to what extent is it correct to claim that AS is basically a new instance of rational choice theory? Second, does DBO theory really have shortcomings similar to those that Gross detects in rational choice theory? My impression is that Gross is simplifying a much more complex picture.

Let us consider the first question. In this regard, the most general feature to be taken into account is that key authors in analytical sociology have expressly distanced themselves from a strict version of rational choice theory. Hedström (2005, p. 60), on whose theory Gross focuses in particular, says:

> my own initially highly positive attitude towards rational-choice sociology has gradually been tempered by what I consider to be an unfortunate instrumentalist tendency among many of its practitioners.

By ''instrumentalism'', Hedström means the epistemological position according to which unrealistic theoretical premises should be accepted provided that they lead to good predictions and/or to elegant formalizations. According to Hedström, despite the fact that any theory leads to some simplification, a theory of action should be based on assumptions which are only descriptively incomplete but not completely false.[31]

[31] Curiously, Gross refers to the section where Hedström makes this argument but does not draw any consequences from his criticism. This is all the more surprising because, as one easily deduces from notes 8 and 9 of his article, Gross endorses Hedström's argument (I shall return to this point later, see section III.3).

Let us also recall that Boudon (2001a, pp. 451-452) explicitly argued that rational choice theory cannot claim to be a sustainable theory of action for social sciences. According to Boudon, three of its premises – ''consequentialism'', ''egoism'' and ''cost-benefit balance'' – excessively restrict the theory's domain of validity.

Last but not least, Elster (2007, p. 5; for an overview of his perplexities, see also pp. 462-463) wrote:

> I now believe that rational-choice theory has less explanatory power than I used to think. Do real people act on the calculations that make up many pages of mathematical appendixes in leading journals? I do not think so.

If one separately considers points 1 to 6 above, it seems equally difficult to argue that DBO theory is a variant of the orthodox form of rational choice theory. On the contrary, careful inspection of the AS theoretical and empirical literature even suggests that each of Gross's counterproposals 1a to 6a already has its place in the way that AS conceives DBO theory.[32]

Firstly, DBO theory makes no assumption concerning the particular type of rationality which drives actors:

> DBO theory makes no assumption that actors act rationally [. . .]; it only assumes that they act reasonably and with intention (Hedström 2005, p. 61).

In particular, this perspective does not at all postulate that actors are uniformly and constantly provided with the informational and computational capabilities assumed by the orthodox form of rational choice theory:

> DBO theory does not exclude the possibility that actors in sufficiently transparent environments may act according to the canons of rationality, but such situations are rare, and it therefore seems inappropriate to use rational-choice theory as the general point of departure (Hedström 2005, p. 61).

On the contrary, rationality takes diverse forms in AS: ''cognitive rationality'' is probably the most general of them (Boudon 1989, 1993, 1996, 1998, 2003, 2006). In that framework, actors are supposed to theorize the context in which they act and develop systems of reasons which they perceive as strong, given the material, symbolic and cognitive resources to which they have access. ''Mimetic rationality'' is another form of rationality used by AS to model the actor's logic of action (Hedström 1998). Here, actors build their beliefs by

[32] It should be noted that the credibility of criticism against DBO theory would be improved if it were based on careful analysis of Elster's numerous books in which he has proposed, extended and defended such an image of actors, instead of restricting itself to its intentionally concise presentation in Hedström's (2005) programmatic manifesto.

exploiting other actors' actions as sources of information. Finally, Macy (1997, pp. 435-438, 1998) has introduced into AS a form of rationality which he describes as ''emerging'' because actors are supposed to choose, not on the basis of the future consequences of their actions, but by thinking about past actions which have shown adaptive capacity.

Thus, within AS, DBO theory is far from being a variant of rational choice theory. Contrary to what Gross claims, the reverse proposition holds: rational choice theory is an instance of DBO theory (Hedström 2005, p. 41).[33]

The second shortcoming stressed by Gross concerns the exaggerated extent to which DBO theory conceives actors' beliefs and desires as consistent and clear entities.

In this regard, the first response to Gross's objections centres on the constant work undertaken by many authors within AS to make actors' beliefs and desires endogenous.

I have already mentioned, on the one hand, Elster's analysis of the dynamic interdependency among beliefs, desires and opportunities, and, more recently, between emotions and beliefs, and, on the other, the role that Hedström attributes to dyadic interactions in explaining the genesis of actor's beliefs and desires. We may now also consider Rydgren's (2009) chapter on beliefs formation in Hedström and Bearman's *The Handbook of Analytical Sociology*, where he studies the role performed by mechanisms of categorization, inference, deduction, network and cognitive dissonance in shaping actors' beliefs. One of the interests of Rydgren's treatment (I shall mention a second one later) is that it demonstrates how these mechanisms can give rise to beliefs that are ''biased and flawed'' (*ibid*. p. 73). Moreover, Rydgren stresses that these mechanisms often operate unconsciously (*ibid*., p. 73, 84). As a consequence, he notes, ''beliefs are not transparent'' (*ibid*. p. 89).

Elster (2007, p. 212) explicitly makes the same point:

> Any choice-based explanation of behaviour is subjective. Not all subjective explanations assume, however, the transparency of the agents to themselves and

[33] On a methodological level, the choice of some authors within AS to develop agent-based models demonstrates their theoretical conviction as to the existence of multiple forms of rationality variously distributed across social groups. From a technical point of view, one of the main interests of this method is that it makes it possible to envisage different types of actors driven by different behavioral rules (which dynamically evolve) within the same model (for trenchant criticism against standard choice rational theory from a computational viewpoint, see Epstein 2006, ch. 1).

> the relentless search for optimality that are the hallmarks of rational-choice explanations.

Thus, contrary to what Gross claims, first, unlike standard rational choice theory, DBO theory conceives beliefs and desires as explananda rather than taking them for granted. Second, DBO theory admits that beliefs and desires are often inconsistent and irrational (Elster 2007, p. 211). Third AS aims at modeling mechanisms that account for such inconsistency and admits that they tend to operate behind actor intentionality.[34]

Gross then criticizes DBO theory for its linear conception of the ends-means relation. Often, he remarks, actors' goals, far from preceding their action, are formed during, or even after, such action.

As we have seen earlier, one of the forms assumed by actors' rationality in the context of DBO theory is what Macy (1997, 1998) calls ''emergent rationality'', which implies that actors build the means-ends relation by trial and error on the basis of actions and interactions that they experience over time. As Macy (1997, p. 435-438) puts it:

> Evolutionary and learning-theoretic models, in contrast, posit iteration, not intention, as the link to the future. More precisely, outcomes attract action via gradient search, without the need for a map of the evolutionary landscape. The outcomes that matter are those that have already occurred [. . .]. Iterative search also relaxes the highly restrictive cognitive assumptions in rational choice theory. [. . .] We thus arrive at the possibility that rationality can emerge from behind the backs of the actors.

To be noted is that Macy (1997, p. 437, 444, note 3; 1998, pp. 222-223) explicitly links this conception of rationality with the pragmatist tradition. He also notes that ''emergent rationality'' is central to an important branch of game theory, *i.e.* evolutionary games (thus showing that it is incorrect to regard game theory as the prototype of orthodox rational choice theory). Macy (1997, pp. 441-443, 1998, p, 234; Macy and Flache 1995) finally insists on the possibilities that simulation methods open for the formalization of such complex forms of rationality. Macy and Flache's (2009) chapter in *The Handbook of AS* has recently iterated this argument.

Although Gross (2009, p. 365, note 7) acknowledges that Macy's contribution is not affected by his objection against DBO theory, he draws no consequences from such a criticism. However, the

[34] Boudon (1995, p. 71, 78, 111; 1999, p. 158; 2001b, pp. 48-49) has also often stressed that actors tend to build and perceive their subjective reasons in an ''intuitive and meta-conscious mode''.

compatibility between DBO theory and Macy's conception of rationality is explicitly admitted within AS:

> Learning theories should not necessarily be seen as an alternative to the DBO theory, however. In my view they should be seen as a specific type of DBO theory that is applicable when actors use information about the past to decide what to do in the future [...]. DBO theory is, for example, perfectly compatible with a selectionist trial-and-error account of action, and it recognizes the importance of various cognitive biases (Hedström 2005, p. 41, 61).

Let us now temporarily set aside the fourth objection that Gross raises against DBO theory – *i.e.* the intentional nature of the action – and first discuss his two last objections, those regarding the role of maximization and culture in DBO theory.

In regard to the first point, while it is undeniable that the maximization principle is at the heart of standard rational choice theory, one can easily deduce from the three points discussed so far that DBO theory contains no element which suggests that maximization is the driving principle of actors' action, which is clearly shown by Hedström's (2005, p. 61) critical assessment of the assumption of ''optimality''.[35]

By contrast, the second point, which concerns the residual place of culture and the simplistic conception of the meaning of individual action within the DBO theory, warrants more detailed discussion.

A first rejoinder to Gross's objection can be found in Rydgren's work. His above-mentioned analysis of the socio-cognitive mechanisms of belief formation indeed fits with a more general effort to integrate ''cognition'' and ''culture''. In a study in which Rydgren draws on DBO theory to understand the role played by the past in the genesis and persistence of inter-ethnic conflicts (2007, p. 226), he writes:

> Only by specifying the situations in which people are embedded may we assess the reasons for their beliefs and actions and understand group-specific uniformities in belief formation. In this article, the concept of culture will be central to this understanding. Following Swidler, culture should in this article be understood as ''the publicly available symbolic forms through which people experience meaning'' (1986, p. 273). Such symbolic forms include language, rituals, ceremonies, narratives, art forms, various institutionalized practices, and so on. Culture, so conceived, offers a tool-kit of symbolic forms that people draw upon.

[35] I note in passing that we may all be wrong on this point. In an article advocating closer integration among social sciences, biology and neurosciences, Gintis (2007, p. 5) identifies a number of studies in neurosciences which ''suggests that maximisation is not simply an as-if story ''.

Cultural embeddedness is also a core component of the ''cognitive rationality'' defended by Boudon. One of the problems with this conception of rationality is that it is difficult to define what constitutes a ''strong'' reason. Boudon proposes two criteria in this regard (1995, p. 34, 51, 69, 79, 133-134, 189-196; 200, 289, 370; 1996, p. 124, 128, 147; 1999, p. 93, 133, 135). A system of reasons will be strong on the one hand if the actor perceives it as grounded and consistent, and on the other, if he judges that his reasons are able to convince other actors (Boudon 1995, pp. 67-68, calls this second dimension of a system of reasons ''trans-subjective''). Of course, the problem here is determining the ultimate source of this subjective conviction in grounds of one's own system of reasons. Boudon (1996, p. 148) stresses the relative character of this ultimate foundation, maintaining that it depends on two factors: first, the quality and quantity of the information to which the actor has access; and second, the set of beliefs and symbols taken for granted in a given community. This is why Boudon (2001a, pp. 455-456) suggests that, in order to emphasize the informational and cultural component of actors' system of reasons, the concept of ''cognitive rationality'' should be labeled ''contextual rationality''.

Gambetta's 2009a and b recent work on signal theory also shows that, contrary to Gross's claims, DBO theory is completely compatible with the assumption that actors always act through ''cultural lenses'', to use Gross's expression. At the heart of the ''signaling theory'' applied by Gambetta is the following problem: how can ego believe in the trustworthiness of alter's signal in a context where alter has an interest in concealing the real state of affairs, and where ego's information about alter is imperfect? Of interest here is that Gambetta (2009b, paragraph 3) stresses that solving this problem requires account to be taken of the norms and symbols which populate actors' social universes, because, in real social interactions, the credibility and, more fundamentally, the meaning of a signal cannot be separated from these symbolic universes.

Thus, in the light of the elements discussed so far, it seems unsound to argue, as Gross does, that DBO theory has all the shortcomings of standard rational choice theory. In fact, DBO theory gradually builds on multiple theoretical and applied contributions whose common goal is to go beyond the shortcomings of rational choice theory. These contributions show that DBO theory now contains the following elements:

1) it makes no assumptions on the specific form of actors' rationality;
2) actors' ends and means may arise from their action rather than precede it;
3) actors' beliefs and desires are conceived as explananda, not as exogenous elements of the problem;
4) actors' beliefs and desires are not conceived as being invariably clear, transparent and consistent;
5) the cognitive and relational mechanisms responsible for actors' beliefs and desires are conceptualized as being able to operate unconsciously;
6) actors' beliefs and desires are inseparable from actors' perceptions of the symbolic context in which they act.

One factor which may help explain these misunderstandings is that AS conceives DBO theory as only a specific part of a broader framework within which the analysis of mechanisms should be conducted. As we saw earlier, this framework equates to a complex form of methodological individualism which stresses the existence of recursive loops among context (values, norms, conventions, etc.), structure (organizations, groups), networks and actions (with its internal components). DBO theory is only the starting point for analysis of the micro component of such loops – I say ''starting point'' because I have sought to show that DBO theory is itself susceptible to very diverse sorts of refinement. Hence DBO theory should not be separated from this larger framework, whose purpose is to conceptualize the circularity between structure and action, not to reduce the former to the latter.

3. Why AS is discontented with the concept of habit

Now it is possible to assess the extent to which Gross's (2009, p. 368) second definition of the concept of mechanism is able to furnish a conceptualization of social mechanisms more sophisticated than the one based on DBO theory. According to Gross's second definition:

> Pragmatists would view social mechanisms as composed of chains or aggregations of actors confronting problem situations and mobilizing more or less habitual responses.

First to be noted is that Gross's definition can be easily reframed in terms of the entities/properties/activities triad on which the AS definition of mechanism focuses. In Gross's framework, indeed, entities equate to actors (individual or collective); the properties of

entities equate to actors' habits (and resources) (Gross insists on resources a few pages after the definition just mentioned); activities equate to actors' efforts to solve given problems within given contexts. In terms of DBO theory, moreover, the property ''habitus'' provides the property ''belief'' with a specific content: without any loss of generality or accuracy, a habit can indeed be thought of as a ''latent'' belief in the effectiveness of a particular mode of action; the property ''resource'' falls into the category of ''opportunities''; and the action principle according to which actors are problem-solvers is only a specific form of desire – desire or preference for adaptation.

If one then considers the broader context of structural methodological individualism in which AS inserts DBO theory, there is an even more obvious compatibility between Gross's general conceptualization of social mechanisms and the one which can be built on DBO theory.

In this regard, let us return to Boudon's (1986, ch. 2) formulation of this complex (in the sense of ''recursive'') form of methodological individualism. Boudon (1986, p. 51) remarks in his presentation:

> The microsociological point m(S) in a sociological analysis can be briefly said to consist in bringing out the adaptative nature of a pattern of behaviour with reference to a situation.

Apart from the specific choice of words and the reference to a different philosophical tradition, my impression is that there is only one substantive difference between this principle and Gross's conceptualization. While Boudon makes it explicit that a complex form of methodological individualism does not contain any specific model of actor Gross starts from exactly the opposite postulate. He believes that the adaptive character of social action must be conceived in terms of habit.[36]

We now have all the elements to go back to Gross's counter-proposal (4a in my above list) according to which habit, and not intentionality, should constitute the basis for a robust theory of action. This is the crucial step in Gross's criticism of DBO theory and the main building block of his alternative conceptualization of social mechanisms.

My point here is that the empirical scope and applicability of the concept of habit is narrower than Gross seems to postulate.

[36] I note in passing that Gross (2009, p. 369) admits that his perspective is close to the weak form of methodological individualism invoked by Hedström and Swedberg. The difference, he says, is Hedström's simplistic conception of the problem of the subjective ''sense'' of individual actions. As I sought to show earlier, however, Boudon, Gambetta or Rydgren's work suggests that this objection is not sound when one considers the wider AS domain.

What criteria does Gross mobilize to claim that a theory of action which builds on the concept of habit is more ''viable'' (p. 359), ''sophisticated'' (p. 359), ''solid'' (p. 359), ''adequate'' (p. 365) or ''promising'' (p. 365) than DBO theory? Since he claims that ''few in the theory community agree that rational choice theory or variants such as DBO theory offer empirically or theoretically adequate descriptions of social action'' (*ibid*., p. 365), one may expect that both empirical and theoretical criteria are discussed. However, only the theoretical argument I have discussed so far is part of Gross's analysis. On an empirical level, by contrast, Gross cites no evidence which demonstrates the realism of his model of actor.

One must concede that Gross (2009, p. 373) notes in the final section: ''I hold it to be an empirical question whether the theory of mechanisms laid out here will give sociology more explanatory purchase''. He is clearly referring here to the consequences of his theory. However, evaluating the empirical solidity of its premises is also an important empirical matter. In light of the overall spirit of Gross's analysis, this aspect is crucial because he himself doubts that ''false models'' can be useful (Gross 2009, p. 365, notes 8 and 9).

In that connection, he might have considered that a rich body of experimental literature has generated a large amount of empirical information which outlines the basic analytic space within which actors' actions unfold.

In this regard, we should first consider the work accumulated over the past three decades in behavioral economics (Camerer, Loewenstein and Rabin 2004). This experimental research has demonstrated that real actors judge and choose according to principles which are not those postulated by standard rational choice theory: diverse kinds of deviations from rationality occur (for a synthesis, Camerer and Loewenstein 2004, Elster 2007, ch. 12 and 20). This conclusion holds both in the case of isolated actors and of actors embedded in interaction structures – behavioural game theory studies this second aspect more in particular (Camerer 2003).

Although Gross does not draw on this literature, his criticism of the orthodox form of rational choice theory is relevant and empirically validated. This literature, however, does not suggest that habit is the alternative mode in which actors act when they are violating the assumptions of the model of *homo œconomicus*.

On the one hand, although purely selfish behavior occurs less frequently than standard rational choice theory predicts, it regularly arises under certain conditions (Camerer and Fehr 2006, Fehr and

Gintis 2007). On the other hand, experimental studies on the cognitive strategies underlying real actors' judgments and choices have shown that habit is only one of the problem-solving methods used under uncertainty (Pingle and Day 1996; Camerer, Ho and Chong 2003). Cognitive psychological research on ''bounded rationality'' has discovered – and modeled – a very wide range of ''heuristics'' which may account for the shortcuts adopted by real actors in real decision-making processes (see, for a synthesis, Gigerenzer and Selten 2001). Thus, on the basis of this experimental literature, judgment formation, decision-making and actions seem to lie along a continuum whose theoretical extremes are habit on the one hand, and ''pure'' rational behavior on the other.

The empirical foundation for this representation has been provided by research in social cognitive psychology. According to research on dual-process models two modes of judgment-forming and decision-making co-exist when real actors act (see for a summary, Chaiken and Trope 1999, for an application to moral judgments, Greene *et al*. 2008; Greene 2009). Real actors sometimes choose on the basis of automatic and non-reflexive responses (often of an emotional nature), while they sometimes also base their action on a more or less detailed assessment of the situation in which they are acting. Four factors have been reported as being responsible for the activation of one or other of these two operating modes:

1) the extent to which a solution is available to be adopted;
2) the presence of unexpected information or events;
3) motivation (how costly a wrong decision will be);
4) the amount of opportunity to think (the time and cognitive resources available).

Thus, Gross's choice of focusing with such insistence on the habitual side of action does not seem empirically justifiable. We simply do not have at present sufficient empirical reasons for favoring this component of action when conceptualizing social action. As a consequence, since conceptualizing social mechanisms as ''chains or aggregations of actors, problem situations, and habitual responses'' (Gross 2009, p. 369) restricts rather than enriches the scope of DBO theory, Gross's proposal seems unable to substitute for it.[37]

[37] To be noted is that Abbott, who, as I pointed out earlier, attacked AS on similar pragmatist grounds, nevertheless qualifies the potential of that approach: ''The pragmatists gave us the foundations of relational thinking, but however sophisticated their social psychology, their sociology is pretty simple-minded'' (Abbott 2007b, p. 2).

A theory of action more realistic than standard rational choice theory usable for a general conceptualization of social mechanisms – which is Gross's overall intent – should integrate habit, emotion and (cognitive) rationality. Specifying under which conditions one of these logics is more likely to be activated would ultimately provide a general theory of action (DiMaggio 2002).

Two items of evidence can be cited to demonstrate the open nature of DBO theory and the AS project to develop a theory of this kind. The first can be found in the section of *The Handbook of Analytical Sociology* devoted to ego-centered mechanisms. Here, the theory of action at the heart of AS emerges at the intersection of seven chapters, each of them dealing with a specific component and/or sub-component of DBO theory (*i.e.* emotions, beliefs, preferences, opportunities, heuristics and norms). The second element testifying to the open character of DBO theory is the dialogue in which AS engages with theoretical perspectives that, starting from the experimental evidence just discussed, seek to integrate habit and rationality into a unified formal theory of action.[38]

In conclusion, one may note that Gross's choice of exclusively focusing on habit seems inconsistent with the philosophical and sociological tradition to which he explicitly refers. As he (p. 365, 368, 372, note 11) himself admits, pragmatism conceives action as lying at the intersection of habit and creativity (see, on this second dimension, Joas 1997). The emotional component of individual action is also central to the pragmatist tradition deriving from James's thought (Barbalet 2007). Moreover, contemporary social psychologists consider James as one of the pioneers of the dual-process models that I have just mentioned (Moskowitz, Skurnik and Galinsky 1999).

So why did Gross's argument lose this balance? The only justification that he gives is the following one:

[38] The *Model of Frame Selection* (MoFS) created by Esser (2009) and formalized by one of his students (Kroneberg 2005, 2006) starts from four premises: a) some *frames* (''mental models for the pertinent situation'') and *scripts* (''habitualised behaviour programs'') enable actors to define the situation in which they are acting; b) these *frames* and *scripts* are activated in actors' minds on the basis of an evaluation process whose degree of consciousness is variable (''variable rationality'') ; c) the selection mode of *frames* and *scripts* is conceived as lying along a continuum whose two extremes are automatic activation (''as-mode'') and conscious cost-benefit analysis (''rc-mode'') ; d) the selection mode is conceived as an ''unconscious process'' which in turn depends on four factors (opportunity for reflection, motivation to reflect, cost of the reflexion, and strength of the script internalization). Current empirical applications – to the rescue of Jews under national-socialism and voting behaviour in contemporary Germany (Kroneberg, Yaish and Stocké 2009) – are mainly based on this last factor, *i.e.* the strength of the script internalization.

> Focusing on the habit over the creativity end of the action continuum, as I do, allows a pragmatist perspective to be better reconciled with the aspirations of a probabilistically causal social science, and to provide action-theoretical foundations for such a science that neglect neither the interpretive nor the agentic aspects of social experience (p. 374, note 13).

One is more surprised than convinced by this argument. Is it not precisely the reflexive, inventive and creative side of social action which enables us to conceive the social world as a probabilistic, rather than deterministic, reality?

Conclusion

The numerous criticisms recently brought against AS raise the crucial question of its specificity. To address this question, I have sought to assemble historical evidence showing that contemporary AS stems from integration and cross-fertilization between two bundles of older ideas. The first concerns the concept of mechanism and explicitly appeared in the 1950s and 1960s. The second set of ideas relates to the concept of ''analyticity'', whose roots reach back to Parsons's intellectual project. I have then tried to show that such integration – which materialized in the mid-1990s – is now being developed in a specific direction.

Without denying the heterogeneity of the authors involved in AS, I have argued that the specificity of the contemporary AS approach must be sought in the combination of a set of conceptual, epistemological, ontological and methodological elements, rather than in each of them taken individually. I have finally sought to show that the most distinctive feature of such an integrative framework is that it only provides a ''syntax'' for explanation: that is to say, a set of rules on how hypotheses about mechanisms underlying the regularities of social life can be theoretically designed and empirically tested.

This finding has guided my response to the criticisms made of AS to date. Accusing AS of being no more than a particular theoretical orientation (rational choice theory, most often) or, symmetrically, of not taking account of some or other principle (habit, for example) amounts to denying, or failing to recognize, both the intimate meaning and the main objective of the AS research program. In both cases, AS is attributed a limitation that by construction it cannot have. To the extent that AS only provides us with a framework in which to construct and test an explanation, the specific content of each explanation depends on the phenomenon studied, as well as on the empirical evidence that permits exclusion of a particular theoretical option from the outset.

Some of the critics of AS have claimed that it is nothing more than what a scientific sociology should be (Ballarino 2005). This is a statement everyone may fully agree with. However, if several authors have felt it necessary to collect the options which seem at present best able to make such a scientific sociology effective, this may mean that scientific sociology still hesitates to impose itself. Hence, according to a social dynamic described by Merton (1936, pp. 903-904), *i.e.* the so-called ''suicidal prophecies'', AS paradoxically contains the reason for its death. It will disappear as soon as it has succeeded in moving sociology toward what sociology should be if it were a science like any other.

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Résumé

Face aux critiques que la sociologie analytique reçoit avec une fréquence croissante, cet article propose une évaluation globale de ce programme de recherche en posant quatre questions : quelle est l'origine de la sociologie analytique ? quelles sont les différences entre l'« ancienne » et la « nouvelle » sociologie analytique ? ; qu'est-ce que la sociologie analytique contient vraiment ? ; les détracteurs de la sociologie analytique ont-ils vraiment raison d'être mécontents ? L'article récent de Neil Gross est discuté dans le détail pour répondre à ce dernier point.

Mots clés : Sociologie analytique ; Mécanismes ; Théorie DBO ; Modèle "agent-base".

Zusammenfassung

Da die analytische Soziologie immer stärker kritisiert wird, geht dieser Beitrag dem Sachverhalt mit vier Fragen auf den Grund: wie ist die analytische Soziologie entstanden, wie lassen sich die Unterschiede zwischen alter und neuer analytischer Soziologie festmachen, wie sind ihre Inhalte zu definieren, und dürfen die Anfechter wirklich unzufrieden sein? Neil Gross Aufsatz wird detailliert besprochen, wobei der letzten Frage große Aufmerksamkeit geschenkt wird.

Schlagwörter: Analytische Soziologie; Mechanismus; DBO Theorie; Mitarbeitermodell.