SUBJECTIVE RATIONALITY AND AGGREGATIVE PROCESSES: AN ACTIONIST PERSPECTIVE ON THE DYNAMICS OF SCIENCE

Michel Dubois
Centre National de la Recherche Scientifique – Université de Paris IV and GEMAS

The evolution of the contemporary sociology of science is paradoxical. Most sociologists have substituted for an institutional vision of science an approach mainly focused on scientific actions and their consequences, but still only a few of them have paid real attention to the strengths of the Actionist research programme (henceforth ARP), at least in comparison to ethnomethodology, interactionism, constructivism or more recently actor–network theory. In the course of this chapter we wish to analyse this apparent paradox from at least two different points of views.

First, from its truthfulness. Are the general principles of ARP really seldom at work in the various studies produced by the sociologists of science? This programme has well-known applications in sociology of education, economy or social mobility. Some sociologists are even seeing the growing number of these applications as a convincing proof of the existence of cumulative knowledge in social sciences. Why could not it be the same for the study of scientific action and by extension the consequences of this action for the scientific community?

Second perspective on our initial paradox: its subjective plausibility. Are there any “good reasons” to believe that ARP might not be a suitable framework to study the various social dimensions of contemporary science? This goes beyond specifying the ARP theoretical core
that objectively contradicts, directly or indirectly, the principles of the leading currents in sociology of science. We have to identify some of the misunderstandings associated, intentionally or unintentionally, with ARP by sociologists of science.

In order to address both issues — truthfulness and subjective plausibility —, we will outline certain aspects of the writings of Raymond Boudon. His general and decisive contribution to ARP is well-known. In the first part of this chapter we are dwelling on Boudon’s contribution on a more specific point: the emergence of a critical point of view on the leading currents in the sociology of science of the 80s and the beginning of the 90s. In fact, during this period many Mertonian sociologists had repeatedly regretted Merton’s silence regarding the radicalisation of a new generation of sociologists of science. Soon after his death in November 2003, T. Gieryn (2004) was recalling Merton’s low opinion of the leaders of the “paradigm shift” of this generation. But for intellectual and organizational reasons, he never felt compelled to engage himself in the controversy with the self-proclaimed “Mertonian enemies”. Our idea is that this missing voice has contributed, in the 1980s, to accentuate the marginalisation of those still working on topics such as social stratification in science, values and norms, intellectual property, etc.

For various reasons, those “traditional” topics are nowadays much more attractive to many sociologists of science. One of these reasons is the evolution of science itself. The transformation which occurred at the end of the 20th century — the emergence of the biotechnologies for example — generates understandably a renewal of interrogations of the values and norms of the contemporary scientific community. Another important source of this renewal comes from the fact that the limits and consequences of the mid-1970s “paradigm shift” have been more and more widely criticized — including by sociologists with no previous interest in the domain.

In contrast to Merton, R. Boudon has never considered himself as a sociologist of science, but much more as a sociologist of knowledge or belief(s). But he has contributed with others in France to fill the Mertonian gap, i.e. to elaborate a critical perspective on the pitfalls of the “new sociology of science” (henceforth NSS). We propose briefly: 1) to specify Boudon’s position in the intellectual aera of sociology; 2) to recall the main aspects of his analyses of NSS; and 3) to identify some of the misunderstandings frequently generated by this analysis.
The second part of this chapter is concerned with a less discussed but no less important dimension of the relation between sociology of science and ARP: the study of aggregative processes in science. Boudon’s studies have demonstrated many times that if the actions of individuals are interdependent, they often have aggregate results that are contrary to what any of the individuals intended or desired. He has frequently acknowledged the value of Merton’s paper on the “unanticipated consequences of purposive social action” (1936). One can see the English translation of the title of his book Effets pervers et ordre social (1977) — The Unintended Consequences of Social Action (1982) — as a direct homage to Merton’s pioneer work. We will argue here that even if R. Boudon did not himself apply his general approach of the unintended consequences of social action to the scientific community, this task remains crucial for any contemporary sociologist of science.

THE “GOOD REASONS” OF COGNITIVE RELATIVISM

In order to apprehend Boudon’s general situation in the intellectual area of sociology, we propose here to combine bibliometrics and methods developed for the analysis of social networks.

R. Boudon Co-citation Networks

Using two databases (Arts & Humanities Citation Index, Social Sciences Citation Index) from Thomson Scientific Web of Science (WOS), we consider two periods: 1985–1994 and 1995–2004. For each period, we retain for our study all cited references of the articles — and articles only — including at least one citation of R. Boudon (articles and/or books). The limits of Thomson Scientific databases are well known (for example the low number of non-English scientific journals) — especially for French sociologists. However, within these limitations, they are useful to produce a first approximation of the different sociological uses of R. Boudon’s writings. From the 6024 cited authors in the 381 articles of the first period, and the 7132 cited authors in the 437 articles of the second period, we produce two successive co-citation networks. In its general principle, the usefulness of a co-citation network is apparent when one considers that every co-citation action — the fact of any author to asso-
ciate two authors, or two documents in the cited references of a single article — represents a bit of information on the intellectual linkages produced by the scientific actors. And this even if these linkages have to be cautiously interpreted.

The table 1 below details elementary indicators for both networks.

The values on the two first lines of table 1 have been already discussed (number of articles, number of cited authors in the articles). The value on the third line is the sum of the symmetrical relations (edges) between the cited authors (vertex) that constitutes each global co-citation network. By principle, there cannot be any non-symmetrical relation (arcs) in a co-citation network. The fourth line presents the average degree — the average number of links by author — corresponding to each network. No need here to distinguish between in-degree and out-degree. The fact that R. Boudon has a degree of 494 for the first period and 498 for the second is, of course, directly related to our initial criteria: every article in our sub-database has to have at least one citation of R. Boudon. Hence his top ranked degree.

In order to visualize R. Boudon’s co-citation networks, we have chosen a three step method, the application Pajek developed by V. Batagelja and A. Mrvar. First we removed all edges from a certain value frequency, then from this reduced network we constructed a partition that keeps only authors who are at a certain distance (distance = 1) from R. Boudon (k-neighbors from R. Boudon). Finally, we used the Kamada-Kawai algorithm in Pajek to visualize our partitions (figures 1 and 2 below). The

Table 1

<table>
<thead>
<tr>
<th>Databases: Arts &amp; Humanities Citation Index, Social Sciences Citation Index (WOS)</th>
<th>Ccn1 1985–1994</th>
<th>Ccn2 1995–2004</th>
</tr>
</thead>
<tbody>
<tr>
<td>Numbers of articles with at least one citation of R. Boudon</td>
<td>381</td>
<td>437</td>
</tr>
<tr>
<td>Numbers of authors (vertex) cited in N articles</td>
<td>6024</td>
<td>7132</td>
</tr>
<tr>
<td>Numbers of lines (edges)</td>
<td>42049</td>
<td>43886</td>
</tr>
<tr>
<td>Average degree</td>
<td>13.96</td>
<td>12.30</td>
</tr>
<tr>
<td>Sd</td>
<td>(43.97)</td>
<td>(36.20)</td>
</tr>
<tr>
<td>R. Boudon degree</td>
<td>494</td>
<td>498</td>
</tr>
<tr>
<td>k-neighbours of R. Boudon (distance = 1) [after removing from initial network co-citation lines with value lower than 15]</td>
<td>69</td>
<td>105</td>
</tr>
</tbody>
</table>
Figure 1: R. Boudon’s co-citation network — 1985–1994
Figure 2: R. Boudon’s co-citation network — 1995–2004
size of each vertex being proportional to its degree value, R. Boudon is represented by the central biggest vertex in our two graphs.

Figures 1 and 2 represent the main co-citing patterns associated with the sociological uses of R. Boudon’s works by the authors of our 818 articles. Figure 1 shows two important clusters. On the upper side of the graph, we can see well-known sociologists regularly co-cited with R. Boudon, but also regularly co-cited with one another (Blau, Coleman, Merton, Granovetter, etc.). On the lower side of the graph, we have a national counterpart to the previous international cluster: French sociologists regularly co-cited with Boudon, and also regularly co-cited with one another (F. Chazel, M. Cherkaoui, F. Gresle, etc.). The expected presence of P. Bourdieu in this second cluster illustrates the interpretative cautiousness needed by this approach. A co-citation is sometimes naively considered as an indicator of proximity between two authors — at least in the mind of those producing the co-citation act. Instead of proximity, we prefer here to speak of “meaningful relation” in the mind of those producing the co-citation act. Sociologists have frequently “good reasons” to cite in their articles two authors commonly considered as bearers of divergent or opposite views on a single general topic.

Figure 2 constitutes a transformation of the previous graph. The French cluster is still present (right side of the graph), but its relative importance in the total network has considerably decreased — even with the appearance of new authors (such as E. Lazega or L.A. Vallet). For the rest of the graph, we still find the previous main co-cited authors, but their relative importance in the network has also changed. The link between Boudon and Coleman or Granovetter, for example, is much stronger (represented by the thickness of the lines) than in the previous network. In addition, instead of having one international cluster, we have now several components which represent a higher degree of complexity of the co-citation network.

**Argumentation and Scientific Knowledge**

How can these two graphs help us to identify the nature of the relation between ARP and contemporary sociology of science? The answer is simple: both of them show that for the sociologists publishing in the journals indexed by WOS, the association of R. Boudon with various
representatives of the sociology of science remains peripheral in comparison to other specialities of sociology (theory, economy, mobility, education, etc.). In fact it is mainly in the first co-citation network, that authors such as P. Sorokin (famous for his study of mobility but also for his study of the dynamic of scientific theories), R.K. Merton, P. Allison, J. Agassi and of course T.S. Kuhn indicate a collective meaningful relation of Boudon’s books and articles with problems related to the analysis of contemporary science and/or scientific community.

This concentration on the first period is doubtless related to the fact that most parts of Boudon’s works on scientific knowledge and/or scientific argumentation were published between 1986 and 1994. Indeed the publication in 1995 of “Le Juste et le vrai: études sur l’objectivité des valeurs et de la connaissance” marks a durable reorientation of Boudon’s interest towards social values and moral feelings. Let us briefly recall here some elements of the previous period. In a chapter of *The analysis of ideology* (1986) — “science and ideology” — Boudon acknowledges the value of T.S. Kuhn’s idea that scientific knowledge implies by principle non-scientific beliefs: “ideology lies within scientific work. And I do not mean here those pseudo-scientific theories which, undoubtedly, are more frequent in social scienc than is required, but the authenti
cally scientific theories”. The notion of paradigm represents then for Boudon a particular instance of general *a priori* forms — categories or general reasoning patterns — which are, in normal conditions, as widely accepted as generally undisputed by most scientists. The study of those reasoning patterns constitutes the centre matter of *The Art of self persuasion* (1992). According to Boudon, all reasoning is set in an implicit framework: “a system of propositions which subjects have good reasons for regarding as self-evident”. Here again *a priori* forms are described as “omnipresent in ordinary knowledge as well as in scientific knowledge”: hence the usefulness of a “cognitive sociology” mainly oriented toward the analysis of the various forms of scientific argumentation. Among his various examples, Boudon has regularly interpreted some of the most provocative propositions of NSS (by contrast to the traditional sociology of science or scientific knowledge [Boudon, 1994; 1994a]) — the conventional nature of scientific knowledge, the absence of scientific progress, the equal cognitive value of science and myth, etc. — as outcomes of the existence of *a priori* forms in scientific reasoning. Why do D. Bloor’s (strong programme of sociology of science in the 1980s) claims that
mathematical truths must be considered as collective representations in the Durkheim sense? Why do B. Latour and S. Woolgar (constructivist programme of sociology of science in the 1980s) view objective reality as a consequence rather than a cause of scientific activity? In both cases those propositions are neither the products of pure rationality or pure irrationality. They are drawn from a line of reasoning with at least two components: (1) a series of solid and explicit statements, and (2) a narrow a priori principle — called by Boudon principle of “uniqueness of truth” — which tends to impose itself by default in the mind of those sociologists of science. This approach of scientific argumentation implies the reconstitution — formalization — of the various reasons of the radical sociologists of science: objective reasons, but also less objective ones called trans-subjective reasons or “good reasons”. And from this point of view what distinguish objective reasons from “good reasons” is not a difference of nature but a difference of degree. The first have to be considered as the methodic continuity of the latter. From a general point of view, those “good reasons” have four main characteristics: inherent to scientific argumentation, they are also the necessary conditions to our ordinary reasoning patterns. They have an extended but non-universal validity which contributes, in a scientific context, to produce false interpretation and/or incorrect simplification. Their plausibility depends on the ability of the individual who endorses them to firmly believe that other individuals could also, just like him, consider them as solid. Finally their diffusion among individuals, and more widely among the various social groups, can vary from a period to another.

Three Ordinary Misunderstandings

R. Boudon has certainly not been the first to question the value of the cognitive relativism of NSS. Specialists such as T. Gieryn (1982), G. Freudhental (1984), B. Matalon (1986) or S. Cole (1992) — to name only a few of them — have, each in their own way, demonstrated the weakness of those pretending to support, through their empirical studies, the general thesis that scientific knowledge is a pure construction which cannot aim at being objective. The sole existence of those internal critics show that the failure of the radical social studies of sciences by no means invalidates the general principle of a more temperate sociological study of science. And in fact much has been done in the speciality since
the general acceptance of the limits of the strong programme and the constructivist programme in the mid 90s.

Boudon’s interest for modern relativism — cognitive, but also moral and aesthetic — is sometimes misleadingly interpreted as a proof of (1) a philosophical inclination of ARP or (2) a consequence of the deep roots of ARP in (2a) positivism and (2b) rationalism. Although recurrent, those three points are easily refuted. As showed in the previous section, in contrast to the internal critic of NSS (mainly theoretical and methodological), Boudon anchors his analysis in a sociological questioning. How do social actor produce the plausibility of his own beliefs? By dwelling on the intersubjective nature of the argumentation which generates cognitive relativism as a collective belief, he explicitly takes place in the tradition of sociology of knowledge. More largely, he conceives the cognitive relativism of NSS as one particular aspect of a macro phenomena in the realm of the western societies beliefs. It is the sociologist’s task to identify the multiple factors characterising those societies — among which the collapse of the great ideologies, the growing cultural heterogeneity — that have created conditions favourable to the positive reception of any theory that nourishes relativism. The second point (2a) is a direct illustration of the reasoning patterns described in *The art of self persuasion*. If a main representative of ARP criticizes the anti-positivism of NSS, then what else can ARP be than a positivist programme in sociology? Although very common in the social sciences, this binary framework (positivism vs antipositivism, explanation vs interpretation, objectivity vs subjectivity, etc.) is of course much too simplistic for ARP which have both interpretative and explicative dimensions. For Boudon (1994a: 126) “the binary question underlying the *Methodenstreit* on positivism in social sciences is clearly meaningless”. Finally the third point (2b) is derived from the proposition regularly made by Boudon that one of the crucial tasks of social sciences is to substitute for a common sense “irrational” explanation of particular behaviour, or a particular belief, a rational explanation. But then again, as shown previously, the model of “good reasons” or subjective rationality used to analyse the diffusion of relativism constitutes a clear demarcation from the traditional — narrow — theory of rationality. Not only in this model one can regard an act as subjectively rational when the actor can give reasons for it, whatever these may be. But as Boudon has many times argued, this remains a methodological principle with no necessary ontological implication. No need for the
sociologist to speak of the rational or irrational nature of the human beings attracted by cognitive relativism. The sociologist deals with social actors relating their reasoning patterns to their social positions.

**SCIENCE AND AGGREGATIVE PROCESSES**

The analysis of aggregative processes within the scientific community represents a second major dimension of the relation between ARP and sociology of science. Once again we have to start with a paradox. Since Merton’s famous paper on the “unanticipated consequences of purpo-sive social action”, the sociology of science has from its origins been closely associated, at least potentially, with the general topic of “perverse effects”. At the same time, looking back through the history of the speciality, one can easily show that this topic has not yet received the full attention it really deserves. In fact, not only have some influential contemporary sociologists of science not taken into account ARP’s central thesis that, at a meso level or a macro level, the sum of individual actions brings about paradoxical outcomes and results contrary to individual expectations. But, somehow more familiar with the idea of “opposition” — probably through their long term involvement in controversy studies — than with the one of “additive process”, they sometimes even questioned the sociological usefulness of the concept of aggregation. We have analysed how some representatives of Actor-Network-Theory regularly oppose the so-called “enigmatic character of aggregation” to the alleged conceptual transparency of the concept of “association” (Dubois 2007).

In order to reduce this remaining enigmatic halo surrounding the concept of aggregation, we start here from a simple example: the action of scientific citation and its aggregate consequences. We will then briefly characterize and illustrate the idea of a sociological analysis of aggregative processes in science.

*Scientific Citation and Its Consequences*

The two graphs of the previous section have been constructed by analysing the references of the articles indexed by WOS. The sociologists who published the 818 articles of our sub-database have chosen to cite
at least once Boudon’s writings (books or articles). Of course, as shown previously, they cite in their articles a lot of other authors in various proportions. Figures 1 and 2 allow us to easily visualize the few names of the authors frequently co-cited with Boudon.

Sociologists of science have frequently been puzzled by the very nature of the practice of scientific citation, and by extension of co-citation. N. Kaplan (1965), H. Zuckerman (1987) H. Small (2004) for example, have proposed a normative theory of citation. Others like N. Gilbert (1977) or S. Cozzens (1989) preferred a strategy-rhetoric model of citation. No need here to evaluate the relative merits of each model, but for normative or strategic reasons — probably both — scientists, within a limited intellectual area (specialties or disciplines for example), generally cite in great proportion only a few references or authors, leaving the great majority of other possible references with very few citation, and most of the time with no citation at all.

The data used in the previous section confirm this fact. The two distributions of citation frequency corresponding to the 381 articles of the first period, and 437 articles of the second period are detailed in table 2 below and visualized in figures 3 and 4.

Figures 3 and 4 show that the number of times an author is cited (in-degree for each scientific publications network) follow a power-law distribution as indicated by the approximately straight-lines forms on the doubly logarithmic scales. Those distributions constitute structural

Table 2

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<tr>
<td></td>
<td>numbers of authors</td>
<td>% numbers of authors</td>
</tr>
<tr>
<td>&gt; 400</td>
<td>1</td>
<td>0.017</td>
</tr>
<tr>
<td>100–400</td>
<td>1</td>
<td>0.017</td>
</tr>
<tr>
<td>50–100</td>
<td>14</td>
<td>0.232</td>
</tr>
<tr>
<td>40–50</td>
<td>11</td>
<td>0.183</td>
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<tr>
<td>30–40</td>
<td>14</td>
<td>0.232</td>
</tr>
<tr>
<td>20–30</td>
<td>42</td>
<td>0.697</td>
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<tr>
<td>10–20</td>
<td>144</td>
<td>2.391</td>
</tr>
<tr>
<td>5–10</td>
<td>400</td>
<td>6.642</td>
</tr>
<tr>
<td>1–5</td>
<td>5395</td>
<td>89.588</td>
</tr>
</tbody>
</table>
Figure 3: Distribution of citation frequency (log log) period 1

Figure 4: Distribution of citation frequency (log log) period 2
properties of the scientific publication networks, *i.e.* attributes that cannot distinguish individual members of the scientific community and that therefore constitute emergent properties of group structure. Those distributions are however the consequences of an aggregative process. One after another, independently or more surely in an interdependent manner, each scientist through his own action contributes to the emergence of those structural properties. The potential paradoxical nature of the consequences of the practice of scientific citation appears most clearly within the strategic-rhetoric model of citation of Gilbert and Cozzens. Why do scientists cite only a few authors within their speciality? Because, according to this model, they generally try to convince their readers that the value of their contribution to common scientific knowledge is at least equal, if not superior, to the contribution of the most well-known authors of their speciality. But by doing so they help to reinforce, at a meso level, the collective visibility of already highly visible scientists. Instead of reducing the distance that separates them from those famous authors — the initial intention —, they help to increase that distance and reduce their own visibility — unintended consequence of their action.

*A Sociology of Aggregative Processes in Science*

This simple example allows us to briefly characterize the general aspects of a generic sociology of aggregative processes at work within the scientific community. To study the aggregative processes consists mainly in analysing the various sequences ensuring the emergence of a collective dimension at an abstract level $n$ from individual actions and/or preferences located at a different abstract level (usually level $n-1$). More specifically a sociological study of aggregative processes in science should be characterized, besides a general interest in the dynamics of science, by (factor 1) a multidimensional representation of those dynamic dimensions (most frequently micro–meso or micro–macro), (factor 2) some hypothesis or conjectures related to the mechanism of accumulation of individual actions or preferences that allows the articulation of the various dimensions, (factor 3) an evaluation of the potential divergence between the initial intentions of individuals located at level $n-1$ and the aggregated consequences of their actions at level $n$.

If one accepts that it is possible to classify the various objects of the sociology of science into two general categories — on one side the various
forms of social action in science (category 1), on the other side the various products, intellectual and material, of those social forms (category 2) — it becomes possible to construct an elementary typology based on our three factors. Table 3 details this typology of aggregative processes.

In table 3, factor 1 represents the multidimensionality of sociological analysis through two types of relations: micro–meso, micro–macro. Factor 2 represents two basic hypotheses concerning the modality of accumulation of individual actions and/or preferences: independency or interdependency. In one case, social actors are producing their actions with no consideration of others actor’s actions (past or present or expected). In the other, social actors are producing their actions with other actor’s actions (past, present or expected) in mind. Factor 3 represents the divergence between initial intention of actions and their aggregate consequence (+) or the absence of divergence (–).

This deliberately simplified typology differs from the one briefly sketched by Boudon at the beginning of *The unintended consequences of social action*:

The number of possible recombinations of the following criteria does, therefore, defines the number of possible configurations: 1. No participants (1a), some participants (1b), all participants (1c) attain their individual objectives; 2. producing, at the same time, benefits (2a) or problems (2b) or else collective benefits and problems (2c); 3. each of these applying only to some (3a) or to all the participants (3b) (p. 6).
This initial classification concentrates on the various configurations corresponding to the factor 3 of table 3. But doing so it contributes unfortunately to reduce the sociological analysis of aggregative processes to the description of the diversity of their outcomes (perverse effects). In contrast, it seems to us very important not to minimize the analysis of factors 1 and 2.

Now coming back to our previous example, where do we locate in table 3 the aggregative process generating the structural property of the publications network of our 818 articles? We are dealing here with references in scientific publications (category 2), produced by interdependent scientists, whose accumulation generates a collective property (power law distribution of citation frequency at the meso level) potentially contrary to individuals intentions (increasing instead of reducing their distances from famous authors). Those various characteristics of our example correspond to the type 6 of table 3.

Even if we cannot go here into details, we have proposed elsewhere to take this elementary typology of aggregative processes as a general guideline for a reinterpretation of classic and contemporaries studies in the sociology of science (Dubois 2009). Authors as different as Price (1963) on the enlargement and emergence of a new “metropolis” in science, Merton (1968, 1988) on the process of accumulative advantage in science, Small and Griffith (1974) on the macrostructure of science, Rip (1986) on the emergence of biotechnology in the Netherlands or more recently Lazega et al. (2004) on scientific innovation within French cancerology — to name only a few of them —, all have their specific location in our general table 3. And consequently all of them are demonstrating the scientific value of a multidimensional approach of the dynamics of science and, by doing so, confirming the necessity to study in a more systematic way aggregative processes in science.

REFERENCES

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