

Should one still read Durkheim's rules after one hundred years?

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SHOULD ONE STILL READ DURKHEIM'S RULES AFTER ONE HUNDRED YEARS?

Raymond Boudon interviewed by Massimo Borlandi

Borlandi: *The Rules of Sociological Method is a book you have never been fond of. Leafing through the pages you have devoted to Durkheim these last thirty years, I constantly come up against this fact: you have always separated the theorised method from that put into practice in Durkheim's work. You maintain there is a net duality between the two: reciprocity is weak and the dominating relationship is that of opposition. As early as your article of 1964 on Tarde's "statistique psychologique" (subsequently republished in *La crise de la sociologie*,¹ you suggested that in order to read Durkheim you had to learn to go beyond the dogmatism of his statements. In *L'analyse mathématique des faits sociaux*,² you distinguished between the method Durkheim adopted in *Suicide*, which you called "implicit" or "not expressed", and that in the *Rules*, which you described as "explicit". In this book (p. 39–40), you went so far as to rewrite the rules of the *Rules*. In *La logique du social*,³ as in the *Dictionnaire*⁴ (entry "Durkheim") and elsewhere, the Durkheim that came to the fore, i. e. an individualist in his way of explanation, contrasted with the Durkheim he wanted to be, i. e. holist or "sociologiste". I find once again your idea that Durkheim used his real method "implicitly" in *L'art de se persuader*.⁵ Lastly, in the *Introduction to Traité de sociologie*,⁶ you are drastic: what Durkheim said and what Durkheim did are two different things. What comments did you have to make then – and would still make now – on Durkheim's theorised method, especially as regards *The Rules of Sociological Method*?*

Boudon: My main objection is against Durkheim's positivism. Durkheim is right when he claims sociology can be as scientific as any other science, but wrong in his conception of science. The basic statements of positivism, in the

1 Genève, Droz, 1971, 75–91 (*The Crisis of Sociology*, London, Macmillan/New York, Columbia University Press, 1981).

2 Paris, Plon, 1967 (*The Logic of Sociological Explanation*, West Drayton, Penguin Books, 1974).

3 Paris, Hachette, 1979 (*The Logic of Social Action*, London/Boston, Routledge & Kegan Paul, 1981).

4 R. Boudon et F. Bourricaud, *Dictionnaire critique de la sociologie*, Paris, Puf, 1982 (*A Critical Dictionary of Sociology*, Chicago/London, The University of Chicago Press et Routledge, 1989).

5 Paris, Fayard, 1990 (*The Art of Self-Persuasion*, London, Polity Press, 1994).

6 R. Boudon (sous la direction de), *Traité de sociologie*, Paris, Puf, 1992.

broad sense, is that being scientific means 1) eliminating from explanation any unobservable elements and 2) analysing the regularities in the relations between observable elements. These two principles are present in the *Rules* as well as in *Suicide*. They have been very influential. They can be detected behind Mach's empiriocriticism in physics, behind behaviourism in psychology, behind functionalism in Malinovski's style of anthropology, behind structuralism in Lévi-Strauss' style of anthropology, etc. Now, this positivist definition of science entails high costs. For this reason, it has been more or less abandoned by all disciplines: today, empiriocriticism, behaviourism, Malinovskian functionalism or Lévi-Straussian structuralism are all, if not dead, at least regarded as highly controversial, to say the least. All these variants of positivism propose a dogmatic definition of science, which has nothing to do with science as it is. All sciences introduce unobservable elements, and have to do so if they want to be fruitful. Consider, to take a trivial example, the "forces" of the physicists. Though unobservable, they are indispensable. The same situation occurs in sociology. Since collective phenomena are effectively the outcome of the behaviour of individuals, explaining individual behaviour is necessarily a crucial moment in any genuine sociological explanation. Now, explaining individual behaviours, beliefs or actions is tantamount to finding out why individuals behaved the way they did or believed what they believed. But these reasons cannot be directly observed. They have to be reconstructed. A sociological theory that ignored these "reasons" would be just as pointless and even impossible as a physical theory which tried to ignore "forces". In the two cases, the theory would in fact not be explanatory, but merely descriptive.

Borlandi: *It seems to me that we need to include alongside your rejection of Durkheim's explicit method that of the language in which he expressed it. In L'analyse mathématique des faits sociaux you attributed the "inappropriate" and also "fuzzy" lexicon of the Rules to a theoretical and obsolete scheme of references, that of the determinist thought of Mill. In La logique du social you warned of the danger of literal (i. e. realist) assimilation of Durkheim's "vague" concepts such as that of society, collective consciousness or social cause. In the Dictionnaire you rejected on the grounds of their fuzziness almost all the keywords in The Division of Labour in Society as well as in Suicide. Your reservations regarding Durkheim's lexicon are much more evenly balanced, I would say, because if there was one goal Durkheim had set specifically in the Rules it was that of providing a rigorous terminology to counteract the "commonly held notions" used in the sociology of his time. How can we persuade ourselves that Durkheim's lexicon hinders and even masks his real method? And how is it that Durkheim is so betrayed by his own vocabulary that it has to be got rid of in order to grasp what is worthwhile in his sociology?*

Boudon: Durkheim has a naive view of definitions. He thinks a notion needs only to be defined to be accepted in science. This is wrong, even in the case of the hard sciences. A word like "cause" cannot be defined, but it is indispensable to most scientists. Thus, many words cannot be defined and are still normally used in scientific language. Durkheim does not see at all, moreover, that, beside the classical Aristotelian form of definition, there are others, such as the definition which was later to be christened "deictic". His naive view on the question of definitions led Durkheim to propose controversial definitions of many notions. Consider his definition of "religion". It is presented as ... "Durkheim's definition of religion" rather than as "the correct definition of religion". In other words, it is not accepted, except by hard-core Durkheimians. The difficulties raised by notions such as "anomie" or "egoism" can be accounted for by Durkheim's theory of definition: he introduces fuzzy concepts, but as he has a traditional view of definitions, he tries to join them with definitions of the classical type.

Borlandi: *However, you do agree that the duality you advanced is singular. Here we have an individual who does the opposite of what he says almost without being aware of it and who, what's more, describes what he does (or rather what he does not do) with words that cannot be trusted. Naturally, the question I am bringing up is, in its turn, one of method and not of substance. In my opinion, every reading of Durkheim, or of anyone for that matter, is legitimate (to state the obvious) provided the criteria that direct it are clear. I would like you to specify the rules you have adopted so far in your approach to Durkheim. Is there, in Pareto, Weber, Simmel or Marx a discrepancy or a similar discrepancy between the method declared and that implemented? And if there is and even if there is not, why? I think I understand (from the last chapter of Effets pervers et ordre social⁷ and the Dictionnaire) that you share John Elster's view that Marx too worked as an individualist, despite the statements in the Preface and the Postscript in Das Kapital and in the Einleitung of 1857. But what about the other three authors I have mentioned? Is it conceivable that, let's say, in fifty years' time somebody will come up with the idea that Raymond Boudon denied in his empirical works the methods he formally professed? And if you consider this a very unlikely eventuality, why?*

Boudon: My method is simply the method called in French the "*explication de texte*". I read what Durkheim wrote on questions as to what definitions are, what science is, what sociology should be, what principles should be used when building scientific theories, etc. Then, I compared these statements to

7 Paris, Puf, 1977 (*The Unintended Consequences of Social Action*, London, The Macmillan Press Ltd., 1982).

what he actually did. Thus, he contended in his methodological pages that the reasons of action should not be considered (*positivisme oblige*), but in many of his empirical analyses, in *Suicide* notably,⁸ he reconstructed these reasons. He was perhaps not totally unaware of the contradiction. He is very prolix in many discussions. By contrast, the moments of his analyses where he tries to “understand” the reasons why e. g. bachelors should be less “protected” against suicide than married people, why women commit suicide less frequently than men, why in periods of economic boom people commit suicide more frequently, etc. are written in a very allusive fashion. He knew that the variable bachelor/married can be held as an indicator of egoism because bachelors are in a situation where they are more exposed to disappointment, say. But, as he saw this moment of “understanding” as “psychological” and had decided to exclude any psychology from sociology, the only way of getting away from the contradiction was to be allusive at this point of the analysis. There is no reason why the same contradiction would appear in the case of Weber. When one reads his methodological writings, one has immediately the feeling that they express the method he actually used currently in his sociological writings. I would say the same thing of Simmel. The case of Marx is more complicated, notably because he played several roles. He wanted to be a scientist, but also a political leader, a prophet, a polemicist, etc. When he played the role of a scientist, as a careful reader of the Scottish philosophers notably, he understood well the interest of individualistic methods. His fulmination against Proudhon illustrates his scientific repulsion against holism. But as a polemicist and prophet, he understood well the interest of describing the course of history as led by anonymous forces. Fairy tales can be rhetorically more adequate than scientific theories. These remarks are of course mere conjectures. As far as I am concerned, I became conscious of the scientific importance of methodological individualism once I instinctively used it in an article in the quantitative historical sociology of judiciary institutions, “Les mécanismes sociaux des abandons de poursuite”.⁹ My ideas on the subject crystallised then with my *L'inégalité des chances*.¹⁰ I tried to develop a theory on them in *La logique du social*, where I wrongly gave the impression that the “reasons” of the actors should always be analysed along utilitarian lines. This was never my assumption.

Borlandi: *Turning once more to the Rules, whose centenary is the occasion for this interview, you have nevertheless saved some of this book or the theses which it supports. In L'analyse mathématique des faits sociaux you justified*

8 R. Boudon, „European Sociology: the Identity Lost?“, in B. Nedelmann and P. Sztompka (eds), *Sociology in Europe. In Search of Identity*, New York-Berlin, de Gruyter, 1993, 27–44.

9 With A. Davidovitch, *L'année sociologique*, 3rd series, 1964, 111–244.

10 Paris, Colin, 1973 (*Education, Opportunity and Social Inequality*, New York, Wiley, 1974).

*the use of “faits sociaux” in the title by referring back to the Rules. L'analyse empirique de la causalité¹¹ contained an extract of the first chapter of the Rules. In the article entitled “Durkheim” written for the Encyclopaedia Universalis¹² you acknowledged that Durkheim's comparative method (which he also called “indirect experimentation”, chapter VI of the Rules) had a certain advantage over Weber's immediate comprehension. The former seemed preferable in those cases where – and you maintained they were numerous – “social facts appear unclear to the intelligence”. It is true that your attitude on Weber and, similarly, on Durkheim has changed since then, or at least that is my impression (we shall speak of this later on); but I find once again the argument of the imperviousness of certain facts to individualistic procedures, and therefore a partial rehabilitation of the method of the Rules (the theorised method), in *Traité de sociologie* (article “Action”). “Il arrive souvent en effet – you say (p. 51) – que le principe de l'individualisme méthodologique ne puisse être appliqué en pratique [...]. Ce cas de figure se produit lorsque les causes individuelles responsables du phénomène agrégé que l'on souhaite expliquer sont trop nombreuses et trop hétéroclites pour être identifiées et décrites”. This case is the one well illustrated in *Suicide*, and you find yourself defending Durkheim's inductive approach from Jack Douglas' criticism claiming that suicide (but really the rate of suicide) is explained through reasons and not through causes. Am I mistaken?*

Boudon: No, you are not mistaken. In some cases, as in the case of suicide, where heterogeneous individual reasons are responsible for aggregate data, maybe it is legitimate to introduce fuzzy causes as “anomie” or “egoism”. But with the individualistic methodology in mind, one could go further. So, if we want to explain, say, why suicide is greater in context A than B, we would try to collect monographs on suicide in the two contexts, ask which types of suicide are more numerous here than there. We would discover for instance that as an outcome of some factors, the feeling of despair, the sense that there is no future, etc. are more frequent in A for some types of people. In practice, of course, some individual reasons for aggregate data can be more difficult than others to reconstruct. It should always remain an objective, though. Douglas is right: “anomie” is not a genuine cause. It is above all, I would add, a stenographic expression of our ignorance.

Borlandi: *Let's talk of Durkheim's real or implicit method, that which you have made explicit. How would you define it?*

11 R. Boudon et P. F. Lazarsfeld (sous la direction de), *Méthodes de la sociologie: II. L'analyse empirique de la causalité*, Paris-La Haye, Mouton, 1966.

12 Vol. 7, 1970, 843–846.

Boudon: I would not put it that way. I do not contend that I have made Durkheim's "real" methodology explicit. I mean only that, in many occasions, Durkheim appears to be aware of the fact that building a sociological theory implies reconstructing the reasons as to why, say, people behave differently in different contexts. Now, again, as the idea of "reconstructing the reasons" goes against his positivistic stance, he minimises this moment.

Borlandi: *A chronological inventory of all you have taken from Durkheim sees first and foremost a "logic of causal inference". This was in L'analyse mathématique des faits sociaux and, immediately following this, in Les méthodes en sociologie.¹³ In short, you maintained that in Suicide Durkheim was a pioneer of multivariate causal analysis.*

Boudon: Yes. Durkheim's main contribution to sociological methodology probably lies in the fact that he anticipated multivariate analysis, or to put it more generally, that he well understood that causal inference from correlations has to be conducted in a careful fashion, using partial correlations as a way of controlling the causal meaning of a global correlation.

Borlandi: *It is, on the other hand, in A quoi sert la notion de structure?¹⁴ that, I believe, you started to expound the idea that the statistic regularities Durkheim wanted to account for by going beyond the motives of individuals can and must be explained in terms of the effects of composition of individual actions (reasoned and reasonable). Taken up once again in L'inégalité des chances etc., this idea becomes central in your work from the time of La logique du social. I have often asked myself, and now ask you, where does the polemical part of this idea end – polemics regarding the "oversocialised conception of man" in all its variants, old and new – and where does the one strictly concerning method begin, creating, in other words, new analytical procedures. What difference does it make if you see the individual as a "passive" object (moulded by social forces that are greater than him) or if you make him an "active" subject when you are dealing with, as Durkheim was, aggregate figures, such as suicide rates provided in relation to rates of alcoholism or the average density of families, or, for example, with the votes the socialist party might obtain in Lyon and Marseilles analysed according to one or the other variable expressing the socio-professional characteristics of the populations of these two cities?*

Boudon: My motivation in defending methodological individualism is not at all polemical. It derives from the fact that this type of methodology leads to a

13 Paris, Puf, 1969.

14 Paris, Gallimard, 1968 (*The Uses of Structuralism*, London, Heinemann, 1971).

more powerful style of research than others: it is more successful in terms of creating additional knowledge. Tocqueville or Weber were able to solve many puzzles in a convincing way partly because they were committed to this methodology. This success was rather obvious reasons: in this methodology, "understanding" (the actions, beliefs, etc. of the actors) is an essential moment. Now, the reasons of the actors are objectively the genuine causes of their actions, decisions, etc. and the collective results they produce. So this methodology aims at discovering the true causes of the phenomena sociology explores. I should add, against the positivist tradition I described earlier, that the individualistic methodology is by no means contradictory with the general ideal of any science: aiming at objectivity. The fact that the "reasons" of the actors have to be reconstructed does not imply that this reconstruction is a mere matter of inspiration. On the contrary, this reconstruction has to proceed along the principles that guide the construction of any scientific theory. Thus, the reconstructed reasons have to be compatible with all observed facts. As to your remark that explaining aggregate data or correlations would be incompatible with methodological individualism, I do not agree. The great methodological individualists, such as Tocqueville or Weber, effectively succeeded in explaining aggregate differences by reasons (e. g. when Tocqueville explains why Frenchmen at the end of the 18th century feel atheism appealing, believe in political planning, see the Tradition as bad, etc., while the Englishmen do not; or when Weber explains why the Mithra cult is more appealing to the Roman civil servants than to other categories in the Roman society or why the Americans are more religious than the Germans or the Frenchmen). As I said earlier, the studies on suicide would be more interesting with the individualistic methodology in mind: they would go much further. The fact is that, as the importance of this methodology is ill-perceived, many studies appear as being stuck at a descriptive level and are content with introducing verbal causes as "anomie" or "egoism". That this "Durkheimian" style is widespread in current sociology does not mean it is the best one. Let me take a concrete example to illustrate this point: many studies show that "irrational" beliefs (for instance in the existence of extraterrestrial beings) are the more frequent the higher the level of education. What seems to you the best of three possible attitudes: registering the correlation without attempting at seeing what lies behind, introducing a holistic cause in the Durkheimian style (e. g. the "credulogenic effects of education"), or trying to disentangle the reasons why people believe in extraterrestrial beings when they are more educated? I have proposed in *L'art de se persuader* a hypothesis on this point. The individualistic methodology is so unfamiliar (and misunderstood) and the causal holistic approach so familiar to some sociologists that they do not even think seriously about finding out the reasons of the people behind the aggregate data.

Borlandi: *Durkheim's explanation of the rise and progress of the division of labour is what has always seemed to you the easiest to incorporate in the canons of individualistic sociology.*

Boudon: Yes. I would say *cum grano salis* perhaps because Durkheim is here very close to Spencer, methodologically at least, even though he has always wanted to minimise his debt to the British sociologist.

Borlandi: *Coincidentally, or perhaps not, you have drawn from Durkheim a theory of individual and collective frustration, which you have put alongside a "law" of Tocqueville's, and which interprets the way, or one of the ways, you understand the notion of anomie. Philippe Besnard has written that you are the only sociologist in France to have kept alive the theme of anomie (besides himself of course), and when all is considered, in a way that accepts Durkheim's orthodoxy.*

Boudon: My analysis on frustration can effectively be interpreted as giving an analytical content in a particular context to Durkheim's notion of anomie.

Borlandi: *It is with The Elementary forms of the Religious Life that you have ultimately put yourself to the test: a theory of the flag or of flag worship in L'idéologie;¹⁵ a theory of magic, one of the genesis of the notion of the soul and one of religion (of its origins) in L'art de se persuader. Going back to the reasons/causes dichotomy, Durkheim's theories of magic and religion would seem to be in opposition. Durkheim explains magic beliefs through reasons (good reasons): the type of explanation you propose. He then would seem to explain religious beliefs through causes (causes that are neither reasons nor passions). Moreover, in Durkheim's wake, you have established a contiguity between beliefs in magic and in science (or at least some of them).*

Boudon: Yes, that's more or less how things went.

Borlandi: *I must confess that I had some difficulty in tracing in the Elementary Forms the passages that would justify your Durkheimian theory of magic. I can't be the first to raise this objection because in the Traité (article "Connaissance") on the subject of this very theory, you hasten to point out that your perspective is "epistemological" and not "doxographical". This perspective would consist in asking what Durkheim really wanted to explain and in taking this further along the same line he had started from, rather than to linger on what he "really thought", this being a question which you regard as "à la fois insoluble et d'un intérêt douteux" (p. 511). If we are dealing with the interest factor, I agree with you. The job of the sociologist is different from*

15 Paris, Fayard, 1986 (*The Analysis of Ideology*, London, Polity Press, 1989).

that of the historian of sociology, and this difference lies precisely in the fact that what interests the former does not (necessarily) interest the latter. Ultimately, that is what Merton says in "On the History and Systematics of Sociological Theory" (although I am aware of your preference for the history of sociology from a cognitive point of view). But why must the "real" thought of Durkheim – or, more generally, that of Pareto or Tönnies – be declared unapprehensible on principle? Is this not the same as admitting that you can make any author say what you want? I am asking you this because I have read a recent article of yours, published in Italy,¹⁶ in which you take objection, even vigorous objection, to the "post-modern conception which states that the meaning of any text is always the creation of the reader", whereas once, "literary criticism was based on the assumption that it was possible to objectively discuss what was the best interpretation". Even taking esoteric poetry, you state that "it can be demonstrated" that one interpretation is more correct than another.¹⁷ Why can't that which is valid for literary texts not be valid for those of Durkheim – or Pareto or Tönnies? Or have you had second thoughts about the doxographical perspective?

Boudon: The analysis I have presented on magical beliefs is, to my mind, a presentation of what Durkheim could possibly have written if he had gone to the end of some of his important intuitions (as I read them) in *Elementary Forms*. These pages have to be read as "variations on some themes by Durkheim" and not as a reconstruction of "what Durkheim really meant". My objective in these variations was to settle a methodological and theoretical problem ("how should magical beliefs be accounted for?"), not to write a piece on the history of sociology. Why this interest in magic? It is because these beliefs are methodologically particularly challenging. My variations draw from the passages where Durkheim focuses on the hypothesis that the natural sciences and modern technology are derived from religion and magic¹⁸, and on the point that "primitive thought" should not be seen as following different rules from modern thought.¹⁹ I was impressed by the fact that the assumptions sketched by Durkheim are contradictory both with Lévy-Bruhl's and with, say, Beattie's assumptions, more acceptable in themselves, and also more compatible with data. I am ready to make more explicit the adjective "*insoluble*" in the quotation you mention: I do not contend it would be impossible to reconstruct the thought of a writer. But you will perhaps recognise that it is easier to reconstruct

16 „Lo scetticismo dei postmoderni”, *Biblioteca della libertà*, 29 (125), 1994, 25–52.

17 *ibidem*, 31.

18 E. g. *Les formes élémentaires de la vie religieuse*, 319–320.

19 E. g. *ibidem*, 340 ff.

a particular analysis proposed by a sociologist on a given point or the meaning of a poem, than “the thought” of a sociologist or a poet.

Borlandi: *I have mentioned before that your attitude to Durkheim changed at a certain point, if I am not mistaken. Up to and including La Crise de la sociologie, you quote from Durkheim much more frequently than you do from Weber. Following this, a rival “cohabitation” between the two is established in your works. A rivalry which only becomes greater when other thinkers are added, the last being Simmel, on Weber’s and not on Durkheim’s side. There is no “Durkheimian paradigm” – in other words there is no recognised Durkheim specificity – in the last chapter of Effets pervers et ordre social. Under the influence of Piaget’s concept of “réalisme totalitaire” and of Bourricaud’s criticism of “sociologisme” (an article from 1975), you dilute Durkheim’s statements in those universal ones of sociological determinism. On the contrary, La logique du social revives Durkheim, but only provided he is understood as Harry Alpert proposed in 1939, which is as a “relational realist”. In this book you do not hesitate to write that between Durkheim and Weber there is a “fundamental agreement” that the individual actor must be considered “the logical atom of sociological analysis”. The Dictionnaire (entry “Durkheim”) and La place du désordre²⁰ undertake the task of re-establishing the distances since it seems to you indisputable that Durkheim chose not to acknowledge the actor or his subjectivity. Durkheim and Weber are once again in step in L’art de se persuader for the way, common to both, in which they explain beliefs. But here, in the Traité (article “Action”), there are two pages on “the intellectual differences dividing Durkheim and Weber” which you say must not be ignored in order to avoid the syncretism which Parsons fell foul of when he unified their conceptions of sociology. If I were to sum up in one single picture the place occupied by references to Durkheim in your books, I would speak of a continuous falling into disgrace followed by a regaining of esteem. But I would also suggest that the Durkheim-Weber conflict reflects a much more important one, one which – at least in the last two decades – you have engaged in with Durkheim and his heritage. Is this so? In the Preface of the new edition of La logique du social (“Du sociologisme à la sociologie”, 1983) you admit that perhaps you overdid it when you “annexed” Durkheim to the tradition of methodological individualism. Can we turn things round and say that ultimately Durkheim has resisted your annexation attempts?*

Boudon: My objective has never been to annex Durkheim, but to ask: is it possible to read him in a way that would make him acceptable given the objectives and constraints of a scientific approach to social phenomena

20 Paris, Puf, 1984 (*Theories of Social Change: a Critical Appraisal*, London, Basil Blackwell/ Polity Press, 1986).

(explaining them with theories meeting the normal criteria of objectivity and scientificity)? In this quest, I found in his work passages which could be easily retranslated and made acceptable. I have never gone as far as to contend that all passages could be translated into acceptable theories. At any rate, my objective in my discussions on Durkheim and others was never Nietzschean (annexing), but Kantian (criticising): my aim was to see more clearly through these discussions why a sociological analysis was valid, acceptable or not. By an "acceptable theory", I mean a theory of which statements and consequences are both acceptable in themselves and congruent with known phenomena. Here is an example (among many possible others) of statements which I find unacceptable: "[...] elle [la société] poursuit des fins qui lui sont [...] spéciales; [...] elle réclame impérieusement notre concours. Elle exige que [...]".²¹

Borlandi: *In your article "Action" in the Traité, at the end of the paragraph on the origins of individualistic sociology, that which conceptualises, tracing back to the forerunners, the opposing sociologies of Durkheim and Weber, you state that there was "toutes les raisons pour que la sociologie française soit davantage attirée par une perspective holiste, la sociologie allemande par une perspective individualiste sur les sociétés" (p. 26). This paragraph and this statement, which I did not read immediately on the appearance of the Traité, took me unawares. I was under the impression that the historical use of the holism-individualism dichotomy, a use which I note is not marginal in France, was an effect which was unwanted (by you, yet nevertheless not opposed by you) following the success of your analytical use of this selfsame dichotomy. While the analytical use would present holism and individualism as typical methods (paradigms) that relate to human behaviour and explain it, with the statements of Durkheim and Weber used simply by way of exemplification, the historical use of the holism-individualism dichotomy aims at – judging by the results at least – rewriting the history or, better still, the geography of European sociology as follows: French sociology is by constitution holist and Durkheimian, that of Germany individualistic and Weberian. It is clear that they are two very different matters. It is one thing to ask how sociological theory, in all its varieties, can be systematised. It is a completely different thing to speculate on the almost endemic characteristics of two national sociologies. In the former case we are dealing with concepts. In the second with facts; and the two things have not been confused. Now I must reconsider because not only have you legitimised the historical use of the holism-individualism dichotomy, but you have made the results your own: holism is French, individualism is German. No matter how I try, I cannot see what facts these theses are based on. If in France we are dealing with Durkheimian holism, this was the monopoly*

21 *Les formes élémentaires*, 295.

of a limited group between the two world wars. Their viewpoints were anything but generally accepted (the most radical criticism of Suicide was made by Halbwachs) and very few traces of them have survived except for Mauss. If I am not mistaken, since Davy's retirement there are no university Durkheimians left. What position does Durkheimian holism hold in French sociology today if the French sociologist who refers most to Durkheim in his works is yourself? As far as Germany is concerned, Weber was but one (and if we add Simmel, we have two) of the 49 sociologists registered from 1909, the founding date of the German Sociological Society up to 1934, when it folded. This Society was full of holists, who made up the majority, so much so that Weber left it almost immediately. An individualistic and Weberian sociology in the post-war years? No more so than an Italian or Swedish one. I would, if possible, like to discuss this topic.

Boudon: Weber and Simmel define sociology as they see it in an intellectual context very different from the French one. In France, positivism is very strong at the end of the 19th century. The Comtian classification of sciences is perceived as an evident truth by many people: sociology being the coronation of the edifice of science, with biology, chemistry, physics, etc. occupying the lower storeys. This Comtian classification of science is the cognitive frame within which Durkheim defines his sociology. It implied among other consequences that history, economics and psychology are not genuine sciences, since there was no room for them in Comte's classification. In Germany, the intellectual climate is very differently structured at the same time. Comte had much less influence in Germany than in France or England, say, possibly because the Germans had a high ranking "functional substitute" of Comte in the person of Hegel. Moreover, at the time of Weber and Simmel, Kant had made a successful comeback and relegated Hegel to the back of the stage. On the other hand, Ranke, Mommsen and others had created a tradition of research in history much closer to the scientific type of history we practise today than, say, Michelet's admirable but lyrical and holistic history. Archaeology, philology, experimental psychology with Wundt, etc. had developed into scientific disciplines in Germany. In the same linguistic area, Austrian economists had developed a novel formalised type of economic theory that impressed Simmel and Weber. It was based on an "ideal-typical" psychology. On the whole, the Germans of the end of the 19th century could not plausibly feel attracted by the idea that history, psychology or economics would be non-sciences. They could not buy the narrow type of positivism which Durkheim inherited from Comte, according to which sociology should aim at discovering laws in the style of Boyle's law. There is one issue in the *Rules* on which Durkheim really differs with Comte: when he makes the point that sociological

“laws” should be of the “conditional” type (I am using here our terminology, not his). Can the history of sociology disregard the history of science and the history of ideas in particular, and history in general? As to general history, Nicolet has shown that positivists have played a crucial role in the foundation of the Third Republic.²² Littré and Laffitte, who considered themselves the natural heirs of Comte, had a tremendous influence on the French political elites of the time. Nicolet stresses explicitly that Littré had a holistic view of societies. One of my students from Japan has shown that Littré must be seen as the missing link between Comte and Durkheim. It is impossible to understand Durkheim's sociology, he contends convincingly, without taking Littré into consideration, and he examines the way Durkheim administered Comte's heritage as well as his tremendous political influence.²³ At any rate, all this story has no equivalent in Germany. The fact that the holistic tradition is “endemic” in France was already rightly noted by Raymond Aron, when he evokes the continuity from Marx to Weber and the other continuity from Comte to Durkheim, from Durkheim to Mauss, and from Mauss to Lévi-Strauss.²⁴ Like Durkheim, Lévi-Strauss defines science by the two principles I mentioned earlier. Structuralism is a program the objective of which is to analyse the relations between observable aggregate data without considering the actors and their reasons. Is structuralism not a typically French product? When I taught in Harvard in the middle seventies, I was imprudent enough to announce a seminar on structuralism. I attracted a crowd of students, in the first meeting at least: they wanted to learn about this strange thing which was being talked about so much and which nobody really understood. Durkheim also had an influence through historians, notably those belonging to the *Annales* school. Its two founders were admirers of Durkheim and of his most orthodox followers, such as Simiand. In a discussion with Seignobos, Simiand advised the historians to get rid of what he called two “idols”, namely the “individual” and the “chronology”. Louis Dumont's holism is another striking example of holism: to him, India, Modernity, the Western world, etc. are described in a substantialist fashion, as Durkheim's “society”. Foucault's *Les mots et les choses* or *Surveiller et punir* would be other examples among many of ill-tempered holism. You are right, German sociology is not more individualistic today than Italian or Swedish sociology. Beside the misunderstandings surrounding it, one main reason for the underdevelopment of the individualistic methodology is that it is

22 C. Nicolet, *L'idée républicaine en France*, Paris, Gallimard, 1982.

23 M. Yamashita, *La sociologie française entre Auguste Comte et Emile Durkheim: le conflit entre la science et la morale*, Paris, Sorbonne, 1993; and the article extracted from this thesis, „La sociologie française entre Auguste Comte et Emile Durkheim: Emile Littré et ses collaborateurs”, *L'Année sociologique*, vol. 45, n° 1, 1995, 83–115.

24 R. Aron, *Les étapes de la pensée sociologique*, Paris, Gallimard, 1967, 15.

difficult to practise. It is easier to explain, say, differential phenomena by occult social forces than by making it the aggregate of what may be heterogeneous reasons. But I maintain that the way the German classical sociologists saw their discipline was very different from Durkheim's for historical reasons that are pretty easy to disentangle.

Borlandi: *Your pronouncements on Durkheim's posterity are frequent yet scattered. For example, in the Dictionnaire (entry "Durkheim") you wrote that "Durkheim a été appelé à la rescousse lorsque le structuralisme et le néo-marxisme [...] se sont trouvés disqualifiés vers la fin des années 60" (p. 193). What events would you say are particularly emblematic of Durkheim's reception in contemporary sociology?*

Boudon: One of my students has recently written an interesting dissertation on the "uses of Nietzsche" in the French social sciences.²⁵ The next path-breaking book on Durkheim could possibly be on the "uses of Durkheim" rather than on Durkheim himself.

Borlandi: *I mentioned earlier some of the sources of your interpretation of Durkheim: Alpert, Piaget, etc. But you can provide others; Lazarsfeld and the "Columbia School", first and foremost. Any recollections of attitudes to Durkheim in French culture in the Fifties and Sixties are welcome.*

Boudon: Yes, I became interested in Durkheim notably through the paper on *Suicide* by Hanan Selvin, a now deceased friend from my Columbia days. Durkheim was at that time (early sixties) almost forgotten in France. I even wonder whether his books were still in print then. I recall Louis Althusser telling me, at the end of my years at the Ecole Normale, that for him, Durkheim was a totally obsolete and unreadable writer: a current opinion then. As far as I am concerned, as soon as I went further in my study of Durkheim, I almost immediately became exasperated by the fuzziness of the concepts, the holism, the narrow view of science, etc. while still admiring a number of intuitions and empirical analyses.

Borlandi: *You spoke of Raymond Aron. We could perhaps wind up with him. I have found in his Mémoires (1983) comments about Durkheim that do not differ greatly from yours. Yet none of his judgements on Durkheim are as scathing as those he expressed about Marx: "équivoque et inépuisable".²⁶ Can we apply this definition to Durkheim? Inexhaustible certainly. But equivocal?*

25 A. Staszak, *Les usages de Nietzsche dans les sciences sociales en France; étude sur la diffusion du nietzschéisme de 1889 à 1993*, Paris, Sorbonne, 1994.

26 R. Aron, *Marxismes imaginaires*, Paris, Gallimard, 1970, 355.

Boudon: I agree to conclude this interview with Aron's somewhat rhetorical formula, though I think Durkheim is more easily exhausted, more controversial and also less our contemporary than a Tocqueville or a Simmel.

(Original English; edited by John Bisk, Weinbourg, France)

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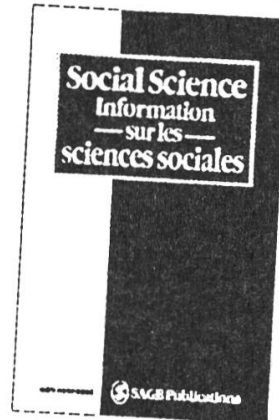
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