

# Why I became a sociologist\*

Raymond Boudon



## Abstract

In this autobiographic work, Raymond Boudon reviews his trajectory from his beginning as a student at the *École Normale Supérieure* to the last stages of his career. Boudon describes his main intellectual influences and concerns throughout his life and how they were displayed in his works.

**Keywords:** Raymond Boudon; sociology; methodological individualism; education; social mobility; objectivity; relativism; rationality.

## Resumen. *¿Por qué me convertí en sociólogo?*

En este trabajo autobiográfico, Raymond Boudon pasa revista a su trayectoria, desde sus inicios como estudiante de la *École Normale Supérieure* hasta la última etapa de su carrera. Boudon describe cuáles fueron sus principales influencias e inquietudes intelectuales a lo largo de su vida y cómo éstas se acabaron plasmando en sus obras.

**Palabras clave:** Raymond Boudon; sociología; individualismo metodológico; educación; movilidad social; objetividad; relativismo; racionalidad.

## Summary

|                               |                           |
|-------------------------------|---------------------------|
| Lehrjahre                     | Moral Feelings and Values |
| Early Works                   | Rationality               |
| Education and Social Mobility | References                |
| Ideas and Beliefs             |                           |

\* This article was first published in: CHERKAOUI, Mohamed & HAMILTON, Peter (eds) (2009). *Raymond Boudon: A Life in Sociology. Essays in Honour of Raymond Boudon*, Vol. I. Oxford: Bardwell Press, pp. 53–75. Reprinted with permission from The Bardwell Press.

## Lehrjahre

As the saying goes, psychologists become psychologists because they have problems with themselves, anthropologists because they have problems with the world, sociologists because they have problems with their society. This is not true in my case. I have the feeling I did what I wanted to do. I had the privilege of not being directly involved in any war, either in my own case or through my family. I had a direct and rapid career. I was appointed professor at the Sorbonne at the age of 33. France has not been in the best of shapes for years or even decades because its governments have done too little to correct the negative effects of centralization and the cult of the State. But I experienced the *Trente glorieuses* and I have always enjoyed the French *art de vivre*. So does my wife. I share with her a deep intellectual, moral and political complicity. She was born in Thüringen in Eastern Germany, and had to flee with her family to Bavaria in order to avoid the Soviet army. She studied law in Munich and taught German in a French college after we married. Thanks to her, I have the feeling that I enjoy a binocular view of French society and as a result practice comparative sociology all day long.

My wife's father was a doctor. After the War, he managed to obtain the indispensable certification that he was never involved in Nazism from the East German authorities and was allowed to practice his job for some months, though exclusively in the Soviet zone, until he too fled to Western Germany to join his family. As to my parents, both came from families of modest craftsmen. My father's passion was music. He played oboe in an orchestra in his youth. I admired his ability to read an orchestra score fluently. He reached a moderately senior position in a big Parisian commercial firm and gave his family a comfortable standard of living. He disliked the communists as deeply as the Nazis. Hitler's *Mon combat* (*Mein Kampf*) and Kravtchenko's *J'ai choisi la liberté* (*Choose Freedom*) were the books that had made the greatest impression on him. My mother was an excellent cook. So is my wife: her reputation as a cook is well known among our friends.

After my secondary school studies, I gained entrance to the *École Normale Supérieure* in 1954. I was proud of this success and benefited from the advantages provided by the institution. I obtained a grant that led me to Freiburg in Breisgau for one year, where I have the great opportunity to listen to Martin Heidegger, although I was not greatly impressed by his course on *Der Satz vom Grund*. I had the distinct impression that he was playing with words and observed that he was held in greater reverence by the many students who came from Latin America or Iran than the German students. So I remained faithful to my earlier philosophical masters, Kant and Hegel. But, in the fifties, most French professional philosophers spent their time more or less exclusively on discussions of classical philosophers. I wanted to orient myself more towards a discipline dealing with the concrete human world. Economics attracted me, more because it seemed to be the most rigorous of the human sciences, than by the topics it dealt with. Psychology appeared to me as artificial in its experimental version and verbose in its clinical version. I was always inter-

ested in history, but never liked the discipline, exactly for the same reasons as, I learned later, Bronislaw Malinowski: because he found human history too dismal, especially that of his native Poland, he created a discipline which dealt with societies but ignored the sound and fury of history. His “functionalism” erased the image of history being a tale told by an idiot and replaced it by the study of the rationality and mutual complementarity of institutions. The history of both World Wars had given me the feeling that history is dark and that I would run the risk of becoming depressed if I tried to become a historian: so many wrong decisions and ideas had led to catastrophes which could have been easily avoided, when considered *a posteriori* at least. The French Revolution seemed to me to have been quite barbarian in its final phase. I regretted that France had been made to suffer so many political convulsions since the Revolution. Thus sociology and economics remained the only possible choices as the outcome of this exclusion process. Sociology attracted me more, in principle at least, because of the broader field it aimed at covering and also because I saw it as a modern and hence more attractive version of philosophy. It also dealt with values, beliefs, ideas, institutions, though in a more concrete way. But I was unconvinced by the books published under the label of sociology in France in the late 1950s, for I found them too rhetorical. I saw economics as narrow, but solid because of its use of mathematics. My mentor Raymond Aron, whom I consulted on my difficulties in choosing between economics and sociology, told me: “you should choose sociology: for a young man, there is more potential in sociology than economics”.

I was easily convinced, since I saw that my lasting interest in philosophical questions was more easily satisfied by sociology than by economics. While browsing among sociological books, I had found Paul Lazarsfeld’s and Morris Rosenberg’s *Language of Social Research*. This book gave me the impression that the type of sociology it advocated was much more scientific than the laborious and boring classifications produced by the great sociological star of the fifties and early sixties in France: Georges Gurvitch. At my request, Raymond Aron recommended me for a grant to study in an American University and I opted for Columbia University in New York, attracted by the prestige of Robert Merton and Paul Lazarsfeld. My wife and I spent an unforgettable year there. At that time, few French intellectuals went to the United States. Many of them were close to the Communist party or at least sympathetic to its ideas. They saw the United States as the Empire of Evil. But thanks to our stay in New York, we discovered the great gulf between American and European Universities, in terms of budget, organization, diversity, facilities for the students, dynamism, and also rejection of rhetoric. As to the sociology that had developed around Lazarsfeld and Merton, it seemed to me that it was inspired by the scientific *ethos*.

### Early Works

I came back from the States with a project for my doctoral dissertation: as economics had seemingly become more scientific as it became more math-

ematical, I decided to combine my interest in sociology and economics by trying to clarify the question of the uses of mathematics in sociology. I see my dissertation now as much too broad, and original only to a very limited extent. It brought little new, but helped me in seeing clearly that mathematics could only have a limited impact on sociology. One chapter alone was original: the one where I used a very simple simulation model to explain statistical data in the field of the sociology of law. The proportion of cases that were abandoned rather than sent to a court had according to the statistical data been regularly growing since the beginning of the 19th century. Why? Gabriel Tarde had asked a similar question about another trend: why had the proportion of trials ending in a verdict of not guilty regularly decreased over the long term? As with Tarde, I tried to make the trend an outcome of the strategy developed by the actors of the judicial system in order to be seen as successful by their peers. This exercise convinced me of two things: firstly, that macrophenomena should be explained as the effects of individual behaviours; and secondly, that a central sociological problem is consequently to find out the reasons and motivations of individual actors. All my later works are elaborations of these basic insights. I did not know then that Max Weber and Joseph Schumpeter had christened this approach *methodological individualism*.

The academic regulations in France at that time insisted that candidates to the *doctorat d'Etat* had to present a second dissertation, on a subject different from the subject of the main one. After discussing the matter, in particular with Paul Lazarsfeld, I decided to work on structuralism. Under the influence of Claude Lévi-Strauss, structuralism had become popular at that time. Structuralism was born in the field of phonetics. The core idea of structural phonetics was that the phonemes of a language constitute a system of sounds aiming at using a minimal set of elementary distinct sounds to make the communication of any message as unambiguous and economical as possible. The idea of structuralism is clear and distinct concerning phonetics, less so concerning the more complex dimensions of linguistics, such as grammar, rather less so concerning anthropology, and even less so in the analysis of literary texts. French structuralists succeeded though only for a while in convincing a number of professionals in the human sciences that structuralism was a method able to make all human sciences, from anthropology and sociology to grammar or even literary criticism for the first time genuinely scientific and moreover to unify them. Previous decades had seen Marxists create the fallacy that so-called scientific materialism could unify and make all human sciences scientific, from economics to literary criticism. This fallacy was slowly dissipated and replaced by the structuralist fallacy. The new fallacy endured until the early years of the 21st century, long after it was discredited in academic circles, because structuralist ideas were diffused from one generation to the next by college and secondary school teachers. I was convinced that these ideas were wrong and started wondering why false ideas were so easily introduced to the market by brilliant writers.

My monograph on structuralism (*À quoi sert la notion de structure? Essai sur la signification de la notion de structure dans les sciences humaine*, 1968) was well received in Britain, as Duncan MacRae's preface to the English translation (*The Uses of Structuralism*, 1971) shows, and in the US, where George Homans informed me that "at last somebody is telling the truth in France about structuralism". The book was translated into German and several other languages. But MacRae saw rightly that it was "un-French". Hence my unpopularity — which was going to last a while — among many rank-and-file French sociologists. I had shown much too early that structuralism was a dead-end, and one which moreover had the effect of discrediting the far more serious and fruitful orientations which had been developed in the social sciences in the past.

### Education and Social Mobility

After this critical work on structuralism, I wanted to deal with a challenging sociological question. I always believed that a good educational system and a high collective level of education is the key to progress and to collective success as well as a condition for the development of democracy and human freedom. The early sixties were characterized by a massive expansion within all Western educational systems. This was a form of progress. Democratization raised the general educational level of the population, but had little or no effect on the "equality of opportunity": the correlation between social origins and educational level, as well as the correlation between orientation status — the status of the orientation family — and destination status — the social status of the subject — was hardly reduced by the democratization of the educational system. Moreover, the inertia created by the inequality of opportunity affected all Western countries. So the topic was attractive for several reasons: this inertia was a stain on the image of democracies, since, while inequalities can be justified particularly when they are functional, inequality of opportunity cannot. Moreover, given the political and social interest in the subject, a huge body of statistical data was available. Thirdly, the problem was intellectually challenging: why was there such inertia? Fourthly, it gave me the opportunity of testing my twin ideas about how macrophenomena should be analysed as the aggregated effects of individual actions and individual actions as the effects of understandable motivations and reasons. Fifthly, the explanations then available on the market seemed to me deeply unsatisfactory. I considered that Pierre Bourdieu's explanation was rhetorical: he explained in a pedantic and tortuous style which evoked in my mind Molière's *Précieuses ridicules* that the situation was as it was because it could not be otherwise. Bourdieu and Passeron had sent the manuscript of their *Reproduction* to my friend François Bourricaud. As he told me, his first impression was that their parody of Spinoza's deductive pseudo-mathematical style was a joke or "hoax" typical of those known as *canulars*, which were traditionally in favour among the students of the École Normale Supérieure. It was not a hoax. The authors had thought that presenting their nebulous ideas in a pedantic fashion was a good strategy. My own

ideal was rather the opposite: to say complicated things as simply and clearly as possible, as Jean Cazeneuve was to state humorously in the speech he gave in 1991 at the occasion of my election to the *Académie des sciences morales et politiques*. But my main objection to Bourdieu's so-called reproduction theory was that it was fatalistic and useless from a political viewpoint.

My own theory of the inequality of opportunity proposed by contrast a practical way of lowering the inequality of opportunity. I diagnosed that reinforcing the evaluation of pupils and students, above all diversifying the educational system, insisting on the main function of schools, i.e. the transmission of knowledge, should reduce the inequality of opportunity. Amongst several other studies, a German article by V. Müller-Benedict using data drawn from the PISA study, has recently provided another confirmation of my views (*Kölner Zeitschrift für Soziologie*, dec. 2007: 615-38). Needless to say, the policy direction which my work recommended was hard to follow for political reasons in the political climate where it was published and I had few illusions about this, given that the intellectual climate of the 1960s and the two following decades was well impregnated by Rousseau's ideas on education: the child should enjoy the school, discover mathematical theorems and grammatical rules by himself, choose his values freely. Teachers were no longer allowed to teach. They could only assist children modestly in their discovery of the world. They were not allowed to evaluate the performance of the pupils.

The theory I had developed in my *Education, Equality and Social Opportunity* (first published as *L'inégalité des chances* in 1973) started from the simple idea that the educational and social ambitions of children and teenagers had their parameters set by their social milieu. For instance a person coming from a family of successful lawyers would in normal circumstances perceive the prospect of becoming a low level clerk as a social demotion, while a person from a modest workers family would see the same prospect as a success. I was proud to see that, once this hypothesis and others in the same vein were modelised, they reproduced correctly — though in a rough way — a considerable number of aggregated macrosociological data. My theory explained in particular the inertia of the level of educational and social opportunity. So, straightforward psychological assumptions, once properly formalized, were able to explain the statistical data available on the relations between educational level and social origins, as well as many other forms of data.

Many scientists in Britain, Scandinavia and the US, and some in France, such as Raymond Aron and my other French mentor, Jean Stœtzl, recognized the relevance of these ideas. Stœtzl had introduced opinion polls in France before the 1939-45 war, established the *Institut Français d'Opinion Publique* and was very active in the development of empirical sociology in France after the War. Stein Rokkan, a leading sociologist from Norway, organized a brilliant symposium on my book which produced a number of important contributions which were published in *Social science information*. A paper by Tom Fararo mathematized the first part of my simulation model. It remains a classic. Others swore exclusively by standard statistical methods. They saw *variables*

and not people as the units of sociological analysis and considered — in line with a long lasting positivistic tradition — that one should not be concerned with what people have in mind and why they do what they do, at least when it comes to scientific analysis. So, they rejected my analyses as moving heretically away from the authorized methods. I think that I had shown that *methodological individualism* was a much more natural and powerful approach to the analysis of social facts than multivariate analysis. The former enjoys an *explanatory*, the latter a mere *descriptive* power. On the whole, my approach attacked a tacit dogma and was perceived as a threat by *statistical zealots*, as Paul Lazarsfeld called them. Lazarsfeld had introduced multivariate analysis to social scientific circles through a seminal article in his *The Language of social research* and was later to inspire its sophisticated versions, such as the now fashionable log-linear analysis. But he was at the same time deeply unhappy with the mechanical methods collected under the label *data analysis*. He had received a solid scientific education, and as a result he saw clearly that *data analysis* and *explanation* are two widely different ideas. So, he welcomed my work without hesitation and told me, with his famous Viennese Jewish humour, that I had shown the Promised Land to him. In general terms my work had met with strong interest among sociologists at the international level. But in France, it also met with strong opposition from the self appointed experts in education. There was an interesting if familiar effect of such success: as Jean-Michel Morin (2007), Michel Dubois (2000) and Michel Vautier (2002) have written, it defined my scientific image: for many social scientists, I became identified as the author of this book alone.

## Ideas and Beliefs

For reasons easy to understand — given the general Rousseau-esque intellectual climate that I have described above — my ideas on education had to wait before they influenced educational politics in France. In fact they never had much of an influence in political circles. Rather they emerged independently within the political sphere — but not before the last decade of the 20th century — as one effect of the reaction against the patent failure of the Rousseau-esque theories which had prevailed in the previous decades. It became more and more evident that, in conjunction with other societal factors, these ideas had produced under-education, anomie, under-employment and school violence. But as I despaired of the evolution of educational systems, which had sacrificed the traditional functions of education for the sake of enhancing the equality of opportunity, although they actually had not succeeded in raising it at all, I decided to turn to another topic: ideologies. This with the basic idea in mind that it would be better to understand why people endorse false ideas than to work on the great ideologies, such as Nazism or communism. Nazism had disappeared. Many countries were ruled by Communist parties. But it was easy to see that communist ideology was disappearing. Moreover, I saw the *great* ideologies as subjects for historians rather than social scientists, for

their implantation cannot be explained without taking all kinds of contingencies into account. As to *small* ideologies they will never disappear and are a normal component of societies, such as those which influenced in particular the politics of education and many other aspects of politics as well, such as the cult of the State or the cult of centralization, both which I saw as powerful brakes on the modernization of France. I considered these *small* ideologies to be a major sociological topic.

With his *Opium des intellectuels* (1955) Raymond Aron produced a brilliant and welcome essay on ideologies, but his contribution to their explanation was limited and had little to add to the question of why people, and in particular intellectuals and politicians, embrace false or suspect ideas so readily. Vilfredo Pareto in particular had been much more creative on the subject in his theory of *derivations*. I left aside the irrational side of the question: we have always known that passions and interests are apt to generate biased views of the world. I wondered instead whether false ideas are more likely to be generated by the normal operation of our cognitive capacities. This led me to raise the basic question as to how and why we become convinced that a theory or a statement is true or false. I started from an assumption directly opposed to Pareto's. He contrasted the true ideas derived from sound "logico-experimental" procedures to the "non-logical" ideas caused essentially, he maintained, by unconscious forces operating in the minds of people. I always felt deeply uncomfortable with the notion of unconscious forces and instead began from the viewpoint that beliefs, false and fragile ideas are generated by the same cognitive processes as those which generate true ideas. This conjecture was implicitly contained in Pareto's sarcastic statement that the history of science is a graveyard of false ideas which have been accepted for a while under the authority of scientists. Now, nobody would maintain that the numerous false ideas proposed by scientists in the past and also in the present are exclusively the result of unconscious affective, cultural and social forces. They were not produced by passions and interests either. Why should the many false ideas produced in ordinary life be the product of such forces? I felt deeply uncomfortable with such assumptions, because the existence of the hidden forces in question could only be confirmed through the effects they were supposed to produce. I saw such circular explanations as rhetorical rather than scientific.

In order to explore these questions in the light of empirical data, I used several approaches. I turned to cognitive social psychology because this discipline had established through experiment that human intuition could be deeply unreliable. I re-analysed data from this discipline and was able to show through many examples that the false answers given by subjects to the cognitive traps they were exposed to by experimenters can actually be explained as the effect of a strategy of cognitive *muddling through*. I showed in other words that, in order to explain failures of intuition, it was not necessary to assume the existence of hypothetical hidden forces, e.g. that the human brain might be wired in the wrong way as the result of some deviant evolutionary process, as some researchers have proposed. I tried to generalize the strategic interpre-



tation proposed by Daniel Kahnemann of the cognitive *biases* revealed by his experiments.

I realized then that such questions about the origins of false beliefs could be clarified not only by the experiments of cognitive social psychology, but also by sociology and anthropology. Many anthropologists and sociologists see false beliefs as explainable by the action of the hidden forces of socialisation. Subjects will accept what we see as superstitions or doubtful ideas because they have been exposed to them in their childhood, and because everybody around them accepts them as true.

Against these facile explanations, I discovered to my great satisfaction that Alexis de Tocqueville, Max Weber and even Émile Durkheim had instead put forward a rational interpretation of beliefs the observer may automatically consider to be irrational. For Weber, magical beliefs — the canonical example of the beliefs most likely to be considered as the irrational effect of hidden social forces — are actually rational. People accept them because they are grounded on an interpretation of the world, which in many of its aspects appears to them as credible and compatible with the real world, and because this interpretation has no serious competitor in their eyes. In the same fashion, Durkheim sees magical beliefs as rational. He goes as far as to say that the *primitive* — as the members of traditional societies were called in the 19th century — uses the same cognitive strategies as modern scientists. They dislike contradictions between their beliefs and the facts they observe and they try, like modern scientists, to develop auxiliary hypotheses to explain these contradictions. In the same way as modern Westerners, they ground many of their beliefs on correlations. These correlations may eventually prove spurious. As they practice rain dances, for instance, in the periods when rain is more likely to fall, they are more likely to observe a correlation between the rituals being practised and rain falling. But modern Westerners do the same. Even scientists base their beliefs on spurious correlations quite frequently. It was long thought, on the basis of spurious correlations that stress is the cause of stomach ulcers, until it was shown that it is more likely of bacterial origin.

As with magical rituals, scientific truths are currently artificially protected against scepticism and criticism by various strategies. For instance, according to an authoritative monograph on the subject, it was long considered an uncontroversial truth that bees have their own language: through their dances they were able to inform their sisters about locations where pollen is available. A systematic analysis of the scientific meetings where these questions were debated revealed however that many entomologists thought that bees are guided, like most other insects, by chemical stimuli rather than by the dance of their sisters (Wenner & Wells 1990). But the assumption that bees have a language was of course much more attractive. This hypothesis was triumphant for a while because those who were against it were not invited to the meetings where this type of question was discussed. Similarly, Lysenkoism was made credible by strategies also used in normal science. The difference is that it was

protected against criticism by the Soviet State itself, a State with powerful resources of social control.

A general assumption then could be formed, in opposition to the conceptions widely held among cognitive psychologists, sociologists and anthropologists, that in fact ordinary and methodical thinking differ from one another only in degree rather than nature. The *primitive* are no more irrational than modern Westerners. The common man struggling with a question he is not familiar with uses the same cognitive strategies as scientists, only in a less methodical way. Differences in what people know or don't know explain the differences in what they believe rather than highly hypothetical differences in the rules of inference they use, differences that would be due themselves to highly hypothetical unconscious forces.

The difference between superstitions and scientific beliefs derives from the fact that they are produced in different contexts. In a context where the laws of the transformation of energy are unknown, no difference can be detected between fire-making and rain-making. Because they do not know the laws of the transformation of energy, the *primitive* do not see any difference between the two practices and treat them as effective because they are based in their mind in the will of some spiritual forces. By contrast, to the Western observer who knows these laws fire-making appears as rational, i.e. as grounded in established laws, while rain-making appears to him as objectively groundless and for this reason objectively ineffective.

In general, spontaneously irrational explanations of beliefs should in most cases be replaced by explanations showing that these beliefs are grounded in intelligible reasons. By irrational explanations I mean those which see impersonal social, cultural, psychological or biological causes as the causes of these beliefs, instead of seeing the *reasons* people have to believe what they believe as the genuine *causes* of their beliefs. At the same time, it should be recognized that different contexts can produce different reasons. In a context where the notion of the *laws of nature* is taken for granted, unexpected and unexplainable phenomena are perceived as *miracles* by some people or as illusory phenomena by other people. In a context where the notion of the laws of nature has no significance to anybody, events can be unexpected and unexplainable, and still be perceived as normal and arouse no real surprise. To the people of the historical Middle East, miracles were an unsurprising and to this extent *normal* event because they had understandable reasons to think of them as such.

From the 1960s and even now, the *avant-garde* in the sociology of science espoused the idea that scientific beliefs cannot be considered to be objectively grounded. The "new" sociologists of science maintained that science rests on undemonstrated and non-demonstrable assumptions; that it is made of conceptual elements produced by the human mind; that human minds are moulded by the social context. Some of these arguments are true, at least in part. But they do not imply the *relativistic* conclusion the new sociologists of science drew from them. In his provocative style, Paul Feyerabend (1975) stated that the scientific vision of the world is a *fairy tale*. Following

his lead, constructivism became the ultimate truth in relation to knowledge and beliefs. As constructivism described truth as *constructed*, the distinction between grounded representations of the world and objectively groundless beliefs disappeared. The very notion of objectivity became meaningless.

This relativistic message is very far from my own views on beliefs and knowledge. While a conviction can have its parameters set by context, as when the ignorance of the laws of transformation of energy makes it possible for a person to believe that fire-making and rain-making are both produced by the interventions of spiritual forces, it can also be rationally discussed by an outside observer belonging to another cognitive context. As already mentioned, once the laws of the transformation of energy have been discovered and verified, the technique of fire-making can be rightly considered as using real natural forces, while this is not the case for techniques of rain-making. Against relativism, the views of the Western observer on the efficiency of the two types of techniques are *objectively* better grounded than those of the *primitive*. So, the relativistic message contained in the “new” sociology of science is groundless.

Being critical — in the Kantian sense — toward the “new sociology of science” seems to me very important, not only from a philosophical or sociological viewpoint, but from a political one as well. If scientific truths were the mere product of convention and construction, moral and political truths should *a fortiori* be treated as objectively groundless conventions and constructions. It seemed to me at this point that a leading cause of the political and moral disarray which characterizes many modern Western societies is the theory of knowledge and beliefs which has been developed and legitimated over the last four or five decades by the social and human sciences more generally. If scientific truths are mere conventions, why would, for instance, the idea that democracy is a better political regime than others be objectively grounded? For a number of years I have been worried, not only by the development of undemocratic practices and public decisions in democratic societies, and the many laws adopted in France recently which violate the principle of the freedom of expression, but also by the fact that, as a consequence of a growing relativism especially among intellectual and political elites, a new wave of criticism against democracy is developing among conservative intellectuals and politicians on both the left and the right. We experienced the Marxist phase and its criticism of so-called *formal democracy*, the Fascist phase which derided parliament as a *Quasselbude* (Gossip-shop), the libertarian phase of the nineteen sixties with its motto that *anything goes* and that all institutions are repressive. Now, we have the idea that democracy is just one sort of regime among others, and that it generates all kind of evils. We also have the idea that the belief in human, political and social progress has been discredited by the horrors of the 20th century, that the notions of truth and objectivity are illusions, and that the notion of the public interest merely conceals private interests.

Maybe this is the point to say that my interest in education, my great disappointment with the educational policies practised for many years in France and

elsewhere, my interest in beliefs and values were probably rooted in strong convictions based on my admiration for the philosophers of the Enlightenment, and especially for Voltaire and Kant. Both maintained that the general interest is threatened above all by false ideas. Voltaire, Kant, Tocqueville and Weber all believed that ideas are at least as important as interests for explaining social and political phenomena — and perhaps even more so. For this reason I never felt very receptive to Marx, Nietzsche and Freud, for these giants seemed to me to have clay feet. What they have in common is a belief in ideas as *dependent variables*: as the effects of unconscious social, psychological, cultural or biological forces. Needless to say, I felt even less receptive to those colleagues who took their inspiration from some vulgarized version of Marxism, Nietzscheism or Freudism and I never tried to hide it.

Finally, this theoretical reflection on the explanation of beliefs, from the false beliefs generated by the experiments of cognitive socio-psychology to the beliefs recorded by anthropologists and sociologists convinced me that the principles of *methodological individualism* were a valid method for explaining not merely statistical data of the type I had met in my work on education and mobility, but also other types of data, and especially those dealing with collective beliefs. The topic seemed highly important. I was pleased to discover that in his *Elementary Forms of the Religious Life*, Durkheim had defined collective beliefs as the main topic for sociology to explore.

### Moral Feelings and Values

My ideas on the origin of beliefs attracted some attention for, alongside *L'inégalité des chances*, my book on *Le Juste et le Vrai* is mentioned in the *Petit Larousse*, an age-old venerable dictionary much used in France in schools and at home, notably to help crossword addicts to solve their puzzles. This was the starting point for developing my ideas on knowledge, beliefs, moral feelings and values.

I raised two questions in the book: one was about the origins of our *representational beliefs*, the other about our *normative beliefs*. I have just noted that the relativistic message sent by the social and human sciences in many of its publications has probably had highly negative and lasting political and social effects. Relativism ensured that many teachers no longer knew what to teach, and how to teach it, that youngsters and adults no longer knew what to think about many subjects. It ensured that social and political life was pictured as being just a confrontation of interests, and that the notion of the *general interest* was seen as a fallacy whose function was to cover up the interests of classes and corporations. It ensured that fundamental principles such the freedom of expression were violated in Europe, while others were violated in the USA after 9/11. Fortunately, the relativistic message of the social sciences is fundamentally wrong. It rests on dubious theories of knowledge and of norms.

In looking at normative and axiological beliefs, I started, (as I had with representational beliefs) by examining a basic question. Why does an individual

belonging to some context in the broadest sense of the word accept or reject any given normative or axiological belief? I started from this basic but difficult question because I had the strong feeling that, as in the case of representational beliefs, there was a lot of confusion in the field of normative and axiological beliefs. Philosophers remained mostly Kantian or neo-Kantian, while sociologists seemed more inspired by the Marxian and Nietzschean traditions. The Kantian tradition was able to explain why we accept *general* normative statements. Even before Kant, Voltaire's answer to Pascal, who doubted whether stable and objectively grounded rules can inspire normative behaviour, was that there is a single powerful rule, "followed by all nations". Do not do to others what you would not like others doing to you.

But the universe of normative and axiological feelings and beliefs is far from being exhausted by such general rules. We spend a good part of our life evaluating things, behaviours, institutions and more generally many kinds of situations. Social action is continually motivated by these evaluations. I started once again from the idea that by scrutinizing how the simplest among the myriads of prosaic evaluations we accept are grounded, we could shed some light on my basic question about normative and axiological beliefs. In a non-systematic fashion, as I had done in the case of representational beliefs, I explored a number of experimental data and theoretical explanations of evaluative data in order to answer my two questions: why do we accept or reject a given normative statement? Why and how can consensus emerge on normative issues?

During my research on this, I found that one of the most illuminating parts of Adam Smith's *Wealth of Nations* is where he wonders why his contemporaries seem to take it as self-evident that some occupations should be higher or lower paid than others. For instance he wondered why people in 18th century Britain took it as self-evident that miners should be paid more than soldiers. His answer was that this evaluative feeling is the conclusion of an implicit system of reasons containing widely accepted principles and factual uncontroversial statements. I saw the implicit theory contained in the particular analyses presented by Smith as proposing *in ovo* a general theory of normative and evaluative feelings and beliefs. I tried to develop this theory, to make it analytical and to apply it to various data, such as the empirical data I had collected. I reached the conclusion that normative and evaluative feelings and beliefs should be analysed as deriving from the systems of reasons that social actors accept more or less implicitly because they are unable to perceive a serious competing system of reasons which appear to them to be equally valid. Of course social actors are in many cases unable to arrive at such a convincing system of reasons. This is true of normative and evaluative beliefs as well as of representational beliefs.

I used these theoretical ideas to explain all kinds of phenomena and in particular to analyse a body of data I extracted from the Inglehart *et al* (1998) survey on *World Values*. I could see that on many normative questions, the English, French, Germans, Italians, Norwegians, Americans and Canadians who had been sampled gave converging answers and that the variations in their

answers were highly structured as a function in particular of age and educational level. I attempted then to explain the statistical structures characterizing the data by making them the outcome of systems of reasons in the mind of the sampled individuals. The strategy was on the whole the same as the one I had used in my work on education and mobility. My ambition had been again to transcend the *descriptive* level and to try to reach the *explanatory* level, in a context where the data used were much more raw than in my earlier study. I concluded from my analysis that many features of the data could be explained by a *rationalization* effect, in Max Weber's sense of the term. Thus, from one generation to the next, the sampled individuals displayed a more rational view of morals, religion, authority and of many other issues. These findings reveal one of the main functions of the social sciences: showing that long term trends are at work although they seem contradicted by the contingencies operating in the short term. *Rationalization processes* are threatened or thwarted by *historical forces*, i.e. by unfavourable conjunctures, stated Max Weber. The 9/11 events and the consequences they have generated have produced the impression that God was back in the Western World and that the teachings of the Enlightenment were forgotten. This impression was further reinforced by the success of the Evangelicals around the world, notably in those parts of the world where human misery, injustice or daily difficulties affect people most severely. In China itself, the government seems to have rediscovered that religion is a useful opium for the people and displays an increasing tolerance toward the many Christian or Taoist sects which are proliferating. These hard facts do not invalidate the *rationalization theory*. There is no chance that the so-called theory of intelligent design would really be accepted in the West, except by a minority of naïve believers and by the few politicians who take the idea seriously that fundamentalism can only be defeated by another form of fundamentalism.

In other writings, I have tried to show that the theory outlined by Adam Smith about feelings concerning the wages of different occupations was also sketched out by Max Weber in his widely discussed though controversial concept of *axiological rationality*. The most important in my view are the passages where he claims that social action always involves the two dimensions of instrumental and axiological rationality, and states that, although the two dimensions are always present in any actual social action, they should be considered as conceptually distinct from one another. While these works have given birth to a lasting flow of comments, with some going as far as to claim that the notion of axiological rationality is meaningless, I tried to make them analytical.

I must confess that I am surprised that theoretical notions such as these are seldom seriously discussed in the contemporary sociological literature, although they are crucial. My guess is that this state of the art results from the fact that the social and human sciences often accept the undesirable *naturalistic* principle that, as in the physical world, material and efficient causes — often called *structural* — are the only ones worth consideration in a genuinely scientific explanation. It is true that the natural sciences became scientific from the moment they substituted mechanical for final causes in their explanation of

natural phenomena. But people have intentions, desires and are able to evaluate. These features belong to their reality. Their intentions, reasons, values, preferences, goals are *facts*, even though they have to be indirectly observed or reconstructed. Human actions are not determined by social context, they are based on reasons whose parameters are set by context. Ignoring this hard fact is to doom oneself to *unrealism*. Now, how can an explanation be both scientific and unrealistic without being contradictory? It seems to me that the widely accepted failure of positivism in all its variants lies in this confusion between *realism* and *materialism*. The two notions are indistinct in the case of natural, but not of human phenomena. A failure to grasp this point is responsible for the decline of all the approaches which, like behaviourism, structuralism and the other variants of positivism, rest on the principle that human behaviour should be explained by some material causes or forces of cultural, social, psychological or biological origin rather than by reasons and motivations, as I have tried to show in my discussion with Jean-Pierre Changeux and Vincent Descombes (Bronner 2009) which for obscure reasons will remain unpublished.

## Rationality

My theoretical interests naturally led me to reflect on the notion of rationality. Weber's *axiological rationality* is widely rejected by contemporary social scientists. Rationality is generally considered as exclusively instrumental (choosing the right means to reach one's goals). To Bertrand Russell (1954: viii) e.g., "*Reason* has a perfectly clear and precise meaning. It signifies the choice of the right means to an end that you wish to achieve. It has nothing whatever to do with the choice of ends". To Herbert Simon (1983: 7–8), "*Reason* is fully instrumental. It cannot tell us where to go; at best it can tell us how to get there. It is a gun for hire that can be employed in the service of any goals we have, good or bad". A consequence of this widely shared view is that the goals, ends, and values of social actors are either taken as mere facts that are worth being registered rather than explained, or explained by irrational causes, as the socialisation effects familiar to sociologists, the obscure psychological forces evoked by Freudians or the hypothetical biological forces referred to by socio-biologists. Being aware of the uncomfortable character of this situation, my friend James Coleman, another student of Lazarsfeld and Merton, proposed to apply the basic principles of economics to sociology, notably its instrumental view of rationality. This gave birth to so-called *Rational Choice Theory*.

The proposal was in part a wise one. Rational Choice Theory had been implicitly used with some success, long before it was given this name, to explain a number of problems concerned with politics, social movements, ideology and many others of interest to the field of sociology. As an obvious example I would simply refer to Mancur Olson's *Theory of collective action*. Nobody working in the field of social and political mobilization could ignore it, even if they propose to revise it in some fashion. But there are also many social facts that Rational Choice Theory is unable to explain for the obvious

reason that, as it has practically nothing to say on *normative, evaluative and representational beliefs* and on the *goals* of social actors, it has also practically nothing to say on social phenomena including normative, evaluative and representational beliefs and goals, whose explanation is not trivial matter. Now, a goal such as “staying alive” is trivial, but a goal such as “becoming a pianist” is not. Such a belief as “it is good to look to the right and left before crossing a street” is trivial; but not the belief that rain dances are an efficient means to help rain falling.

The success of RCT was understandable. It offered a solution to a widely recognized problem among contemporary sociologists: the problem of the identity — of the backbone — of sociology. At this point, I came to the conclusion that Rational Choice Theory employed a wrong theory of rationality. To be more precise it operates with an overly narrow conception of rationality. In other words, it is better thought of as a special case of a more general theory. I tried to show that this theory could be defined in an analytically acceptable fashion and applied to the explanation of a wide range of data. In doing this, I had the feeling I was merely elaborating on some implicit insights that were already present in a many great sociological works, past and present.

I must, somewhat immodestly, confess that I do not have the impression that the theory of rationality I have developed and proposed has yet received the attention it deserves. My Theory of Ordinary Rationality could, it seems to me, provide a backbone for the social sciences (Boudon 2009): a backbone with a basically cognitive orientation (Hamlin 2002). But I also have the feeling that this situation can be easily explained. The success of the social sciences, the fact that they are consulted on all kinds of questions today, has the consequence that it is much more rewarding for a social scientist, say, to produce reliable data on hot topics such as discrimination or poverty than to spend time on strategic but austere and difficult theoretical questions. Moreover, the same success has led to the development of more or less closed “corporations” among social scientists. These corporations are organized along a variety of dimensions. Some are defined by the paradigm they follow. Some by the goal they pursue: explanation of puzzling phenomena, collection of reliable data, but also political, cultural or social militancy. Many are concerned mainly with producing descriptive data on issues, such as elections or consumption, on which they aim at being recognized as experts. This heterogeneous character of the social sciences is widely recognized today. It explains why *sociological theory* and *general sociology* have practically disappeared. Not long ago, a German sociologist concluded from the present state of the social sciences that their true essence was revealed by this. They could not be a genuine science, despite Durkheim’s or Weber’s naive ambition, as he saw it. They are instead a type of *third culture*. But this culture is unfortunately of the *neither-nor* type: neither art nor science. It seems to me that, rather than be satisfied by the present state of the art and to christen and bless it, it is more fruitful to wonder whether this state of the art is really satisfactory, whether it optimises the production of new knowledge and finally whether it contributes to the enlightenment of



social and political actors and of citizens. Without a backbone or a grammar providing a discipline with a positive identity, it cannot be taught nor expect to be really cumulative.

As far as I am concerned, I deeply endorse Durkheim's statement that the main goal and social usefulness of sociology and the main service it can offer to society is to produce genuinely valid new knowledge on social phenomena. The tragedies which have covered and continue to cover the world and the persistence of strong inequalities in democratic societies ensure that social scientists often prefer militancy to the creation of new knowledge, while the complexity of the modern world inspires in others the idea that describing as honestly and reliably as possible the events occurring in some corner of the planet or in some dimension of the various economic, political and social activities is the only reasonable objective the social sciences can pursue.

I endorse Weber's view as well, one also shared by most classical philosophers, that men have in common a basic good sense. Albert Einstein (1936) maintained rightly that "Science is nothing more than a refinement of our everyday thinking". In the absence of this assumption many valuable ideas become empty. The idea of democracy has no meaning if it is not supposed, in agreement with the philosophers of the Enlightenment and the theorists of classical liberal democracy, that citizens forge their normative and representational beliefs on the basis of their good sense in every case where their opinions are not biased by their passions and interests (Boudon 2007). Now, many topics exclude such biases. If one prefers the irrational view of men to the rational view developed by the best philosophers and sociologists, politics becomes a mere confrontation between incompatible interests, democracy is an empty word and it is impossible to explain why, beyond the hocus-pocus of history, significant trends can be identified, such as the abolition of the death penalty in a growing number of countries. This trend is due to the fact that ideas tend in the long run to be rationally selected by the good sense of citizens, in other words by Adam Smith's *impartial spectator*. Contemporary impartial spectators no longer debate the death penalty because they recognize, in Europe at least, that it ought to be abolished everywhere for objective reasons: it is cruel, inefficient as a means of dissuasion and irreversible in case of wrong judiciary decisions. These remarks led me to propose, following in particular from Weber's and Durkheim's lead, a neo-Darwinian theory of social and political evolution where the role of mutations is fulfilled by mental innovations and the role of natural selection by rational selection.

Many political and social, as well as representational ideas, appear effectively in the long run as rationally selected. This selection supposes that men are guided in the long term by their good sense rather than by the hypothetical hidden forces so easily used by the contemporary social sciences. I see my own ideas in this respect as more beautifully expressed than I could by a quotation from Tocqueville's *Souvenirs*: "the future, enlightened and impartial judge, but who, alas, comes always too late" (*L'avenir, juge éclairé et impartial, mais qui arrive, hélas, toujours trop tard*).

A final note. *Zwei Seelen wohnen, Ach! in meiner Brust* (Goethe): “I have two souls, oh! in my breast.” I worked hard because I wanted my writings to be as clear and uncontroversial as possible and for this reason rewrote many of my articles several times in order to achieve an illusory perfection. But I also appreciate not just the French *art de vivre*, as I mentioned at the beginning, but the *art de vivre* shortly. I must confess that I prefer many things to work: taking walks along the sea shore, fossil hunting, fishing, listening to my favourite composers, reading books and newspapers or sitting in *cafés*. So, the length of my list of publications results, not from my zeal at work, but from the fact that I have for many years been invited to many conferences taking place in fascinating places I wanted to experience, in Europe, America or Asia, and that I had to pay for this pleasure by writing a paper. In fact, none of my articles except the first one, a popularization of Lazarsfeld’s *latent class analysis*, was written spontaneously. All are the products of these temptations. As to my books, they also testify to my basic laziness: for most of them are collections of articles drawn from these papers.

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