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# **<sup>1</sup>MIDDLE-RANGE THEORIES AND THE UNIFICATION PROBLEM IN SOCIAL SCIENCE.**

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In Robert Leroux, Thierry Martin & Stephen Turner, *The Future of Sociology*, London, Routledge, chap. 6, pp. 91-115

## *Abstract*

*This chapter aims at evaluating the relevance, within the current philosophical and sociological contexts, of the search both for “middle-range theories” (as Robert Merton famously called them) and for a unification of these theories. In his time Merton’s view was, on the one hand, a crucial complement to both quantitative and qualitative studies, remarkably illustrated by his colleague at Columbia, Paul Lazarsfeld, and, on the other hand, an alternative to “Grand Theories” such as Talcott Parsons’, whose inconvenience was to be too abstract at the risk of being merely verbal. Many contemporary theories in sociology have similar flaws. They are either purely statistical surveys or mere ethnographic case studies, on the one hand, or all-encompassing theories, on the other hand. But not only have middle-range theories been recently rediscovered in the social sciences, some leading figures in philosophy of science have even argued that biologists and physicists regularly use similar methodologies very fruitfully even though they are not fully aware of doing so. Although these philosophers have sometimes gone so far as to support the value of “disunity” in science, which may result from the co-existence of local or partial “middle-range theories”, I want to make a plea for unifying middle-range theories in sociology which do not fall back in the false abstraction of Grand Theories.<sup>3</sup>*

## Introduction.

In this paper, I want to examine the relevance of Robert Merton’s middle-range theories within the context of the current social sciences, in which mere statistical surveys and ethnographic case studies, on the one hand, and all-encompassing theories, on the other hand, prevail.<sup>4</sup> I want also to address a related issue and highlight it: as middle-range theories are both various and numerous, it is difficult to escape the issue of their compatibility and, furthermore, of their unification – a goal that Merton (1948, 1957, 1967) himself actually did not abandon – if only to avoid participating in the “Balkanization” of sociology. I claim that these two issues can be illuminated to a certain extent by the contemporary philosophy of physics and biology, in particular by the provocative views of the Stanford School of Philosophy (Nancy Cartwright, John Dupré, Ian Hacking, etc.) on the value of *disunity* in science. In the first section of this chapter, I will remind readers of the nature and the role of middle-range theories on taking a few examples in the social sciences and I will briefly explain what they are in other sciences since, according to certain scholars such as Nancy Cartwright, they can be encountered also in biology and even in physics. In the two following sections, I will address the issue of the unity *versus* disunity of sciences, which arises if one highlights the value of only partial and local models (as the middle-range theories are supposed to be), either in science in general (second section) or in the social sciences in

particular (third section). The fourth section will be devoted to investigating James Coleman's *Foundations of Social Theory* as a largely unnoticed yet powerful attempt to systematically unify a broad range of middle-range theories. In the fifth section, I will examine two options other than Coleman's: a much weaker alternative which nearly renounces unification, which will be illustrated by examples borrowed from economics and biology, and an intermediate option exemplified by the *Handbook of Analytical Sociology* edited by Peter Bearman and Peter Hedström, which is representative of a modest but reasonable first step towards a real unification of the social sciences.

### *1. The value of middle-range theories in the sciences.*

Many contemporary sociological studies are either qualitative or quantitative case studies. Most of them, even when technically very sophisticated, as many quantitative studies are nowadays, are much more descriptive than explicative. Paul Lazarsfeld played definitely, after World War II, a major role in the promotion of both quantitative and qualitative methodologies and as a remarkable methodologist.<sup>5</sup> Lazarsfeld simultaneously also worked closely and fruitfully with Robert Merton,<sup>6</sup> who was more famous, as a theoretician (Merton, 1957).<sup>7</sup> The great value of this specific kind of cooperation seems to have been somewhat forgotten in many research programs in the current social sciences. Merton introduced the idea of searching for far less ambitious theories than all-encompassing theories such as Talcott Parsons' (1937, 1951 (which were ironically but legitimately labelled by critics: "Grand Theories"): these "*middle-range* theories" were intermediary between particular case studies and very general and very abstract theories.

Middle-range theories, the most famous of which is surely the theory of the "self-fulfilling prophecy", are not only less ambitious than very general theories, they are also focused on social "mechanisms" – or causal chains of events – and their components (see below for further comments). This aspect has been recently rediscovered in social sciences and developed quite explicitly by Jon Elster (1983, 1993, 1998, 1999a, 1999b) (e.g. "sour grapes", "wishful thinking"), Raymond Boudon (1991, 1998) (e.g. "unexpected effects") – one of Lazarsfeld's former students – and implicitly by many other scholars such as Thomas Schelling (1978) (e.g. "segregation effect"), Mark Granovetter (1978) (e.g. "threshold effect"). The search for mechanisms had been in fact already used a little earlier, without any specific reference to Merton, by many other famous scholars, such as Erving Goffman (1961) (e.g. "total institution"), just to add the name of a scholar coming from an entirely distinct background (though they were alike in their strong opposition to Parsons' theoretical style).<sup>8</sup> The search for middle-range theories is now often also known under the label of "analytical sociology" (Hedström and Bearman, 2009, Hedström, P. & Udehn L, 2009).<sup>9</sup> Scholars using this specific concept try to systematically "dissect" social phenomena (Hedström & Swedberg, 1998a)<sup>10</sup> like anatomists and physiologists (and unlike authors such as Goffman, whose style was often intuitive and impressionist).

Merton was well-known as a leading figure of the sociology of science preceding the radical Kuhnian "turn" (Kuhn, 1970) in this domain. But Merton's main contribution (1957) to social science was rather in the discovery and the rigorous analysis of plenty of social micro-mechanisms and, moreover, in the epistemological reflection on the scientific advantages of orienting research in that direction. Self-fulfilling prophecy is one of these mechanisms. Even if this mechanism is rather well known, it seems relevant to return to its analysis because, as a paradigmatic example, it has been re-investigated in detail by analytical sociologists.<sup>11</sup> The phenomenon of self-fulfilling prophecy consists of a prediction made by somebody that something will happen, and it happens partly *because* it was predicted. The typical example is this of a man in charge of the central bank of a country who would say

unwisely that the banks might be soon not able to give back money to the customers who would ask for their assets. The goal of such a man might be only a descriptive: giving a descriptive account of the state of the economy. But the goal might be more likely as well to give a kind of indirect warning towards the heads of companies, urging them to be more innovative, or towards the government to do more to favor free enterprise. The message might be also just a rumor whose source is unknown. Unfortunately, in both cases, the message might be listened to by ordinary customers who might be afraid of a possible bankruptcy of banks and, as a result, would prefer to withdraw their assets immediately. If many of them, possibly all of them, withdrew their money more or less at the same time, banks will not be able to fulfill their previous financial commitments, which require that large amounts of money are held in reserve; so they will not be able to give back money to the customers. Thus the consequence not only will fit in with what was predicted but will be partially produced by the prediction itself. This phenomenon can be described as a “mechanism” in the sense that it looks like *a chain of almost unavoidable events triggered by an initial event*.<sup>12</sup> There are other classical examples of the same process, such as the placebo response, interpersonal expectancies, reactive conflicts, investment bubbles, and even the Marxist prediction of the proletarian revolution.<sup>13</sup> Self-fulfilling prophecy also reveals other characteristics, on which I will soon focus.

Merton (1967) spoke of “middle-range theories” but one could also speak of “local models” or “micro-models” to match other more usual concepts in philosophy of science (or “meso-models” if one takes into account the fact that there is a hierarchy of middle-range theories, some more abstract than others). A characteristic of middle-range theories and models in general is that, on the one hand, the conceptualization they offer, which aims to explain a sequence of events, is not simply *ad hoc* in the sense that it is not constructed to account for a unique historical phenomenon (a specific bankruptcy) or even a unique kind of phenomenon (any bankruptcy) but for various phenomena and kinds of phenomena (from bankruptcies to interpersonal expectancies to revolutions)<sup>14</sup> and that, on the other hand, it is not as general as a standard or typical theory (such as Newtonian Mechanics) since it applies to a rather restricted range of phenomena.

Merton added that when social theorists use these middle-range theories they are usually not able to *deduce* the model of one specific mechanism from a more general explanatory theory (as it would be the case in a Hempelian representation of science, whose “deductive-nomological model” fits in particularly well with Newtonian Mechanics and, paradigmatically, with Kepler’s laws of planetary motion). Merton admittedly did not really launch a real research program following the path he had designed but instead constructed numerous “middle-range” theories revealing social processes that may be all encountered in various domains. One example is peculiarly interesting as a point of comparison with other famous sociological theories. Based on macro-sociological data, Merton showed that there was a strong statistical correlation between Puritanism and the emergence of science, which suggested that the former may be a cause of the latter and that the underlying process might be asceticism, which can be observed at the micro-sociological level in each case. At this stage, one cannot speak of a middle-range theory yet since one is dealing with an account of only *one* historical phenomenon, the historical emergence of science, notably physics and biology. But Merton’s analysis has continued to command attention because it evoked Weber’s analysis of the emergence of capitalism, which invoked a similar process. In fact, according to Weber, Puritanism also played a major role in the emergence of capitalism and the underlying process was also asceticism. Asceticism was inspired initially by Puritanism but remained as an “*ethos*” favorable both to capitalism (Weber’s historical case) and to science (Merton’s historical case), after the religious motivation was forgotten. In both cases,

the same mechanism played a major causal role: the transformation of a certain behavior produced by religious sanctions into an autonomous *ethos*.

A comparison between Weber and Bourdieu is also instructive here. Bourdieu, unlike many of the contemporary scholars I mentioned earlier, was *not at all* an advocate of middle-range theories – whose relevance is always methodologically supposed to be limited to a certain range of phenomena. Bourdieu always strongly claimed, on the contrary, that the concept of *habitus* had a universal relevance: this concept was expected to explain the global process of the reproduction of any social structure. But Bourdieu's concept of *habitus* is also sometimes mentioned by analytical sociologists as an example of a quite interesting mechanism (among many other mechanisms). The concept of *habitus* may seem at first sight to have the same content as the Weberian concept of *ethos*. However Bourdieu's concept is devoted to what is instead another specific phenomenon, the fact that people participate unconsciously in reproducing the social structure in which they have been educated. What is similar is the unconscious aspect of both *habitus* and *ethos*. Other social phenomena are arguably better understood by supposing that social actors have good reasons of which they are conscious, for example pragmatic reasons or axiological reasons, to act as they did (and it is also what Weber showed most of the time). A more analytical way of proceeding would have required a further distinction between the specific process of *habitus* and a process or mechanism that seems close to it, such as the process or mechanism of the *adaptation of preferences*. This concept was introduced by Elster (1983) under the metaphor of *sour grapes*, a concept that is again the core of still another micro-model or another “middle-range theory”, which can explain the reproduction of social inequalities in a different way than *habitus*, but as well – just to take another example among many others – why despotism may persist for a long time despite the fact that people may initially have opposed it strongly (see also Veyne, 1992).<sup>15</sup>

Within this general context, the most striking and most instructive fact, philosophically speaking, is that the philosopher of physics Nancy Cartwright (2020) recently argued provocatively that the best that has been done in *physics* itself was not the discovery of any law nor the construction of general theories such as Newton's or Einstein's mechanics (as claimed by Carl Hempel, 1965), but of middle-range theories. Cartwright, in this paper, explicitly referred to Robert Merton's and to Jon Elster's analyses as excellent examples of that trend in the social sciences but also mentioned biologists and philosophers of biology (Machamer, Darden and Craver, 2000, Darden, 2007) regarding their own domain. In physics, Cartwright (2020) took the many theories of lasers employed in practical laboratory contexts (Cartwright, 1983), as paradigmatic examples of “middle-range theories” (*sic*), although this specific expression had been so far never used in other domains than sociology. This is how Cartwright opened the conclusion of this important paper: “Middle-range laws, Elster-type mechanisms,<sup>16</sup> maxims and precepts are fine in science.<sup>17</sup> They are more than fine, they are a boon.” And she added: “We do not need to recast them as propositions, we do not need to take them literally, we do not need to try to ‘derive’ our conclusions from them. We do need to learn how to use them. We do need, as a community, to fill our tool-box with a big variety of tools and to learn how to use them together to craft reliable finished products.” (p. 319). Cartwright wants above all to distance herself from Hempelian views on science, where the goal is to “derive” conclusions from more fundamental principles, either deductively (the deductive-nomological model), as in Newtonian and Einsteinian Mechanics or inductively (the inductive-statistical model), as in Quantum Mechanics. Instead, she insists on a pragmatic view of explanation (“how to use [these tools]”) based on the fact that, depending on the goal within a specific context, it may be wiser to use a particular conceptual micro-model, and use another one in a distinct context.

Cartwright's stress on the role of middle-range theories in the natural sciences in her philosophy fits with her valorization of "disunity" in science. However, the disunity of science might not be related only nor even mainly to middle-range theories. In fact, Cartwright supports the more stimulating idea of a possible "dappled" constitution of the world. This idea obviously needs further comment. The theoretical background of the social sciences, in any case, is far less refined than that of natural science: no theory is comparable in sociology with Newton's nor Einstein's theory, and for that reason alone, more abstract and more general theories might still be needed in the social sciences. Thus it is worth exploring the question of the kind of unity that can be reached on the basis of middle-range theories. This what I will do in the fourth and fifth sections. But I want first to address the issue of unity and disunity in more detail both with respect to science in general and to the social sciences in particular.

## 2. *The unification problem in sciences: should we really value unity or rather disunity?*

The unification problem is, of course, a problem that emerges in every scientific domain and not only in social science. A brief survey of the different options at the level of the general philosophy of science will be useful as a background for the specific problem in social science.

In the beginning and in the middle of the 20th century, the unification of the various parts of physics was considered as one of the main problems both in physics and in the philosophy of physics. In physics, one of the major challenges was the issue of the links between quantum mechanics and classical physics, more precisely the possible complementarity of their principles or the possible correspondence of their concepts. The Volta conference in Como in 1927, where Bohr as a spokesman of the Copenhagen School challenged Schrödinger's view, the Solvay Congress in Brussels a few months later where Bohr and Einstein struggled on the issue of indeterminism were important steps in this debate. Heisenberg, a leading figure of the Copenhagen School, was already much less worried by the unification of physics than, notably, Schrödinger or Einstein (Beller, 1999). In the philosophy of physics, the issue was raised at a much more general and vaguer level in the Manifesto of the Vienna Circle. This movement promoted the idea of the unification of all sciences, from physics to sociology, which culminated in the project of the *Encyclopedia of Unified Science*, in which Neurath (1938) wrote the entry on sociology. Unlike the physicists, these philosophers, even those trained in physics, such as Carnap or Reichenbach, were all oriented towards the value of theoretical unification, in contrast to Heisenberg or Bohr, for whom the the empirical predictive power of the theories and the discovery and understanding of new phenomena were more important.<sup>18</sup>

Unification remains a goal for many physicists and for many philosophers as well.<sup>19</sup> But the idea that the world might be too much varied and complex for a unified theory to be found, or so fundamentally "dappled" (Cartwright, 1999) that the project might simply be irrelevant, has spread into the scientific and philosophical community. This idea has been elaborated in many ways. Twenty five years ago or so, Peter Galison and David Stump edited a collective volume entitled *The Disunity of Science* (Galison, P. and Stump, D.J. (eds), 1996), which served as the manifesto of the so-called Stanford School.<sup>20</sup> One of the main ideas of this book was based on the fact that science progresses along various paths which are not necessarily closely connected. For example, there are not strong logical links between particle physics and condensed matter physics or between molecular biology and organismic biology, domains that recently appeared.<sup>21</sup> Furthermore, the progress of instruments often has its own pace. In many ways, Hacking's work, both epistemological and historical (in particular

Hacking, 1983, 1996), is an expression of a similar idea. Hacking also contributed a chapter to this volume, insisting on the multiple senses of the concept of unity itself. But it is probably Nancy Cartwright (1999) who gave the most impressive and systematic formulation of this view, perhaps because she was trained in quantum mechanics, where pragmatic or heuristic aspects tend to be dominant, and also because she experienced how unenlightening general Quantum Mechanics was for the design of lasers (Cartwright, 1983). In fact, a close examination of sciences, notably physical science and economics shows that there are certain parts of the world that are known and many others that are not, even though they may seem close to each other: it as if the world of scientific knowledge was more comparable to an archipelago than a continent. Cartwright insists so much on this factual aspect of the absence of unity of theoretical physics and that scientific knowledge nevertheless progresses that she often seems to have renounced the ideal of unification itself. Furthermore, she chooses to give priority to a somewhat fascinating ontological interpretation of this situation, since she argues that this is the *world* that might be intrinsically “dappled” and not only that the science is “disunited”.

This suggests three conclusions. First, the fact that one cannot logically eliminate the possibility that the world itself is dappled – and “untidy” as Cartwright also likes to say – does not mean that the world is *necessarily* dappled and untidy. An epistemic interpretation rather than an ontological interpretation, the more traditional idea that it might be only science that is not unified, which is a more moderate reading of the same situation, is also defensible.<sup>22</sup> Second, the acknowledgment that the ideal of theoretical unification ought not to prevail on the ideal of increasing empirical knowledge, which seems to me the *main* concern, should not lead us to necessarily *abandon* the ideal of unification, just not to give it as much importance and not to seek its realization prematurely. Third, that the requirement of unification is encountered not only at the very general level of the overall unification of physics (without speaking of the still more general level of the Vienna Circle ideal of unification of *all* science), but in every specific science. As already noted, Cartwright (1983, 1999, 2020) often takes the example of theories of lasers, which are especially numerous and where each is relevant depending on the specific issue at stake.

Cartwright’s comments on the notion of middle-range theories are nevertheless not sufficiently elaborated, to my knowledge, for reader to really grasp their relationship with the claim of the disunity of science. In particular, while Cartwright, on the one hand, insists on the value of searching for micro-models instead of general theories and on the “mechanistic” features of the phenomena that are grasped thanks to these micro-models, she does not focus on the fact that middle-range theories, in Merton’s or Elster’s sense, account for phenomena from quite *distinct* and *distant* domains. According to Cartwright, it seems that the middle-range theories on which she comments are not only micro-models or meso-models that cannot be deduced from general theories (at least currently) but also *local* models, that is, models relevant only to local domains.

### 3. *The unification problem in the social sciences*

In the philosophy of the social sciences, one can also observe an oscillation between the two poles (unity / disunity) comparable to what has been seen in the philosophy of the natural sciences. Jonathan Turner (1991) even formulated the following objection, three decades ago, against middle-range theories: “Merton’s middle-range theories encouraged the proliferation of [...] empirical generalizations [that] have been defended as theory, despite the fact that they are not sufficiently abstract” (p. 81). Jonathan Turner added: “ Thus sociology has spawned a large number of theories about such specific empirical processes as juvenile delinquency, family conflict, race relations, social mobility [...], urbanization, and other

empirical events” (p. 81). Turner seems to think that middle-range theories are specific to *local* domains. This is surely not the case, on the basis of an example as paradigmatic as the model of self-fulfilling prophecy. And we will soon give other examples (in the fourth and fifth sections). But Cartwright’s understanding of middle-range theories may be representative of a more widely held view to which criticisms similar to Turner’s would apply.

There is no doubt that, as Jonathan Turner suggests, there is a fragmentation (or a “Balkanization”) of sociology into almost autonomous various sub-domains and little doubt that we should consider this problematic situation (Boudon, 1980, Turner and Turner, 1990) and, as a consequence, also consider the question of the relevant degree of abstraction (Bouvier, 2008). Finally: what should we do in the social sciences? Should we search for unity even if we value middle-range theories far more than Jonathan Turner or finally rather renounce the unification of social science as Cartwright seems to recommend, not only for physics but for any science?

The discussion in the social sciences has taken a different form from the one in the natural sciences, not surprisingly, given not only the different nature of these sciences but also the different degree of development – and abstraction – of these disciplines. Unlike the natural sciences, most parts of the social sciences rest on common sense concrete intuitions (that people often act according to their self-interest, but sometimes they feel sympathy for others, they often try to find the best means adapted to their ends, but sometimes they act without thinking thoroughly, etc.), so that the lack of unity and the resultant disagreements in these sciences, however strong they often seem to be, are not really based on conflict over *substantial* reasons as is the case in the natural sciences, where two paradigms often differ over the nature of “unobservable” entities or processes.

For example, there is nothing in social science comparable to the conflicts in natural science on the nature of electrons – are they particles or waves? – which was part of the debate between specialists of quantum mechanics at the beginning of the 20<sup>th</sup> century, or on the structure of DNA before the discovery, in 1953, by Francis Crick and James D. Watson of its double helix shape. Of course, there must be a correspondence between what social scientists call “self-interest” and certain neuronal processes as well as between what they call “sympathy” (Smith, 2002, Sen, 1977, 1987, 2005, Coleman, 1990) and other neuronal processes on which the former ones “supervene”, such that here one may encounter the same kind of problems regarding the nature of these neuronal processes that one encounters in physics or in biology. Indeed, in certain cases, it is probably relevant to go back directly to such neuronal processes directly, without considering what occurs at the conscious level to explain social phenomena, for example in cases involving addictive behaviors (see, on neuro-economics, Bickel & Yi, 2010) ; or, it may be relevant to at least look for a transitory level of explanation, identifying a plausible unconscious mechanism, such as the so-called “reduction of cognitive dissonance” process without specifying its – unknown – neuronal basis (Festinger, 1953, Heider, 1958, Kiesler, 1971, etc.).<sup>23</sup>

But usually, the relevant level of explanation in social science is at the conscious – or accessible to consciousness – level (Elster, 1999b, Bouvier, 2018a). Thus when the issue of the unity is at stake, the relevant debates are on the content of the concept of self-interest, the content of the concept of (conscious) rationality, on the issue of knowing whether it makes sense to introduce other psychological mechanisms such as, for example, the concept of sympathy or of identification (Coleman, 1990, Hardin, 1995), the concept of trust (Coleman, 1990, Gambetta, 2000, Hardin, 2000, 2002, Cook, 2009), the already mentioned concept of commitment (Heider, 1958, Kiesler, C.A., 1971, Sen, 1977), the concept of self-commitment or the concept of self-binding (Elster, 1977), as well as the concepts of *ethos* (Weber, 2010), *habitus* (Bourdieu, 1971, 1984, 1990a, 1990b) or adaption of preferences (Elster, 1993). The introduction of the concepts of *habitus* and *ethos* would mean the additional recognition that



some previously conscious states of mind leading to conscious choices have been so much integrated or “incorporated” (Bourdieu, 1990a, 1990b) that they have a hidden although determinant influence on behaviors: they are no longer conscious but they were conscious previously (Weber (2010), Bourdieu (1990a, 1990b) and the processes may still be accessible to consciousness.

These discussions, however, occur at the level of theory. But, as I have previously said, most investigations in sociology narrowly understood (the same could be said of ethnology) have been for decades so case-oriented that the concern of finding more general explanations has been almost just ignored (For a more detailed diagnosis, see Boudon, 1980, 1981; Turner, 1986, Turner & Turner, 1990, Elster, 2007). And even when these higher level theoretical concerns were present, attempts at unifying the social sciences, until recently, arose under the format of vast syntheses. These usually aimed at keeping the alleged best *intuitions* of the previous global (and themselves often weakly analytical) theories set up by the founders (such as Marx, Weber, Durkheim, Pareto, etc.). Talcott Parsons (1937) is typically representative of this kind of attempt, but the same projects have reappeared later, around four decades ago, often varying depending on countries. Habermas (1984 [1981], 1987 [1981]) is still currently the most representative in the world, but Niklas Luhman (1995 [1984]) in Germany, Anthony Giddens (1984) in the UK, Jeffrey Alexander (1982-4) in the US, to quote just a few other examples, have been also characteristic of this still strong trend in the social sciences. Even if Bourdieu (1977, 1990), as already mentioned, and Foucault (1977) did not provide a general theory, they are often considered as having themselves outlined such a theory because of their tendency to try to understand social phenomena through very few lenses supposed to have a universal scope (cf. Hedström, P. & Swedberg, R. (dir.), (1998)).

#### 4. *Coleman’s attempt to unify middle-range theories in the social sciences: a strong option.*

In analytical sociology (Hedström, P. & Swedberg, R. (eds), (1998), even among those sociologists who were convinced by the irrelevance of too vast abstract or too intuitive syntheses and the reciprocal relevance of middle-range theories, the aim of constructing a unified theory has not entirely vanished. James Coleman, a former student of both Lazarfeld and Merton, is particularly representative of this pole of theorization in the social sciences. Coleman’s approach can be characterized as the strongest option so far among the various attempts to solve the problem of the unification of the middle-range theories (and therefore possibly the unification problem in social science, generally speaking). Yet although it has been widely discussed, his *Foundations of Social Theory* has often been strongly rejected by the sociological community as an impressive but much too reductionist application of Rational Action Theory to sociology (see Clark, 1996, Tilly, 1997, Elster, 2003). This is not surprising, although the most typical and significant characteristics of this work might be, with some hindsight and by comparison with the numerous other theories in sociology, that it is very *analytical* in the sense that all the most important concepts are carefully defined, and second that it is very *systematic* (Bouvier, 2003). In this respect, the fact that the principles of this system are borrowed from RAT turns out to be secondary. Nevertheless, most of the middle-range oriented social scientists themselves (see especially Swedberg, R., 1996; Van der Berg, A., 1998) have been skeptical toward *Foundations*, for reasons which have not been fully clarified. The more or less implicit reason might be that such a theoretical construction sacrifices *heuristics* to an abstract and to some extent verbal unity, a little like Parsons’

theory, that is to say the theory that has been typically criticized by Robert Merton and his followers (Favell, 1996). These issues deserve consideration.

Unlike Habermas and Bourdieu (or Foucault), Coleman tried to base social theory on a list of elementary concepts as explicitly as possible and to reduce as much as possible the other concepts to these same ones, with an ideal of parsimony (Bouvier, 2003). Unlike Parsons, he did not take his departure from abstract collective entities (such as systems or sub-systems) whose contours are difficult to draw and the existence is dubious, but on individual actors, whose existence is not dubious and whose features are much easier to define, and on elementary relationships between individual actors. It can be useful to enter here into certain details because nowadays usually the very first pages of Coleman's *Foundations*, on the micro-macro methodology (chapter 1, pp. 1-23) are referred to. In the *Foundations*, the relation of interest is one of these primitive concepts as is the relation of control. Both are closely connected in the analysis of exchange, which requires that someone is interested in something that is under the control of someone else (chapter 2, pp. 27-44). The notion of exchange itself is not primitive but is drawn from the more primitive notion of transfer and characterized as a bilateral transfer. The notion of a primitive right as the pure reciprocal recognition of a state of nature plays a role too (chapter 3, pp. 45-64). Another, more implicit, primitive notion is that people choose rationally in the sense that they always seek the means adapted to their goals and, more narrowly, within a few contexts, the "maximization of their utility"<sup>24</sup> (and this is what most readers have initially retained). One of the challenges of this theory is to examine to what extent the notions of authority (chapter 4, pp. 65-90) and trust (chapter 5, pp. 91-118) are reducible to the previous notions, given first that both trust and deference to authority may violate the norm of rationality and second that these two notions are the core notions of important local theories that seem to have few relationships between them, for example the economic theory of market, the theory of organization and the theory of revolution.

One of the best examples of an attempt to identify elementary social mechanisms in social phenomena in the *Foundations* is provided in chapter 18 ("Revoking Authority"), which is entirely devoted to a presentation and an evaluation of a large set of theories of revolution (pp. 466-502). Coleman wants to refute these theories and to show that a Rational Choice Theory of revolutions better explains revolutions. And again, this is what struck the first readers. But the examination of these theories themselves is probably much more interesting. In fact, in this chapter, Coleman explains the so-called "frustration theories" (p. 472-9) and the "power theories" (p.479-483), identifies their elementary components and tries to evaluate their respective scope. I will focus here only on the frustration theories. The general issue that these theories (a subset out of the theories of revolutions) tackle is this one: "How can frustration increase when conditions improve?" (p. 473). This question was first raised by Alexis de Tocqueville (1955)[1856] who noticed that the French revolution occurred not when the economics and political conditions were at their worst, that is to say in 1787, but two years later when these conditions were, on the contrary, improving.<sup>25</sup> Coleman compares four distinct frustration theories : the theory of rising expectations, the theory of short-term setbacks, the theory of relative deprivation and finally the theory of status inconsistency (pp. 473-478) and concludes: " These theories have certain elements in common [...] [They] differ only [...] in their hypotheses [...]. These hypotheses are sufficiently distinct and precise that empirical evidence could be used to distinguish among them" and he adds: "It is tenable, of course, that all of them are true [...]. It seems more likely, however, that careful examination would disconfirm some of these hypotheses" (p.478). Coleman might be partly wrong here: all these theories might be really true, but under certain specifications and depending on the particular situations at issue.

But what is still more remarkable in this chapter (the same could be said about other chapters), from my viewpoint, even if Coleman does not highlight this point, is that Coleman has identified in his analysis these theories a micro-mechanism that can be encountered in *many other local domains* than the theory of revolution. Thus the *theory of rising expectations* (Birnton, 1965) does not only explain revolutions, but also suicides, as Emile Durkheim (1897) brilliantly demonstrated. Similarly, the *theory of relative deprivation* (Runciman, 1966) is not only a theory that could explain the origin of revolutions (p. 475), but also the relative frustration of officers in the Air Force, where promotion was more rapid than in the military police (Stouffer and al. 1949). Thus, the same micro-mechanism could be working in apparently entirely distinct and distant local domains. Therefore one can easily argue that the theories of these micro-mechanisms are excellent examples of middle-range theories, even if most of these authors never referred to Merton's term. Furthermore, Coleman shows that all these theories are theories that deal with a set of mechanisms consisting of "revoking authority", and this is not a trivial matter, since other theories, on the contrary, involve relation of power (instead of authority), which is quite distinct. Someone may have institutional power but no authority if his/her power is not viewed as legitimate by subordinates, and reciprocally someone can have authority (in case of charismatic authority) and nevertheless not any institutional power. Furthermore authority is one of the relationships that have been established as elementary in the first chapters of the book; therefore the theoretical construction sounds very coherent.

Of course, Coleman's analyses are not original in every part and certain local attempts of reduction were already well known, such as the analysis of exchange as involving a transfer of control, which could be found in Herbert Simon's work (Simon, 1996). Sometimes comparable local attempts also are more elaborated than Coleman's, such as the attempt at reducing trust to self-interest suggested by Russell Hardin (1995). Besides, to take another example, one of the questions at stake regarding the reducibility of trust to self-interest and rational choice was already partially but clearly raised, even though it was under another respect, by Thomas Reid (1997) in the 18<sup>th</sup> century. This analysis has been recently rediscovered within the context of social epistemology (Goldman, 1999): is trust always based only on rational expectation or is not there a more primitive kind of trust, which is necessary, for example, to the transmission of knowledge? Reid took the empirical example of the spontaneous trust that children have towards everyone. He was focused only on what is now called "epistemic trust" (Goldman, 1999, Hardin, 2003), but the question is relevant for other forms of trust. Thus, social bonds such as friendship and love may be based on something else than a kind of trust that is *not* reducible to self-interest but involves an element of generosity, therefore a not calculated risk? Coleman did not leave a space for such a possible autonomous source of trust, which violates the norm of rational choice as previously defined, but he did leave a room for authority relations, recognizing the probable existence of a kind of deference to authority whose source is *not* self-interest, which is necessary to explain charismatic authority. Moreover, one can go still further in the analysis of trust and, in particular, in its relationship to commitment, a concept which I already mentioned earlier and which Coleman did not mention at all.

Coleman's attempt to unify sociology is not entirely convincing in many other ways. Thus, when Coleman tries to explain panics as a case of transfer of control or a rational transfer of "authority", it is hard to find what the heuristic advantage of such a reduction is. Coleman does not want only to claim that the same overall theory, based on concepts such as self-interest, control, rational choice, transfer of control, and Right can explain markets and organizations, but he also wants to prove that it can also explain such apparently irrational behaviors as panics. First, Coleman argues that what is irrational at the *collective* level, such as running to the unique exit of a building if a fire suddenly breaks out, may be rational at the

*individual* level. In fact, even if everyone believes that the optimal collective solution would be that everyone should leave the building in line and in one's turn, everyone may also know that some individuals may prefer to ensure their own safety without considering the collective good and that they will prefer to run, even though, on the other hand, everyone knows as well that it is not entirely sure that people will act that way. In this case, the best solution for everyone is to observe one's neighbor and to do what he/she does: if they does not run, it remains better not to run; but if they run, it becomes better to run immediately as well, hoping to reach the exit before a bottleneck emerges. This rational reconstruction is not implausible.<sup>26</sup> But Coleman goes further and, in a second step, interprets this kind of behavior as an example of a rational transfer of control of one's actions by oneself to "control" by neighbors. It is hard to accept that the reduction to the notion of transfer of control adds something new in the understanding of the phenomenon; the most abstract and general explanation seems rather more obscure than the more specific one.

##### 5. *Alternative views to Coleman's on the problem of unifying middle-range social theories.*

This said, Coleman's view on unification can be seen as a strong option – and I would say: an overly strong option – for the resolution of the problem of the unification of middle-range theories, resulting in some respects in a rather verbal theory, like Parsons' theory. Besides, Coleman is too obsessed by what seems as an a priori defense of the Rational Choice Theory, that is to say even when empirical data could also support alternative theories. Happily, other options than Coleman's exist. I will investigate two of them, both weaker than Coleman's. A first one, which I will call "*the weak option*", consists in merely and simply, at least virtually, abandoning the project of building a general theory (similar to Newton's Mechanics or Quantum Mechanics), which results in verbal formulations that are too often empirically irrelevant, and accepting the irreducible plurality of far less abstract theories (as some philosophers of physics have done for their own domain) without necessarily assuming that the world is intrinsically (or "ontologically") dappled and "untidy" (as Cartwright, 1999, suggested), but based simply on the consideration that our knowledge of the world is too limited to do otherwise (as Kellert, St.H., Longino, H.E, & Waters, C.K., 2006, argued). However, this option has the disadvantage that it leads to treating micro-models as local models, that is to say models proper to local domains even when these micro-models or meso-models are applicable to many local domains. To avoid this result, what would be really required in the social sciences would be at least to carefully clarify the distinct uses of the same concepts (or to distinguish between distinct concepts labelled by the same words) far more than it is currently the case in sociology. For that latter reason I will not borrow examples from sociology but from economics, where conceptual analysis is much more widespread than in sociology, and from biology, which has for a while played the role of a leading domain in the contemporary philosophy of science regarding the examination of "mechanisms" as regular causal chains of events.

A second option, which I call the "*intermediate option*", would consist in keeping the construction of a general theory as a goal, possibly either by refining and enriching Coleman's theory while keeping its elementary components as the core of this novel theory or by taking quite other elementary building blocks than Coleman's, likely too much focused on exchange relationships. In this second option, it is also assumed that it is wiser not to try to directly construct a *too* general theory but to at least *temporarily* accept a lower level of abstraction, and as a consequence, admit the existence of weakly related partial theories in sociology. This would mean, of course, that the balkanization of sociology would not be entirely avoided, but only reduced. Unlike the previous option, this one carefully distinguishes micro-models from

mere local models on insisting, on the contrary, on their transversal relevance. I will take the *Handbook of analytical sociology* (2014) as an illustration of this second option.

α) The weak option.

This option consists on simply *lowering the degree of requirement regarding conceptual unity*, by arguing that the clarity of concepts in every *local* issue is a sufficient requirement, provided that heuristic in every specific domain is secured. This option is compatible with the Stanford school stance (ontological pluralism) as well as with the Minnesota school stance (epistemological pluralism). I will illustrate this option by an example borrowed from a debate on the relevance of the concept of commitment in economics and in social sciences and a second one in genetics; this second example will permit us to go a little further in the analysis of the relationship between middle-range theories and purely local *versus* models which allows for the application of heuristic structures to different local domains, which I will call the transversal model of unification.

Amartya Sen's position, as it emerged from an important workshop on Rationality and Commitment that was held at the University of St Gallen (Switzerland) in May 2004 on the concept of *commitment* (Sen, 2005) is close to this kind of pluralist position. Sen wanted to introduce the concept of commitment in economics, an innovation that was much debated. Sen (1977) had previously argued, in a famous lecture especially influential in normative economics and expanded in many later publications (Sen, 1987), that for explaining a few strictly economic behaviors (such as the decreasing rate of oranges in the seventies), and not especially or not only for normative reasons, it was necessary to introduce the idea that people do not act always according to their material self-interest (as Hobbes assumed) or because of "sympathy" for relatives or "identification" to the self-interest of these relatives (as Adam Smith supposed), that is to say the "other-interest", but because they have a sense of "duty" (in Kant's sense) or of "commitment" to certain altruistic values (thus in the cases mentioned above, lots of people in Europe boycotted oranges and avocados because at that time, most oranges were cultivated in Spain, when Franco was ruling as a dictator and most avocados in South Africa, where apartheid prevailed).

The soundness of introducing the concept of commitment in economics and, more generally, in RCT was especially at issue in this workshop (Hausman, 2005 Pettit, 2005). It was argued, in particular, that the concept of preference is broad enough to integrate the various motivations that can urge economic actors to act, were they either self-interested in the narrow (material) sense of the term or self-interested in the "egotist" sense of the term (which can pave the way for altruism).<sup>27</sup> But other discussants noticed, to a certain extent more trivially but quite reasonably, that the concept of commitment had already been used in quite distinct meanings in the social sciences. Sen's response to this latter objection was that it is not necessary at all to have an overarching (and transversal) concept of commitment and that clarity in each local domain is sufficient. This response is rather frustrating and it is especially so because some of the examples of commitments chosen by Sen (1977) to illustrate his concept could also be interpreted as illustrations of commitments in other uses of the same term (e.g. Gilbert's use; see Bouvier, 2016, 2018a, 2018b). This suggests either that the two concepts are sometimes but not always used with exactly the same empirical content or that a single phenomenon may involve too distinct micro-mechanisms. However Sen's general response to objections that the concept of commitment has many other contents, depending on domains, as frustrating as it is, seems more satisfying than arguing in favor of a general concept of commitment of which the unity would be only verbal.

The weak option can be still enlightened by recent philosophical debates about the use of the concept of "gene" in contemporary biology. Genetics is currently a typical example of a

domain on which various mechanisms (such as the replication of AND) in the specific meaning of “almost unavoidable causal chains triggered by a certain event” (see above) have been discovered (Machamer, P., Darden, L. and Craver, C.F., 2000).<sup>28</sup> However, specialization in this domain has reached such a point that, as Jean Gayon (2007) argued, that in terms of continuity with previous historical investigations (Burian 1985), the concept of gene has now acquired such diverse contents depending on sub-domains in which it is applied that it does not even make sense to try to define it at the most general level. Furthermore, the concept of gene does not play an important role anymore in these particular sub-domains (unlike the concept of commitment in the current social sciences): it has been usually replaced by more technical concepts. However, Gayon quite interestingly noticed that the *word* “gene” is still useful to permit communication between scholars working in these distinct domains because, on the one hand, the ambiguity of the word is acknowledged by anyone and, on the other hand, the use of this rather vague word nevertheless gives a minimal and sufficient piece of information about the global domain that is a stake in a general debate among biologists. In other words, everyone is aware that there is a “family resemblance” between the various uses of the word. In this case, the fact that the unity of the concept is verbal to a certain extent is explicitly assumed but its use is preserved for pragmatic reasons. Of course other concepts than the concept of commitment, such as the concept of self-interest, should also be investigated. But the ambiguity of the concept of self-interest is acknowledged by everyone, so that its use to facilitate the communication is not acknowledged, unlike the case of the concept of gene. Unfortunately conceptual analysis is currently still hugely lacking in sociology, so these distinctions are not observed.

b) The intermediate option.

This second option consists in aiming at a general and unified theory, but accepting that there are still many steps to take before reaching this general theory. One must be aware that, actually, “middle” is used by Merton and his followers in a very vague sense: what is really “in the middle” between particular facts and general theory? There are obviously *multiple degrees of generality* in relation to the number of phenomena that can be understood under the same model, and reaching one more level of abstraction constitutes one more step towards a general theory. For that reason, it is not ironical to say that between rather weakly abstract middle-range theories and a very general theory there is space for many *intermediate* middle-range theories; therefore one has to think of a *hierarchy* of middle-range theories from the most particular ones to the most general (Bouvier, 2008).

Furthermore, the co-existence of a plurality of theories similar to Coleman’s theory and just as strong, but with distinct premises is not excluded at all. I will not investigate this issue here in more detail because it would be nowadays a purely hypothetical possibility since there is not any theory similar to Coleman’s currently. It is sufficient to compare the building blocks of Coleman’s *Foundations* and those of the collective book I will now consider to grasp that still other elementary notions would be relevant as well as alternative building blocks.

The recent *Oxford Handbook of Analytical Sociology*<sup>29</sup> edited by Peter Hedström and Peter Bearman (2009a) could be seen as a serious attempt in the direction of more generality rather than the mere juxtaposition of coexisting middle-range theories without attempting to offer another general theory competing with Coleman’s. While the elementary notions in Coleman’s *Foundations* are “actors and resources, interest and control” (chapter 2), the *Handbook* distinguishes between social statics (Part II) and social dynamics (Part III) and starts the investigation of “statics” with the concepts of “emotions” (chapter 3), “beliefs” (chapter 4), “preferences” (chapter 5) and “opportunities” (chapter 6) – a starting point that is

nowadays rather common in social theories of action. Self-fulfilling prophecies (Biggs, 2009), threshold effects (Watts & Dodds, 2009), and segregation effects (Bruch & Mare, 2009) constitute some of the few mechanisms that are analyzed (in the part devoted to “social dynamics”) in more or less explicit conceptual relationships with emotions, beliefs, preferences and opportunities. There is still no systematicity in this handbook, but it is obvious that this could be rather easily improved. Furthermore, it is clear that the level of abstraction is not the same in every chapter: thus, the alleged mechanisms are sometimes rather *sets* of mechanisms, like “segregation dynamics” (Bruch & Mare). Besides, when some sets of mechanisms such as the mechanisms of diffusion are investigated, they are sometimes illustrated rather arbitrarily (by the diffusion of divorce, Aberg, 2009, and by the diffusion of tastes Salganik & Watts, 2009). There is no chapter devoted to the relations of coordination and cooperation, although the distinction between them has proved to be relevant in many domains (e.g. in economics and in the international relationships) while there is a chapter devoted to the relations of conflict (Kalyvas, 2009). Similarly a chapter is devoted to trust (Cook, 2009) but no chapter on authority.

A few investigations presented in this handbook are good examples of a search for a higher level of abstraction (and therefore of one more step at least towards unity) than is usually the case in the construction of middle-range theories. I will take two related examples. Within this framework of a balanced solution between, on the one hand, too high abstraction and weak heuristic value and, on the other hand, strong heuristic value but weak level of abstraction and, as a result, multiplicity of isolated local models (see the weak option, above), Michael Biggs (2009) has tried to reduce certain aspects of self-fulfilling prophecy, if not the full process, to more primitive phenomena, actually psychological phenomena, especially the *belief* that the number of people who perform an act is probably a sign of the value of the act, a component that was not much highlighted by Robert Merton.<sup>30</sup> Biggs also insists on two empirical facts necessary for the process to occur: first, that the diffusion is *progressive*, second, what Granovetter called the *threshold effect*, that is to say that a sufficient number of other people share the belief,. Finally, without entering into all the details, the self-fulfilling prophecy could be described as composed of simple elements such as: erroneous beliefs, epistemic trust, threshold effect, informational cascade and unexpected effect. This list is non-exhaustive but is representative of an attempt to decompose a social phenomenon in its elementary components (in a way distinct from Coleman’s).

Let us focus on one of these components, the threshold effect. This mechanism can be observed, of course, in many other different social phenomena and the analysis of the threshold effect itself is another example of a middle-range theory of a little higher level of abstraction than the self-fulfilling theory. Mark Granovetter wanted to account for very common processes such the following ones. One can observe that the number of people in a restaurant or of people waiting outside an ice-cream parlor influences the behavior that other virtual customers will adopt; if there are few people in a restaurant, few others are to be expected, except usual customers, that is those who already know the quality of the restaurant. On the contrary, if there are a lot of people, there will be a tendency for people to join them, and paradoxically do so above all if there is a line. One explanation (which actually Granovetter himself did not suggest), is that it is probably a case of epistemic authority, which may be wrongly motivated, of course. People who do not know the restaurant or the ice-cream shop think that if there are many people, it is probably because the food or the ice-cream is good and that this knowledge is already wide-spread. But there is a threshold, which furthermore seems to vary according to the individual: before a certain amount of customers is reached in a restaurant or in front of an ice-cream shop, there is no acceleration of the amplification of the number of customers. Similarly, when people want to cross a busy street outside pedestrian walkways, most of them prefer not to cross the street alone; they will wait

for others to do the same. And suddenly, after a certain number of individuals is reached – this is the threshold effect – they all try to cross the street together, yet without any deliberation. What was initially a local model regarding trivial events can be applied to many others and much more significant phenomena, such as the emergence of a revolution. In this case also, for people to revoke authority, a threshold has to be reached. The initially local model turns out again to be a transversal model.

### Conclusion.

To briefly conclude, I would like to recall that the issue is to ask, on the one hand, whether it is still worth constructing a general theory, however elegant it is, if it doesn't permit us to make discoveries as well as a less abstract and less logically simple theory and, on the other hand, whether one can content oneself with a multiplicity of local models. Although one can be satisfied, for pragmatic reasons, with the elimination of ambiguities between different uses of similar concepts depending on domains, one can probably also go to a higher level of abstraction than self-fulfilling prophecy, the paradigmatic example of a middle-range theory, not only without any loss of explanation power but, on the contrary, obtaining a more unified view of the underlying micro-processes. The notion of threshold effect and the notion of epistemic authority might be candidates for these more abstract middle-range theories (or models).

In any case the modest search for analytical middle-range theories, that is to say transversal micro-model or meso-models of social mechanisms, possibly at several levels of abstraction, appears to be a reasonable way to compensate some of the shortcomings of current sociological theories. One has to add that these models can be encountered not only in sociology institutionally speaking, but also in other domains such as economics, social psychology or philosophy, as the example of the concept of commitment may have illustrated. Moreover, comparisons with the natural sciences, especially with biology, domains on which reflection has been profoundly renewed by the contemporary philosophy of science, might provide the scholar with a deeper understanding of what should be searched for to better understand the social world, as “untidy” it may be.

### References

- Aberg, Y. (2009), “The Contagiousness of Divorce”, in Hedström & Bearman (2009), chap. 15, pp. 342-364.
- Alexander, J., (1982-4), *Theoretical Logic in Sociology* (vol.1-4), London, Routledge.
- Barbera, F., (2004), *Meccanismi Sociali. Elementi di Sociologica Analitica*, Bologna, Il Mulino.
- Beller, M. (1999), *Quantum Dialogue. The Making of a Revolution*, Chicago: The University of Chicago Press.
- Bickel, W.K & Yi, R. (2010), “Neuroeconomics of Addiction. The Contribution of Executive Dysfunction”, Ross, D., Kincaid, H., Spurrett, D., & Collins, D. (eds), *What is addiction?* p.1-25), Cambridge, MIT Press.
- Biggs, M. (2009), “Self-Fulfilling Prophecies” in Hedström and Bearman (eds) (2009), Chap. 13, pp. 294-314.
- Boudon, R., (1980) [1971], *The Crisis of Sociology. Problems of Sociological Epistemology*, Palgrave Macmillan
- Boudon, R., (1981) [1979], *The Logic of Social Action. An Introduction to Sociological Analysis*, Routledge & Kegan Paul in London, Boston.



- Boudon, R. & Bourricaud, F. (1989) [1982], *A Critical Dictionary of Sociology*, Chicago, University of Chicago Press.
- Boudon, R., (1991), “What Middle-Range Theories Are”, in *Contemporary Sociology*, 20, pp. 519-522.
- Boudon, R., (1993) “Introduction” in Paul Lazarsfeld, *On Social Research and its Language*. Chicago, Chicago University Press.
- Boudon, R., (1998), “Social Mechanisms without Black Boxes”. Hedström, P. et Swedberg, R. (eds), 1998, pp. 172-203.
- Bourdieu, P., (1977) [1972] *Outline of a Theory of Practice*, Cambridge, Cambridge University Press.
- Bourdieu, P. (1990a) [1980], *The Logic of Practice*, London, Polity Press.
- Bourdieu, P., (1990b) [1987], *In Other Words: Essays toward a Reflective Sociology*, Stanford, Stanford University Press.
- Bourdieu, P. (1984) [1979]. *Distinction: A Social Critique of the Judgment of Taste*, Cambridge, Harvard University Press.
- Bouvier, A., (2003), “Dans quelle mesure la théorie sociale de James Coleman est-elle trop parcimonieuse? ”, in *Revue française de sociologie*, 2003, T. 44-2, pp. 331-356.
- Bouvier, A., (2008), “ La théorie sociologique générale comme système hiérarchisé de modèles de portée intermédiaire”, in *Revue européenne de sciences sociales*, n°140, t. XLVI, pp. 87-106.
- Bouvier, A., (2016), “Analytical Sociology, Argumentation and Rhetoric. Large Scale Social Phenomena significantly Influenced by apparently Innocuous Rhetorical Devices ”, in D. Mohammed & M. Lewinski, *Argumentation and Reasoned Action, Proceedings of the First European Conference on Argumentation, Lisbon 2015*, vol. II, chapter 12, pp. 291-299.
- Bouvier, A., (2018a), “Intentional, Unintentional and Sub-intentional Aspects of Social Mechanisms and Rationality. The example of commitments in political life” in Gérald Bronner & Francesco Di Iorio (eds), *The Mystery of Rationality: Mind, Beliefs and the Social Sciences*, Dordrecht, Springer, pp. 17-36.
- Bouvier, A., (2018b), “The Empirical Relevance of the Joint Commitment Model of Collective Beliefs in the Social Sciences: Strength and Weakness”, in *Protosociology*, vol. 35, *Critical Essays on the Philosophy of Margaret Gilbert*, pp. 41-59.
- Bruch, E. & Mare, R., (2009) “Segregation Dynamics”, in Hedström, P. Bearman, P. & (eds), (2009a), chap. 12.
- Burian RM. (1985), “On conceptual change in biology: the case of the gene”, in Depew DJ, Weber BH, eds. *Evolution at a crossroads: the new biology*, Cambridge (Mass.) A Bradford Book.
- Cartwright, N., (1983), *How the Laws of Physics Lie*, Oxford, O.U.P.
- Cartwright, N., (1999), *The Dappled World: A Study of the Boundaries of Science*, Cambridge, Cambridge University Press.
- Cartwright, N. (2020), “Middle-range Theory: without it, what could anyone do?” in *Theoria*, 35/3 pp 269-323.
- Cat, J., (2021), “The Unity of Science”, in Edward N. Zalta (ed.), *The Stanford Encyclopedia of Philosophy*, <https://plato.stanford.edu/archives/sum2021/entries/scientific-unity/>.
- Clark, J., (ed.) (1996), *James S. Coleman*, London, The Falmer Press.
- Coleman, J. (1990), *Foundations of Social Theory*, Cambridge (Mass.) : University of Harvard Press.
- Cook, K.S., (2009) “Trust”. Hedström, P. and Bearman, P. (eds), (2009), chap.10.
- Cudd, A.E., (2014), “Commitment as Motivation: Amartya Sen’s Theory of Agency and the Explanation of Behaviour”, in *Economics and Philosophy*, Vol.30 – N. 1 – pp.35-56.

- Darden, L. (2007), "Mechanisms and Models" in D.L. Hull et M. Ruse [eds.], *Cambridge companion to philosophy of biology*, Cambridge University Press.
- Demeulenaere, P. (ed.) (2011), *Analytical Sociology and Social Mechanisms*, Cambridge, C.U.P.
- Dupré, J., (1993), *The Disorder of Things. Metaphysical Foundations of the Disunity of Science*, Cambridge, MA: Harvard University Press.
- Elster, J. (1977), *Ulysses and the Sirens. Studies in Rationality and Irrationality*, Cambridge, Cambridge University Press.
- Elster J., (1983), *Sour Grapes. Studies in the Subversion of Rationality*, Cambridge, Cambridge University Press.
- Elster J., (1989), *The Cement of Society. A Survey of Social Order*, Cambridge, Cambridge University Press.
- Elster, J., (1993) [1990], *Political Psychology*, Cambridge, Cambridge University Press.
- Elster, J., (1998), "A plea for mechanisms", in Hedström, P. et Swedberg, R. (dir.), 1998a, pp. 45-73.
- Elster J., (1999a), *Alchemies of the Mind. Rationality and the Emotions*, Cambridge, Cambridge University Press.
- Elster, J. (1999b), *Strong Feelings: Emotion, Addiction and Human Behavior*, Jean Nicod Lectures, Paris, CNRS.
- Elster, J., (2003), "Coleman on Social Norms" in *Revue française de sociologie*, 2003, T. 44-2, pp. 297-304.
- Elster, J., (2007), *Explaining Social Behavior: More Nuts and Bolts for the Social Sciences*, Cambridge, Cambridge University Press.
- Edling, C. & Hedström, P., (2005), "Analytical Sociology in Tocqueville's *Democracy in America*", *working papers series, on line*.
- Favell, A. (1996), "Rational Choice Theory as Grand Theory: James Coleman's Normative Contribution to Social Theory" in Clark (1996), pp.285-298.
- Festinger, L., (1957), *A Theory of Cognitive Dissonance*, Evanston (Ill.), Row & Peterson.
- Foucault, M. (1977) [1975], *Discipline and Punish. The Birth of the Prison*, New York, Pantheon Books.
- Galison, P. & Stump, D.J. (eds), (1996), *The Disunity of Science* (Stanford, CA: Stanford University Press).
- Gambetta, D. (ed.), (2000), *Trust in Society*, New York, Russell Sage Foundation.
- Gayon, J. (2007) "The Concept of the Gene in Contemporary Biology: Continuity or Dissolution?" In: Fagot-Largeault A., Rahman S., Torres J.M. (eds) *The Influence of Genetics on Contemporary Thinking. Logic, Epistemology, and The Unity of Science*, vol 6. Springer, Dordrecht, pp. 91-95
- Giddens, A., (1984), *The Constitution of Society. Outline of the Theory of Structuration*. Cambridge: Polity.
- Gilbert, M, (1989), *On Social Facts*, Princeton, Princeton University Press.
- Goffman, E. (1961). *Asylums: Essays on the Social Situation of Mental Patients and other Inmates*
- Goffman, E. (1969). *Strategic Interaction*. Philadelphia, University of Pennsylvania Press
- Granovetter, M., (1978), "Threshold Models of Collective Behaviour", *American Journal of Sociology*, n° 66, pp. 246-258.
- Granovetter, M. (1983). "The Strength of Weak Ties: A Network Theory Revisited" in *Sociological Theory*. 1: 201-233. .

- Habermas, J., (1984) [1981]. *The Theory of Communicative Action*. Vol. I: *Reason and the Rationalization of Society*, Boston, Beacon. [German, 1981, vol. 1]
- Habermas, J.,(1987) [1981]. *The Theory of Communicative Action*. Vol. II: *Lifeworld and System*, Boston, Beacon. [German, 1981, vol. 2]
- Hacking, I, (1996), “The disunities of science”, in P. Galison and D. Stump 1996.
- Hacking, I. (1983), *Representing and Intervening. Introductory Topics in the Philosophy of Natural Science*, Cambridge, C.U.P.
- Hardin, R. (1995), *One for All. The Logic of Group Conflict*, Princeton, Princeton University Press.
- Hardin, R., (2000), “Conceptions and Explanations of Trust”, in D. Gambetta (ed.) (2000), *Trust in Society*, New York, Russell Sage Foundation.
- Hardin, R., (2002), *Trust and Trustworthiness*, New York, Russell Sage Foundation.
- Hardin, R., (2003), “Why Know?”, *working paper*. (website : Russell Hardin’s Research Page)
- Hausman, D. M. (2005), “Sympathy, Commitment, and Preference”, in *Economics and Philosophy* 21: 33–50.
- Hedström, P. (2013), “Raymond Boudon (1934-2013)”, in *Revue française de sociologie*, vol. 54, p.I-III. »
- Hedström, P. & Swedberg, R. (eds), (1998), *Social Mechanisms. An Analytical Approach to Social Theory*, Cambridge, C.U.P.
- Hedström, P. Bearman, P. & (eds), (2009a), *The Oxford Handbook of Analytical Sociology*, Oxford, Oxford University Press.
- Hedström P. & Bearman, P. (2009b), “What is Analytical Sociology all about? An Introductory Essay” in Hedström P. & Bearman, P. (2009a), chap. 1., pp. 3-24.
- Hedström, P. & Udehn L, (2009), “Analytical Sociology and Theories of the Middle-Range” in Hedström, P. Bearman, P. & (eds), (2009a), chap. 2, pp. 25-50.
- Heider, F., (1958), *The Psychology of Interpersonal Relations*, New York, Wiley.
- Hempel, C., (1965), *Aspects of Scientific Explanation*, New York, Free Press.
- Hooker, C.A., (2007), “The Unity of Science” in Newton-Smith W.H. 2007, *A Companion to the Philosophy of Science*, chap. 78.
- Jahoda, M., Lazarsfeld, P., Zeisel, S. (1979) [1933], *Marienthal. The Sociography of an Unemployed Community*, London, Routledge
- Jeřábek, H., (2001), “Paul Lazarsfeld — The Founder of Modern Empirical Sociology: A Research Biography”. *International Journal of Public Opinion Research* 13:229–244 (2001)
- Jeřábek, H., (2011) “Merton and Lazarsfeld: Collaboration on Communication Research – Two Papers, two Research Instruments, and two Kindred Concepts”. *Czech Sociological Review*, vol. 17, n°6, pp. 1191-1214.
- Kalyvas, S., (2009) “Conflicts” in Hedström, P. Bearman, P. & (eds), (2009a), chap. 25.
- Katz E. & Lazarsfeld, P.L., (1955), *Personal Influence: the Part played by People in the Flow of Mass Communications*, London, Routledge.
- Kellert, St.H., Longino, H.E, & Waters, C.K. (eds.), (2006), *Scientific Pluralism*, Univ of Minnesota Press
- Kellert, S.H., (2008), *Borrowed Knowledge. Chaos Theory and the Challenge of Learning Across Disciplines*, Chicago: University of Chicago Press.
- Kiesler, C.A., (1971), *The Psychology of Commitment. Experiments Linking Behaviour to Belief*, New York, Academi Press
- Kitcher, P., (1981), “Explanatory Unification”, in *Philosophy of Science*, 48: 507.

Kuhn, Th. (1970), *The Structure of Scientific Revolutions*. Chicago, The University of Chicago Press.

Kuran, T., (1998), “Social Mechanisms of Dissonance Reduction” in Hedström et Swedberg, 1998, pp. 147-171.

Lazarsfeld, P. (1993), *On Social Research and its Language*. Chicago, Chicago University Press.

Lazarsfeld, P. F. and Merton, R.K. (1948), “Mass Communication, Popular Taste, and Organized Social Action”, in L. Bryson (ed.), *The Communication of Ideas*. New York: Harper, 95–118. Reprinted in: John Durham Peters and Peter Simonson (eds), (2004) *Mass Communication and American Social Thought: Key Texts, 1919–1968*. Lanham, MD: Rowman & Littlefield, pp. 230–241.

Luhman, N. (1995) [1984] *Social Systems: Foundations of a General Theory*, Stanford: Stanford University Press.

Machamer, P., Darden, L. and Craver, C.F. (2000), “Thinking about Mechanisms”, *Philosophy of Science*, 67, pp. 1-25.

Manzo, G. (ed.), (2014), *Analytical Sociology. Actions and Networks*, Chichester, Wiley.

Merton, R., (1948), “Discussion of Parsons’ “The Position of Sociological Theory”, *American Sociological Review*, 13, pp. 164-8.

Merton, R., (1957), *Social Theory and Social Structure*, Glencoe, Ill., Free Press.

Merton, R., (1967), “On Sociological Theories of the Middle-Range” in Merton, R. *On Theoretical Sociology*, New York, The Free Press.

Merton, R., Coleman, Rossi, P.R., (1979), *Qualitative and Quantitative Social Research: Papers in Honor of Paul F. Lazarsfeld*, London, The Free Press.

Nagel, E., (1951), *The Structure of Science*, New York: Harcourt, Brace and World.

Neurath, O. (1938), “Unified Science as Encyclopedic Integration”, *International Encyclopedia of Unified Science*, The University of Chicago Press, vol. 1, no 1, p. 1-27.

Noguera, J.A., (2006), “Why We Need an Analytical Sociological Theory”, in *Papers, Revista de Sociologia*, Barcelona.

Oppenheim, P. and H. Putnam, (1958), “The Unity of Science as a Working Hypothesis”, in H. Feigl et al. (eds.), *Minnesota Studies in the Philosophy of Science*, vol. 2, Minneapolis: Minnesota University Press.

Parsons, T. (1937), *The Structure of Social Action*, Dubuque, McGraw Hill

Parsons, T. (1951). *The Social System*. England: Routledge

Pettit, Ph. (2005), “Construing Sen on Commitments”, *Economics and Philosophy*, 21, pp. 15-32.

Pieter, F. & Spiekermann, K. (2011), “Rules, Norms and Commitments” in Jarvie, I.C. and Zamora-Bonilla, J. (eds), (2011), *The Sage Handbook of the Philosophy of Social Sciences*, Los Angeles, Sage pp. 217-239.

Pollack, M. (1980), “Paul Lazarsfeld: A Sociointellectual Biography”, *Knowledge*, vol. 2, N.2, pp. 157-177.

Reid, Th., (1997) [1764], *An Inquiry into the Human Mind on the Principles of Common Sense*, University Park, Pennsylvania State University Press.

Ruphy, St. (2016), *Scientific Pluralism Reconsidered*, Pittsburgh: University of Pittsburgh Press.

Salganik, M.J. & Watts, D.J. (2009), “Social Influence: the Puzzling Nature and Success in Cultural Markets”, in Hedström & Bearman, 2009, chap. 14, p. 315-341.

Schelling, Th. (1978), *Micromotives and Microbehaviors*, New York, Norton and Company

- Sen, A. (1977). "Rational Fools: a Critique of the Behavioral Assumptions of Economic Theory". Reprinted in *Philosophy and Economic Theory*, ed. Hahn, F. and Hollis, M., 87–109. New York, NY: Oxford
- Sen, A. (1987), *On Ethics and Economics*. Oxford: Basil Blackwell.
- Sen, A. (2005), "Why exactly is Commitment Important for Rationality" in *Economics & Philosophy*, Vol. 21, 1, p. 5-14.
- Simon, H., (1996), *The Sciences of the Artificial*, Cambridge, MA: The MIT Press.
- Smith, A., (2002) [1759], *The Theory of Moral Sentiments*, Cambridge, Cambridge University Press
- Swedberg, R., (1996), "Analysing the Economy: On the Contribution of James S. Coleman" in Clark (1996), pp. 313-328.
- Tilly, Ch. (1997), "James Coleman as a Guide to Social Research", in *The American Sociologist*, 28, pp. 82-7.
- Tocqueville, A. (1955) [1856] *The Old Regime and the Revolution*, New York: Anchor Books.
- Turner, J. H. (1974). *The Structure of Sociological Theory*. Homewood, Illinois: Dorsey Press.
- Turner, St., & Turner, J. (1990), *The Impossible Science: An Institutional Analysis of American Sociology*. Beverly Hills and London: Sage.
- Turner, St., (1986), *The Search for a Methodology of Social Science: Durkheim, Weber, and the Nineteenth Century Problem of Cause, Probability, and Action*. Boston Studies in the Philosophy of Science, 92. Dordrecht, Holland: Reidel. 1986.
- Van der Berg, A., (1998), "Is Social Theory too Grand for Social Mechanisms ?" in Hedström, P. et Swedberg, R. (dir.), 1998, pp. 204-237.
- Veyne, P., (1992) [1976], *Bread and Circuses: Historical Sociology and Political Pluralism*. London, Penguin Books
- Watts, D.J. & Dodds, P. (2009), "Thresholds Effects of Social Influence", Hedström, P. Bearman, P. & (eds), (2009a), chap. 20
- Weber, M. (2002) [1905]. *The Protestant Ethic and The Spirit of Capitalism*, translated by S. Kalberg. Roxbury Publishing.

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<sup>3</sup> A very first version of this paper was given in September 2009 at Genève (Switzerland) at the annual SOPHA (SOCIÉTÉ DE PHILOSOPHIE ANALYTIQUE) Conference.

<sup>4</sup> I will not directly tackle here other frequent shortcomings of contemporary sociology from the point of view of the scientific ideal: the intentional absence of axiological neutrality and the abuse of metaphors and other rhetorical devices.

<sup>5</sup> See, e.g., Jahoda, M., Lazarsfeld, P., Zeisel, S. 1979 [1933], Katz E. & Lazarsfeld, P.L., (1955), Lazarsfeld, P. (1993). And for historical comments : Merton, R., Coleman, Rossi, P.R., (1979), Pollack, M. (1980), Boudon (1993), Jeřábek, H., (2001),

<sup>6</sup> E.g. Lazarsfeld, P. F. and Merton, R.K., 1948.

<sup>7</sup> For a comment: Jeřábek, H., (2011).

<sup>8</sup> In particular, while Schelling, Boudon and Elster have worked within the context of Rational Choice Theory (sometimes to insist on its limits, as Boudon, 1981, and yet more Elster, 1977, 1983, 2003 did), this was not the case of Granovetter (1983), more closely connected to network analysis, in the continuity of Harrison White (1978), or Goffman (1969), who worked within the “symbolic interactionism” framework. See fourth and fifth section for other examples of either well-known or unnoticed middle-range theorists.

<sup>9</sup> On the specific role of Boudon in this process, see Hedström (2013): “[Raymond] Boudon argued that statistical models of the sort advocated by [Robert M.] Hauser [a famous methodologist] are useful for many purposes, but that their usefulness for explanatory purposes was considerably more restricted than assumed by Hauser. Boudon argued that explanations normally are arrived at not by the means of descriptive statistical models, but through theoretical models that show the abstract *logic* of the process being analyzed [...]. This generative view of sociological explanations is now at the very core of analytical sociology ».

<sup>10</sup> See also Barbera (2004).

<sup>11</sup> See this section and the fifth section too.

<sup>12</sup> The concept of mechanism has so far not been discussed at length. Its specific meaning in analytical sociology seems to come from biology: in biology, unlike in physics (especially in astronomy), necessary predictions *cannot* exist because there are always plenty of exceptions (mainly due to a probable great deal of interferences with multiple other unknown causal chains): even when an effect *seems* unavoidable, this is often wiser to only claim that this effect is only *almost* unavoidable. Within the context of analytical sociology, the concept of mechanism is therefore quite distinct from the Cartesian and Newtonian view of Mechanics (which implies absolute necessity) but still often confused with them.

<sup>13</sup> I do not have the space to elaborate these examples, all of which I borrow from Biggs, 2009 (p. 298).

<sup>14</sup> This is why the wordings “micro-models” or “meso-models” are, strictly speaking, definitely more relevant than the formulation “local models”: middle-range theories are not really proper to a particular domain but rather *transversal* to various domains. See the fifth section.

<sup>15</sup> Bourdieu’s goal of constructing a general theory sacrifices a very interesting micro-model (or meso-model) – wonderfully illustrated in many case studies (Bourdieu, 1977, 1984) – to what can be viewed as an artificial too global theory which, as such, erases relevant empirical differences between other close micro-mechanisms. Furthermore, as it has been often noticed, if *habitus* can explain the reproduction of certain structures, it cannot explain their transformation (which happily sometimes nevertheless occurs) (Boudon, R., (1981) [1979]). Of course, intellectual “hubris”, that is the temptation to claim that a local model is relevant beyond its domain of validity, is frequent and Bourdieu was surely not an exception.

<sup>16</sup> For example, the mechanism of self-commitment (illustrated by Ulysses and the Sirens in Homer (Elster, 1977)), the sour grapes mechanism, illustrated in La Fontaine (Elster, 1993).

<sup>17</sup> Cartwright alludes to the fact that, according to Elster, common sense maxims and precepts often express suggestive although sometimes contradictory kinds of explanation in social sciences. Among many examples: “Nothing is easier for passion than to overcome reason; its greater triumph is to conquer interest” (Elster, 1999a, p. 79, quoting La Rochefoucauld) and: “One may forgive an infidelity, but not to forget it” opposed to “One may forget an infidelity, but not forgive it” (both formulated by Mme de Lafayette) (Elster, 1999a, p. 81).

<sup>18</sup> See, e.g., Nagel, E., (1951), Oppenheim, P. and H. Putnam, (1958).

<sup>19</sup> See, e.g., Kitcher (1981) and, for an overview of the issue, Hooker (2007).

<sup>20</sup> See also Dupré, J., (1993), another leading figure of this school.

<sup>21</sup> See Cat (2021) for other examples.

<sup>22</sup> This stance is typical of the current “Minnesota School” (Kellert, Longino and Waters 2006, Kellert 2008) as distinct from the “Stanford School”. See Cat (2021). See also Ruphy (2017), close to the Minnesota School, and initially trained by Philip Kitcher. The problem is different from the one raised by Thomas Kuhn (1970). The main idea here is that various theories on a same domain may *not* be incommensurable: they just offer distinct kinds of explanation, each of them relevant for a specific scientific goal (like parallel classifications of animals in genetics and in ethology). See Hacking (1983) for a well-balanced evaluation of Kuhn’s ideas.

<sup>23</sup> For brilliant uses of the cognitive dissonance theories in social sciences, see Kuran, T., (1998) and Elster (2007).

<sup>24</sup> Coleman (1990), p. 15; p. 18-19.

<sup>25</sup> See also, for a useful comment, Edling, C. & Hedström, P., (2005).

<sup>26</sup> I am constrained to much simplify Coleman’s analysis of this first step because Coleman envisages many different cases.

<sup>27</sup> See also Pieter, F. & Spiekermann, K. (2011); Cudd (2014).

<sup>28</sup> One could speak of “analytical biology” here by comparison with analytical sociology if this wording had not already been chosen to characterize the use of mathematical models in biology.

<sup>29</sup> See also Noguera, (2006), Demeulenaere (2011), Manzo (2014), all in line with Hedström, P. & Swedberg, R. (eds), (1998).

<sup>30</sup> Biggs also distinguishes conceptually with between self-fulfilling prophecies, which are often fascinating but probably very infrequent, and inductively derived predictions, which are much more trivial but also very likely much more frequent, as far as one can distinguish between them empirically, which often turns out to be difficult.