CODA

CROSS-CUTTING FIELDS AND SOCIAL CIRCLES

A Conversation with Mohamed Cherkaoui

Mohamed Cherkaoui
Interviewed by Gianluca Manzo and Peter Hamilton

By focusing on specific topics from particular disciplinary viewpoints, the essays contained in this book have shown how Mohamed Cherkaoui's intellectual path evolved by travelling through a variety of research fields and institutional contexts. In this closing chapter, we aim to provide the reader with an overall assessment of this social and scientific journey via his own account of the route his intellectual career has taken.

With this aim, we asked Mohamed Cherkaoui to provide us with detailed responses to a number of questions touching upon: (1) the years of his intellectual training; (2) his transition from education to work; (3) his early work in the field of the sociology of education; (4) his relation with quantitative methods; (5) his contribution to the sociology of social mobility; (6) his analysis of the classics of sociology; (7) his

The interview's structure and content were designed by Gianluca Manzo. Peter Hamilton translated Mohamed Cherkaoui's replies from French.

epistemological reflections; (8) his more recent studies on Moroccan society; (9) his editorial activities; (10) the reception of his work; and (11) his contribution to the management of research in France and elsewhere.

Through his patient replies to our questions, Mohamed Cherkaoui delivers, in addition to many personal details, a penetrating, critical analysis of the last fifty years of French sociology and its relationships with the international intellectual community.

EARLY INTELLECTUAL TRAINING

Let us start from the very beginning of your intellectual trajectory. Can you tell us about your education at the secondary level and why finally you decided to enrol in a French university?

I began my secondary education in a Moroccan *lycée* in Casablanca before joining the French *lycée* in Rabat where I studied for the baccalaureate exams. Because I wanted to study architecture at Darmstadt in Germany, I had already taken some steps in this direction during my last year in high school. I was accepted at this school on condition that I would take a German course at the Goethe Institute for a semester.

After obtaining my baccalaureate, I applied to the person in charge of scholarships at the Moroccan Ministry of Education, a small Frenchman whose name I have forgotten. After considering my file he decided this was an inappropriate choice. This person, who had control over French and Moroccan scholarships required that I should enrol in a preparatory class for the entrance examination to the Ecole Normale Supérieure if I wanted to obtain a scholarship. He could not understand why somebody who had obtained his baccalaureate with such good marks in the humanities would want to give up *belles lettres*. When I told him it was probably too late to apply for a place in a Parisian *lycée*, he sent me away and told me to sort it out for myself. To be honest, I gave up my plans to study architecture without cynicism or bitterness because the friend who planned to accompany me gave it up for family reasons and went to work.

I wrote about this to my cousin who used to be the Moroccan ambassador in Paris and the friend of several ministers in the French

government, before later taking up posts in the government of Morocco. He was able to enrol me at the Lycée Henri IV thanks to his many contacts, whose extent and influence I only discovered much later when I accompanied him during his visits to Paris in the 1980s and 1990s after he had finally given up any role in government in 1967. It was during these years that I was fortunate to meet many French and foreign politicians visiting Paris who came to visit him and discuss the affairs of the time with him. I was thus a witness to discussions and decisions whose consequences I cannot relate out of courtesy to those people who still preside over their countries' destinies.

Thus I found myself in preparatory classes during 1965. It was a memorable experience. I was fortunate to have excellent teachers. The courses were like a continual intellectual firework display that fascinated the young provincial I was at that time. But soon I realised that the flowers of rhetoric were fading rapidly and that there are limits to any art or technique of composing combinatorial abstract concepts at which we must excel and which prepare us to make a brilliant presentation on any topic. It reminded me of the teaching of the great Sophists in ancient Greece who enchanted Plato. He had given them a central place in his *Dialogues* and had given them their roles to play and to make statements that are not far from his own theses. But he also criticised them especially because they were a threat to the search for truth. How could it be otherwise? Protagoras, for instance, was a great charismatic figure whose relativistic doctrine bewitched the mind. This is still the case today. All the relativists of past and present are the heirs of Protagoras. The famous sentence that "man is the measure of all things" actually means that there is no transcendental foundation of truth. This is the foundation of relativism and the expression of atheism that Plato perfectly understood and condemned. It is the beginning of the world disenchantment process theorised by Weber. This phrase echoes the famous *pensée* of the French philosopher Blaise Pascal that "the eternal silence of these infinite spaces terrifies me." I would add that it is also the beginning of the depopulation of the heavens.

Curiously, I later encountered the same mindset and the same virtuosity in some British politicians, alumni of Oxford, who I have met in recent years. I also noted that they share the same exceptional *esprit*

de corps as the alumni of great colleges and universities whether they be French, British or American.

In addition, the social life of the boarders at the *lycée* was gloomy and unattractive. The fierce competition between the students pushed them to do ridiculous things or even worse. The most disconcerting and the most disturbing thing was that most of my classmates did not care about how long they spent in preparatory classes. They gave me the impression that they were monks who had joined a regular order for which only eternity counted. This somewhat acosmic and timeless attitude is grandiose in itself.

At the same time, I was witnessing a paradox that I could not solve. As a sign probably of the time before the events of 1968 that would shake the foundations of French society, the majority of my friends were members of the Union of Communist and Trotskyist students, apart from two students affiliated to the SFIO (Section Française de l'Internationale Ouvrière) which later became the Socialist Party, two Gaullists, three members of the Union of Catholic students and some sceptics lost like me in this century in which doctrinal extremism was dominant. Some of them made up the generation of leftists of the Ecole Normale Supérieure in the early 1970s for whom the little red book of Chairman Mao became the gospel.

When I see young men who sacrifice years of their youth to cultivate excellence, and when I see the cream of the French elite that peopled the Ecole Normale Supérieure fall prey to such absurd and farcical nonsense, I do not know how to relate so much intelligence and ingenuity to such extravagance.

I am not far from thinking that it was the same thing for certain of my comrades as it was for French intellectuals of the eighteenth century, for the avant-garde and theorists of the French Revolution: as Tocqueville so aptly said of them, the unreality and eccentricity of their political projects can only be explained because they lived outside their own century, cut off from business and public administration, in which they took no part, unlike English intellectuals of the same period.

At the end of the year, I decided to take the examination for the propaedeutic certificate from the Sorbonne, and passed. At the beginning of the next academic year, I enrolled in philosophy and sociology,

two subjects that had common courses and qualifications. Very few students chose to combine philosophy and political science, law or economics. Following two distinct types of study simultaneously is not a French speciality. It is found in Germany and elsewhere.

Can you also tell us why you decided to receive your university training in these disciplines?

The choice of philosophy is easily explained: I had tasted the delights of the dialectic of virtuosos who were able to locate the new ideas of contemporary schools of thought, when structuralism and Marxism prevailed in France, into the great tradition of Western thought that they placed in a line going back to Plato and the Presocratics. This assuredly cultivates and sharpens a form of intelligence; but it often leads to the development of a superficial talent and to insularity.

The choice of sociology was almost self-evident. One of the two philosophy teachers at the *lycée* had started us reading the classic texts of the discipline that had opened up new paths for me. In addition, I felt the need to confront reality and not stay in this dangerous ethereal sky of ideas and combinations of philosophical concepts. Keeping young people away from the issues of their time was one of the pedagogical principles of Jesuit teachers, the masters of all that counted in France ever since the late sixteenth century. It was a little later that I discovered that many French sociologists and anthropologists had first received training in philosophy. This was the case for Durkheim, Mauss, Lévi-Strauss, Aron, Boudon, and Bourdieu.

On an intellectual and personal level, which French intellectuals and professors played a central role during this stage of your training?

At the Sorbonne, the two great tenors of the time in sociology were Raymond Aron with whom I was associated, albeit from afar, until his death, and Georges Gurvitch—who disappointed me because of the lack of attention he gave to his courses and the violence of his opposition to Lévi-Strauss, his competitor who taught at the Collège de France. However, in my opinion, one should remember the work of Gurvitch

for its beautiful reflection on the multiplicity of social time in the style of Halbwachs, and for his work in the sociology of law and morality that is in the direct line of the Durkheimian tradition, even though he was critical of the master of French sociology. He also helped us to discover Sorokin whom he frequently cited, especially to warn us against "quantophrenia". Gurvitch thus dangerously accentuated the allergy of students to quantitative methods. This was not Sorokin's aim, for although he warned against the worship of numbers, he used such methods frequently himself.

My central memory of Aron's teaching was his analytical ability that allowed him to understand events, to place them in their political and geopolitical context, and to see them in a way enlightened by sociological and political theories. I cannot forget his remarkable course on the sociology of international relations and his *Paix et guerre entre les nations (Peace and War between Nations)*—which prefigured his *Penser la Guerre. Clausewitz*—which opened a field of thought that I use in my current work in geopolitics. Much later, when I had the opportunity to attend Aron's seminar, I had the opportunity to meet the brightest Aronian intellectuals who have played and still play a central role in French political and intellectual life.

The Savage Mind by Claude Lévi-Strauss was one of my favourite books. This is a response to issues raised by the Kantian philosophical tradition that Durkheim's sociology inherits and claims to solve, and in particular *The Elementary Forms of Religious Life* and *Primitive Classification*. The book was also a response to criticism of the analytical approaches of the social sciences developed by Sartre in his *Critique of Dialectical Reason*.

Like many students, I tried to understand the lessons we were supposed to learn from the structuralism that dominated in France at the time and that was the great intellectual fashion alongside Marxism. I confess that I did not understand much of it despite my repeated efforts and my extensive reading that ranged over linguistics, anthropology and even extended to some mathematical formalisations of kinship. Some high priests of the two doctrines of French intellectual orthodoxy went so far as to attempt to provide a synthesis of these two hegemonic currents. Structuralism even engulfed psychoanalysis with the blessing of

its leading shaman, Jacques Lacan, the founder of the Freudian School of Paris whose seminar was attended by "all of Paris". But what remains today of these monumental but fragile edifices built by these architects of thought?

In philosophy, only a few historians and philosophers of science held my attention to the point where after my degree in philosophy, I prepared a dissertation for the *Diplôme d'Etudes Supérieures* (Diploma of Higher Education) on Descartes' *Géométrie* at the Institute of History of Science at the Sorbonne. By placing Descartes' treatise in its historical and mathematical context, and by taking into consideration the great Cartesian science project that has such a remarkable unity, I was discussing and putting myself at a distance from the thesis of Jules Vuillemin, a distinguished epistemologist and professor at the Collège de France who defended the purely algebraist point of view whilst I thought that it was about the invention of a new discipline of geometry. It is intriguing to note that it was this same Vuillemin who planned the path that led Michel Foucault to the Collège de France. It was partly thanks to these historians of science that I felt the need to go back to studying mathematics.

Just after this I had the chance to get a German scholarship that allowed me to learn German and take courses in several universities, in Konstanz am Bodensee, Tübingen, Frankfurt, and Göttingen. It was during this trip that I had planned to write a doctoral thesis in the history and philosophy of mathematics, particularly on the synthesis of Ernst Steinitz and his famous thesis on *Algebraische Theorie der Körper* (*Algebraic Theory of Fields*, 1910), that I had translated during my stay in Germany.

My trip also allowed me to discover the rich and fascinating German and especially Austrian literature of the turn of the nineteenth and twentieth century that I continue to read and reread today.

After two years in Germany, I returned to Paris where I had to make a definite choice about which path I should follow, the philosophy of mathematics or sociology.

The only two supervisors who taught history and philosophy of mathematics at the Institute for the History of Science were Suzanne Bachelard, daughter of the brilliant epistemologist Gaston Bachelard, and René Taton, but they lacked the intellectual breadth I was looking

for to definitively take that direction. Taton, whom I had seen on my return from Germany, was even kind enough to offer me a job as a researcher at the CNRS during the preparation of the doctoral thesis that I had discussed with him. But his approach to the history of mathematics, that I found linear and shallow, did not attract me. I almost went to work with I. Bernard Cohen at Harvard.

I think I needed a mentor, a personality on the scale of Alexandre Koyré, all of whose admirable books I had read. He fascinated me with the extent of his knowledge, the depth and clarity of his superb analyses of the history and philosophy of science. The stars of philosophy in France in the late 1960s, Dérrida and Foucault in particular, seemed to me superficial compared to Koyré, although I liked both of Foucault's doctoral theses on the *History of Madness* and the *Birth of the Clinic* and the early work of Dérrida on the origins of Husserl's geometry. *Les mots et les choses* and *L'archéologie du savoir* (*The Order of Things* and *The Archaeology of Knowledge*) were, in my view, superficial books written by the brilliant Foucault to impress and thus be elected to the Collège de France.

However, I remembered that I had browsed through the manuscript of Raymond Boudon's doctoral thesis on the mathematical analysis of social facts when I was a student at the Institute of History of Science from 1967 to 1968–9, thus just before I left for Germany. In addition, in this year, 1971, I learned by chance he was a professor at the University of Paris V. I decided to attend his courses and seminars which were at the time on the analysis of causal structures and social mobility, subjects on which he had produced two books.

I also discovered that, at the same university, there was a Department of Teaching and Research in mathematics and statistics whose courses were geared towards the social and human sciences. I had requested an interview with Marc Barbut, director of this department. He spent a considerable amount of his time with me during which I told him of my intellectual journey and asked for advice. He suggested I should register for a degree in applied mathematics while completing my training in sociology. He offered me a post as a research assistant which was my livelihood for two years. The work I carried out introduced me to computers in part thanks to Jean Pisani Ferry, a professional engineer,

whose acquaintance I had made. Jean eventually became a prominent economist and currently holds a high position of responsibility in the current French government. This work mainly took place in the evening. I had enough time to take maths courses and begin research for my PhD dissertation.

INTELLECTUAL TRAINING: THE DOCTORATE TIME

Let us talk more about your graduate training. Who supervised it and what topic did you chose to address?

A clarification is needed here: at that time we had to prepare two doctoral theses for examination. The first was known as the 3rd cycle doctorate (*doctorat de 3ème cycle*), broadly equivalent to a PhD in other countries; the second was for the doctorate in humanities and human sciences (*doctorat ès lettres et sciences humaines)*, a higher level doctorate, which no longer exists in that form as it was phased out in 1984.

It was Raymond Boudon who supervised my PhD thesis. Moreover he suggested that I focus on secondary analyses of what, at the time, was considered the largest international survey in the sociology of education. He had copies of the data tapes and provided them for my use. We saw each other about twice a month to review the results of my analysis.

Today's scientific researcher who is able to analyse data from any large survey on his laptop in record time, could not imagine the immense difficulties we encountered doing the same thing. At the time we only had one big and powerful CNRS computer that was installed on the site of the University of Paris Sud in Orsay. It was impossible to use it during the day on a regular and constant basis due to the large number of users, its limited memory and the size of our samples. The only solution was to wake up very early and take the *métro* so as to be at Orsay by five o'clock in the morning. The few individuals who needed a large amount of computer memory, such as me, could then submit their data analysis as many times as they wanted until eight o'clock. After this time, we had to pack up and return to Paris. None of us could dream that one day we'd have a computer as powerful as the current

supercomputers that can run the craziest simulations one can imagine in astrophysics or meteorology.

One should also be aware that, at the time, it was necessary to have credits to use the computer. The University of Paris V gave me some; but they were insufficient. Fortunately, the Conservatoire National des Arts et Métiers (National Conservatory of Arts and Crafts) where I taught a course also gave me some. Without their generosity, I would never have been able to do the hundreds of analyses used in my PhD thesis.

What do you consider to be the crucial personal and intellectual events and influences during the preparation of your PhD dissertation?

My supervisor taught me that a good thesis is one where each chapter can be turned into a journal article. According to him, this is the proof that the thesis is rich and novel. It's still the rule that I have always respected, even later for all my books. While I was working on it, I published articles drawn from my data analysis and others on the history of sociological theory; all of them designed to support my application to the CNRS.

My encounters with researchers in the hard sciences, mainly physicists and chemists at Orsay, allowed me to improve my knowledge in applied mathematics and numerical analysis.

I also had the opportunity to be in touch with Basil Bernstein, whose work was of interest in my own research. He was intellectually open and very generous with his time, answering all the questions I asked him in our correspondence while I was still just a young doctoral student without status. During the preparation of my doctoral thesis, I devoted a long critical analysis to his books and those of his colleagues.

Can you tell us who were the members of your jury and what memories you have of your PhD defence?

In early 1975, two days after submitting the manuscript of my PhD thesis to Boudon, he phoned me and said he was satisfied with the work and the composition of the jury would take care of itself. Besides him, the supervisor, he asked the mathematician Marc Barbut and Alain

Girard, a demographer and sociologist of education, all professors at the University of Paris V, to take part.

A few years later, when, in early 1981, I had to defend my doctoral thesis *ès lettres et sciences humaines*, we had roughly the same jury, Boudon, Barbut, Girard, but also François Bourricaud and Viviane Isambert-Jamati.

Thesis defence is a rite of passage in which senior figures evaluate a young person who aspires to join their circle. They accept or reject the request with discretion in their evaluations that are included in the report of defence. This report pursues the new doctor until the end of his professional life. But the thesis defence is also an opportunity for the jury to put on good performances as actors and to shine; because they have an audience. Do not forget that my thesis was publicly defended in the beautiful and solemn Louis Liard hall of the Sorbonne.

FROM EDUCATION TO WORK

After the completion of your doctorate dissertation, did you consider going back to Morocco?

After the defence of my PhD thesis, I had to start thinking immediately of preparing the thesis for the *doctorat d'Etat ès lettres et sciences humaines*, which was necessary for anybody who wanted to become a university teacher. I registered with Boudon as my supervisor.

I did not think about returning to Morocco although a good friend recommended me for a post as a prefect at the Moroccan Home Office, which was seeking to recruit young PhD graduates. For my part I wanted to pursue a research career in France. The only possible way was in the CNRS. The alternative, teaching, did not really attract me.

In order to earn my living before I could compete for a position at CNRS, I agreed to work as a researcher in a private multi-purpose consultancy. I had met several of its staff including the great designer Pierre Paulin who was well known for having redecorated the Elysée Palace at the request of Madame Pompidou. We had not worked together since, at the time, he was designing new bathroom products for the Alibert

company, while for my part I was involved in urban sociology with one of his architects. The work we had to do was to provide a historical sociology of the Temple from its construction by the Knights Templar in the thirteenth century, its role as a prison for the monarchy during the French Revolution, and its destruction and replacement in the mid-19th century by the current Square du Temple, in the Third arrondissement of Paris. That is, I helped with architectural surveys of the archives to provide a description and explanation of the functional changes in the constructed edifice.

Did you spend any time abroad to complete your training?

Apart from my long stay in Germany, I could not complete my education and my experience until later when as a young researcher at CNRS, I received a grant from the British Council to spend a full term at the University of London, at the invitation of Basil Bernstein with whom, as I said earlier, I was in contact since my PhD. He received me in his office where we spent an entire rainy afternoon drinking white wine and smoking non-stop. Our discussions were about Parisian and British intellectual circles. He always came back to his theory of codes and accepted its relationship with Durkheim's theory of two types of solidarity as I understood it. We saw each other several times outside of the university during my stay.

I also spent a term at Nuffield College, Oxford as a visiting scholar where I had the chance to meet colleagues from different intellectual backgrounds at Nuffield as well as in other colleges where I was invited. I found that the lifestyle, type of transmission of knowledge, the selection and the construction of Oxfordian excellence corresponded to certain propositions derived from Bernstein's theory. Later, I spent time in a number of universities as both a professor and visiting professor.

What was your first job as researcher, teaching assistant, or professor?

My first job was as a research assistant at the University of Paris V during my PhD. At the same time I taught a course at the Conservatoire National des Arts et Métiers. I was attached to the chair of Professor

Ducassé, a philosopher and historian of technology. He had not been elected to the leadership of the Institute of History of Science at the Sorbonne. Too many personal interests had frustrated his candidacy for a function for which he was well prepared. This is a common story in academic circles, not just in France but also elsewhere.

During his stay in the United States in the early 1970s and 1980s Boudon asked me to replace him at the University of Paris V and University of Geneva.

Can you describe the circumstances under which you applied to the CNRS?

I applied for a position as a CNRS researcher having published several articles. This was in 1976. My candidacy was strongly supported by Jacques Lautman and the rapporteur of my application, Doris Bensimon. One morning, the president of the Sociology and Demography section of the Committee of the CNRS called me to tell me that I had been ranked among the six candidates to be appointed by the directors. It was obvious that my choice of a unit of attachment could only be that of the Groupe d'Etudes des Méthodes de l'Analyse Sociologique (GEMAS), headed by Boudon. The following year, I was asked to teach a course in social science methodology at the Institut d'Etudes Politiques in Paris. I took this on for ten years before leading a research seminar in the same institution. The French Ministry of Planning also offered me a job as a consultant. With an economist colleague we were responsible for managing all the applications for research funding in social policy for the ministry.

EARLY WORK

During the 1970s and 1980s, sociology of education was your main research interest. What was the state of sociology of education in France at that time?

I explained the circumstances under which I began research in the sociology of education earlier. When I began my research career in

sociology in France, the positions were defined with two poles that had the work of Bourdieu and his team at one end and that of Boudon at the other, who had always been a liberal loner who did not attempt to build a school. The GEMAS that he created and directed was a kind of gentleman researcher's club whose members must only respect one rule: the excellence of production set by international standards. Boudon had never asked anyone to take up a particular position. In GEMAS, there were liberals, people committed to the left and even a member of the French Communist Party.

Bourdieu was undoubtedly the most popular in France and abroad as I would note during my many trips outside of France. He was brilliant, attractive. At first I had very cordial relations with him. We used to have long discussions in his office or in the hallways of the fourth floor of the Maison des Sciences de l'Homme on boulevard Raspail, where our offices were next door to each other.

Unfortunately, our relationship became execrable following the publication of my second book, on the changes in the education system in France (*Les changements du système éducatif en France*) in which I questioned some of his theses on reproduction by subjecting them to the test of analysis of longitudinal data. We remained at odds for twenty years until his serious illness, just before his death, when, by chance, we met at the Maison des Sciences de l'Homme. We spoke to each other in terms that were full of a civility and serenity that were far from the virulent correspondence we had exchanged twenty years earlier.

This sad episode speaks volumes about the difficult scientific dialogue with Bourdieu who had given up the use of reason. He had moreover made sure to exclude from his inner circle some of his former followers who had dared make comments that were unacceptable in his eyes. In comparison, let me recall the intellectual nobility and exemplary behaviour of Michel Crozier, the most brilliant French theorist of organisations. He never took umbrage at the critical discussion that I devoted to his thesis on the conditions of the reforms in France and their consequences. While leading a research unit composed at the time of excellent researchers on whom he could have drawn to fill the position of professor at the European University Institute in Florence, he

chose instead to ask me as he considered me worthy of representing French sociology. I had other projects and had to decline the offer.

I had never been attracted to Pierre Bourdieu's way of working nor to his weak theories, which often consist of hyperbolisations of sensible remarks translated into the sophisticated language of philosophers and which remind me of the rhetorical exercises and word games of my first year as an undergraduate. His explanations were not satisfactory because they were based on unreliable and poorly analysed data. The analyses that he had carried out by some of his colleagues whom I met in the new computer rooms in the basement of the Maison des Sciences de l'Homme did not go beyond simple cross tabulations and percentages or even systematically exploited factor analysis.

This methodology was very fashionable thanks to the malign genius of a brilliant French mathematician and statistician, Benzécri, who had seduced, God knows how, this new generation of researchers in the social sciences and humanities. This false Leibniz of modern times proposed a "*mathesis universalis*" able to answer any questions of the poor social scientists who lacked the means to answer them, and who were also unable to question empirical data intelligently. If you open both volumes that Benzécri devoted to the analysis of data, you will be amazed by his encyclopedic references that range from St. Thomas Acquinas to Linnaean classifications, physical theories, and all the social sciences and humanities. You will then understand just how seductive he could be.

What was the target of your research in this field?

The only audience I was aiming at was that of my peers. Besides, I was living in the closed world of research. If I cared about social issues it was only as sociological problems and not as opportunities to change the world. I noticed that those who shared the philosophical perspective of social reformers or revolutionaries were especially concerned with doing militant sociology and not scientific sociology. Their research was at best similar to the sociographic practice of the social hygienists in the early nineteenth century. This is pre-Durkheimian sociology, a compassionate sociology.

A lot of current sociological research on urban planning, poverty, inequality, on marginal groups, etc., does not go beyond this level and contributes almost nothing to scientific knowledge. Policy-makers, who believe they can be used to conduct informed social policy, treat them royally by providing budgets that are in some cases very substantial. I feel sad that there are so very few who learn from the past and that the same mistakes are repeated indefinitely.

Could you tell us more about your views on the debate between Raymond Boudon and Pierre Bourdieu with respect to inequality of educational opportunity?

The very same question was put to me by Peter Abell during a dinner at the first meeting of the European Academy of Sociology. There is no simple answer. Bourdieu was convincing because he had an argument of great simplicity that everyone could understand even though it was expressed in academic language. It was also in the *air du temps*. His militant sociology was a constant accusation that appealed to the audience of clerics whose main motivation is often resentment but in some cases generosity as well.

Boudon was too complex for anyone looking for an easy argument to reproduce. His endless distinctions and intellectual prudence were barely tolerated by the average reader. His sociology was not based on resentment and social critique.

For my part, I confess that I have never read any of Bourdieu's writings where he offers a scientifically based explanation of educational and social inequality or any other sociological problem. Everything about him is reduced to social determinants, to mechanisms of socialization that are also overdetermined, to black boxes such as *habitus* (a term borrowed from Max Weber regardless of the context in which the master of German sociology had used it), the use of conspiracy theory by those he calls the ruling fractions of the ruling classes! It was only much later that he would realise his mistake and try to take account of the actor in *Choses Dites (In Other Words: Essays toward a Reflective Sociology,* Eng. edn. 1990). In addition, the Bourdieusian machine to explain reality was so well oiled so that it could apply to any class of phenomena.

What could be better? A universal panacea, similar to factor analysis was now available: put any data in your machine and you will get results that you can interpret according to your needs, in an opportunistic way. You will notice that many followers of the "theory" of Bourdieu take it and apply it ritualistically in the most diverse and varied fields. It is such a caricature that it's downright laughable. The masterful rhetorical talent and brio that characterizes Bourdieu's writings are absent from those of his students.

Far be it from me to say that all Bourdieu's intellectual production should be discarded. Some anthropological works on the Kabylie and philosophy are excellent. His occasional studies on a range of subjects, on Cassirer or Panofsky for example, are important. Other modest empirical works such as that on photography are of a classic but illuminating nature.

It seems that Bourdieu was actually the victim of the success he had with French public opinion and later specific circles abroad. I am sure he would have contributed to the advancement of sociological knowledge if he had limited himself only to his peers, the sole essential audience for any scientific discipline. This is the great dilemma of all researchers in the social sciences and humanities, unlike other disciplines. It is also the biggest challenge facing any scientific institution that risks collapsing if it does not pay attention to the choices of its members and if, instead of using strictly professional criteria, is lulled by the siren song of the media. We must never forget that our institutions are fragile.

A physicist communicates only with his peers. He only requires the recognition of this circle. Of course he may well be concerned with popularisation when he reaches a certain age or when his intellectual abilities weaken. This is not the case with the sociologist, the economist or the historian who constantly have to choose between these two audiences. Our colleagues are sometimes tempted to choose the public rather than their peers. They prefer the recognition and legitimacy of the media and the general public as if they were politicians. It is a choice that generally proves fatal for scientific research. Have you noticed that they often subjugate our colleagues? Basically, they would rather be advisors, powers behind the throne. I have noticed that some colleagues are obsessed with power.

For Boudon however, there is no panacea, no universal methodology that applies to all, no general theory of society, because each class of phenomena requires a specific methodology and specific explanations. The phenomena of mobility require special methodology. Some social processes need to be understood and explained using simulation models, as he admirably demonstrated in his *Inégalité des chances* (Eng. edn. *Education, Opportunity and Social Inequality* (1974)). How could one imagine that this book, which completely revises our entire perspective on mobility, with an unparalleled scientific rigor that is technically difficult to access, could be read by the average person? This is a book that is intended only for our peers.

In your opinion, what is your main contribution to the sociology of education?

The contributions of my first book titled *Paradoxes de la réussite scolaire* (*Paradoxes of Educational Achievement*) are modest. I think I questioned some false beliefs that dominated the sociology of education: about selection, about the exemplary achievement of some students from disadvantaged classes in the most selective programmes and schools, about their educational plans, about the futility of some teaching methods that were thought to be crucial for the educational outcomes of schoolchildren, about the crucial role of teacher training and excellence especially for schools with socially disadvantaged students, and about the need for social diversity especially for disadvantaged children. I also demonstrated that certain beliefs—such as that when the number of students in a school class decreases, (a thesis supported by the unions), or when the number of hours of instruction in a subject increases, the educational achievement of school students will automatically increase—are false.

I was only heard by a few colleagues who were kind enough to review my work in academic journals. When I published my first book, only the right-wing newspapers praised me because I stated, based on analysis of data from seven European and American educational systems, that selection is more beneficial to children from disadvantaged classes than other classes; it is even more beneficial to the most

disadvantaged when its criteria are clearly visible and easy to understand than if they require the sort of informed decoding of which only parents with higher levels of education are capable.

I explained in fact why the school of the Third French Republic, as it had been conceived by Jules Ferry, had been more socially just. To be honest I only formulated my thesis in this way later when the righteous left attacked me. They accused me of being a right-wing sociologist instead of taking the trouble to read what I had written. I remember a meeting at the highest level in a ministry in Paris, during which I was attacked by both government officials and trade unionists. I had to explain to them the relationship between my results and the republican ideal; more importantly, it was not until a very eminent socialist figure who was present during these discussions confirmed my comments that people start to listen. The support of this personality had earned me a tempting offer to head a large institute specialising in education, which I declined.

My second book on the changes in the education system was better accepted—except by Bourdieu and his followers, because I falsified his hypotheses. Using rational choice theory I tried to describe and explain the models of change in the school and university populations of France, the paradox of the choice of science streams by high school students who intended to study literature, the social and educational strategies of children from privileged classes, how they redefined the meaning of educational institutions, the sometimes good and sometimes unfortunate consequences as well as the unintended effects of educational reform, how the elites in the Ecole Normale Supérieure are over-selected, the effects of feminisation of the social composition of streams and types of studies such as medicine, etc.

Later on, from 2005 to 2009, I conducted several surveys on teaching and research in Morocco whose results were intended to be used for a comprehensive reform of the university system in Morocco. The great Mass attended by the Moroccan government as well as the King's advisers and all the senior civil servants, during which I presented my research results did not help much. My discussion and my proposals have been superbly ignored even though one of the King's advisors, a friend of mine who unfortunately died just after a withering lung cancer,

assured me that he would send a synthesis of my speech to the King. My book *La crise de l'université* (*The University in Crisis*) published in 2011 did not help in tackling the serious issues in the Moroccan educational and research system. But the power of the feudal groups is such that no fundamental reform can be carried through. I have always said this and repeated it in several articles and interviews in the Moroccan media. I broke down and analysed for example almost all of the intellectual production of Moroccan university professors and researchers as well as teachers and non-professors from 1960, the date of establishment of the first university, to 2006. The wealth of results that I collected allowed me to falsify a wide range of hypotheses, and to strengthen and put forward others on the intellectual markets, about the paradoxical consequences of public policies, those of Weber, Lotka, Simon, Merton, Ben-David, Bourdieu, Boudon etc.

QUANTITATIVE METHODS

In your research on education and on the analysis of large-scale datasets, what quantitative techniques did you use?

Basically, we were self-taught in quantitative sociology. The statistics courses that I followed helped me a lot. But in practice, one had to learn on the job, starting with the simplest models and then going on to the most complex. In my early work I used linear and nonlinear regression models, those of analysis of variance, covariance, with transformations of dependent variables according to the procedures of Box and Cox. I even published a purely technical article on these transformations and their sociological significance. I went on quickly to log-linear models in which a friend of mine, Jim Lindsey who had come from Imperial College in London and wanted to do a PhD in sociology, had initiated me in the early 1970s. I read the first articles and books on statistical analysis of categorical data by Cox, Plackett, Haberman, *et al*.

In my work on changes in the education system, the methodological approaches were different. I applied laws and dynamic models to describe longitudinal data. It is much more complex.

What was the state of quantitative sociology at that time in France?

In the early 1970s, the training of French sociologists and political scientists did not prepare them to use quantitative approaches. Those who ventured to do so were limited to cross-tabulations and intuitive analysis of percentages. The most quantitativist researchers used rudimentary techniques or at best the most basic statistical models offered by the first versions of the software such as OSIRIS, SPSS and BMD.

But most took refuge in what I call the asylum of ignorance, that is to say, factor analysis. I remember a research engineer who worked in a large Parisian consultancy, which built polynomial models saturated in n equations to "explain" 100% of the variance!!! This wonderful tour de force is the sign of the absurdity of blind methodological approaches that actually reflect the poverty of thought that sought to hide behind highly technical approaches.

I would add, however, that some rare sociologists and anthropologists used more complex statistical or mathematical models such as graph theory in some essential work on social psychology, those of lattice algebra which Marc Barbut introduced to us to process partially or totally ordered variables or the game theory that was taught us in the undergraduate degree in applied mathematics.

Did you have any international contacts and/or collaborations who helped you to develop your quantitative approach?

By devoting his higher doctoral thesis to the mathematical analysis of social facts, and as a result of having run a seminar for two years that he later published under the title *Mathematical Structures of Social Mobility*, Boudon showed us the way. The teachings in mathematics and statistics of Barbut and others were original. Such lessons were concentrated in only a few enlightened locations in Paris. Finally, a small number of American colleagues, who visited us at GEMAS and who had mastered quantitative models in relation to rational choice theory, helped me to understand and use them.

SOCIAL MOBILITY

At the end of the eighties, social mobility started to become a central topic in your research. What links do you see with your early work on education?

Actually, my interest in social mobility began in the early 1970s when I attended Boudon's seminar on this subject which led on to the publication of two major books, *L'inégalité des chances* of 1972 (English edition, *Education, Opportunity, and Social Inequality: Changing Prospects in Western Society*, New York, 1974) and *Mathematical Structures of Social Mobility* (1973) for which I had made several simulations using stochastic models at Boudon's request.

In fact, it is difficult to conduct research in the sociology of education without being concerned with social mobility. Why? Because we always end up by questioning the influence of education on the change of status, on social promotion. One of the Durkheimians, Paul Lapie, who conducted research on education, was the first to construct and analyse mobility tables. I wrote an article on him in order to rescue him from being unfairly forgotten. Moreover, at that time I was finishing my second thesis on the dynamics of the French education system, and I was interested in how Tocqueville and Durkheim conceived the consequences of change, which had led to some of their publications. I could not fail to come up against the problems of anomie, frustration, political instability, the redefinition of the meaning of institutions by their users. This means that it was also my reflection on the history of the theories that got me interested in the consequences of mobility.

Compared to the most common forms of research done in this field at that time, you approached social mobility in terms of its consequences, in particular with respect to feelings of justice and political behaviour. Why did you choose to study these aspects?

Subsequently, I collected empirical data on the political consequences of mobility and subjected them to secondary analysis. I collected all the available data published in journal articles and those that my colleagues from the CEVIPOF (a CNRS and Fondation des Sciences Politiques

research unit), Daniel Boy and Nonna Mayer, had sent to me. My interest in this topic would lead me later to propose an interpretation of Tocqueville's work.

Moreover, at GEMAS, we conducted a major survey on the perception of inequality. We had a contract with the French Ministry of Planning to conduct empirical research based on interviews as well as a questionnaire survey on that topic. Some of the GEMAS members took part in the data collection and in writing a report under Boudon's supervision and intellectual influence. It offered me the opportunity to write a long report on social mobility and fairness that I published in condensed form as an article. I discovered a new field of thought that already had strong intellectual traditions and a specific methodology.

Almost simultaneously, Boudon asked me to write two chapters of the *Traité de sociologie* (1993), the first on stratification, the second on social mobility.

In 1995, you edited a special issue on social mobility for the Revue Française de Sociologie. *Could you tell us more about this project*?

I suggested the idea of a special issue on mobility for the *Revue Française de Sociologie* (RFS) to my late friend Philippe Besnard. He asked me to work with Louis-André Vallet on this project. Vallet had just published an article in the RFS and been awarded his PhD on women's mobility with Boudon as his supervisor. He had mastered and applied the odds ratio technique to data on mobility. He is a very good technician and a conscientious researcher.

THE CLASSICAL SOCIOLOGIST

Over the years you have written extensively on Durkheim and Weber, and, more occasionally, on Tocqueville and Hobbes. Why did you pay so much attention to the classics of our discipline?

In fact, my interest in the sociology of education and inequality had led me to a deepening interest in the work of the founding fathers of the discipline, especially in Durkheim and his school, and later in Weber.

In truth, throughout my life as a researcher, I have carried out both types of research.

I would point out that I am not an historian whose work it is to appraise texts, to date them, to assess their truth and to offer an interpretation of them. No. History for me is more of a reservoir of ideas, issues and experiments conducted by people who have faced problems, proposed solutions, and also made mistakes. In short, in my opinion, history is a laboratory. This is not a tomb but a living treasure of inexhaustible resources that will answer the questions one wants to ask. The best way to ask them is to be at the forefront of research, to know the problems faced by contemporary theories before consulting the past and seeking its help. But this view of history is not my own idea. It is found, for example, in the work of Georg Cantor and Joseph Schumpeter. Read, for example, the article "Grundlagen einer allgemeinen Mannigfaltigkeitslehre" ("Foundations of a general theory of aggregates") or the mathematical and philosophical papers on infinity by the founder of set theory; you'll be edified. You might also browse the wonderful pages of *History of Economic Analysis*.

Are there any of the classical sociologists who you consider to be especially important in your vision of sociology?

I devoted several publications to Durkheim and Weber, and less to Tocqueville. I also spent a lot of time on studying Schumpeter, as both economist and sociologist. When we were preparing the *Dictionnaire historique de la pensée sociologique*, I suggested to Boudon that I should write the entry on Schumpeter, but he had already asked another colleague to do it.

I was in a sort of dialogue with these masters of the past. I questioned them every time I faced a theoretical problem. This does not mean that I was not in dialogue with living colleagues as well …

What is the sequence in which you read and discovered the classics?

The periodisation of my publications is actually misleading. It was my work on Durkheim that appeared first. In truth though, I had made

a huge study of Max Weber's work when I was in Göttingen. One of my German friends had greatly encouraged me by giving me books by Weber which were out of print. On my return to Paris, I wrote a long paper on the education, power and bureaucracy of the Mandarins in China based on Weber's writings. The Weberian idea that I was developing is related to the existence of macro-social correlations between types of power, modes of elite selection and forms of legitimation. In fact the case of the Mandarins is an example of Weber's thesis that applies to several different social configurations, whether the Greek aristocrat, the new democratic Greek parvenu who sought the teaching of the Sophists, the cleric of the Middle Ages, the generalist gentleman of the eighteenth century, or the specialist in industrial societies. One can make the same comparisons by taking account of forms of democracy. I should improve it and publish it, but circumstances have never been right. It still sleeps in my filing cabinets.

It seems as if Pareto played a smaller role in your thinking to Durkheim and Weber. Is this correct?

You are right. However, when in 1998 I started writing a book on social stratification that I have not as yet completed, I devoted an entire chapter to the Paretian conception of stratification and mobility. In addition, two years ago, I had to write an article on social mobility and its consequences in Pareto's work as well as its legacy for contemporary sociology for the special issue on him of the *European Journal of Social Sciences*. I collected all the documents I needed and did all the necessary reading. Because of lack of time and since I had to complete other more urgent work, I had to abandon it.

Did Philippe Besnard, another great scholar of Durkheim, have an influence on your own reading of this author?

Philippe Besnard had a post at the Maison des Sciences de l'Homme. I had met him in 1973 when I submitted my first article to the *Revue Française de Sociologie*. As an editorial member of the RFS, he had read it. He contacted me at GEMAS to discuss the form and substance of the

article. It was with intelligence and exceptional generosity that he did this. I did not know at the time that he was preparing his doctoral thesis on anomie. It was when I suggested another paper to the RFS that related to Durkheim that I discovered his immense knowledge of Durkheim and his school. We became inseparable and loyal friends until illness and death separated us. He joined the CNRS late in life. Then he came to be with us in GEMAS before leaving to direct the Observatoire Sociologique du Changement. Despite the real success of his efforts to make this unit one of the best research groups in sociology, he had the bitter experience of facing an ungrateful coup from within that tried to dislodge him.

There is no doubt at all that there were reciprocal influences on our interpretations of Durkheim. The obituary that I asked Boudon to write on his life is a proof of the friendship between us, our complicity and our mutual intellectual consideration. In fact we were a trio with Massimo Borlandi, one of our best historians of sociology who is a person of great intellectual rigor.

EPISTEMOLOGICAL RESEARCH

Since the mid-nineties, the micro–macro link and the theory of explanation became central lines of your research. What is the connection with your early work on inequality of educational opportunity and educational systems?

The problem of explanation has always been one of my concerns. I guess it is for everyone except for those who believe that science is limited to description and classification, and cannot go beyond that. The great minds share this vision, such as Pierre Duhem, who was in some ways the successor of Comte, or closer to home the economist Paul Samuelson, at least in his statements during debates about the epistemology of economics.

In my early work on education and mobility, I encountered this problem in the use of rational choice theory. It was clear to everyone that we had to decide between explanation based on the determinism of structures or by rational, although forced choice. But I was unable to move from the individual to the collective level. I stayed at the individual level. I looked at the solutions suggested by physics and economics.

The first did not allow me to understand the emergent phenomena that are common in sociology, perhaps due to my ignorance; and the second were unsatisfactory.

From 1987 to 1988, I made my point of view on the subject increasingly clear. At the inaugural annual lecture at the University of Lausanne in 1989, when I was appointed professor, I devoted my paper to the subject of explanation in the social sciences and the problem of the relationship between the micro and the macro. I took this further in several other lectures and seminars at the Universities of Geneva, Rabat and elsewhere. As you rightly point out, it was from the mid-1990s that I started to publish more articles on these topics.

How does your book on "Invisible Codes" *relate to the international debates on social mechanisms and analytical sociology?*

The conception of generative mechanisms as a basis for explanation and a possible solution to the problem of the transition from the micro to the macro seemed to me at the time to be increasingly productive. I would draw your attention to the fact that there are two problems. As usual, I used and investigated history; and I drew upon contemporary theories. I pointed to the existence of a positivist tradition that refused to search for mechanisms. It goes back to Auguste Comte who explicitly called for anathema on any search for "generative mechanisms", a phrase he had used in his *Course of Positive Philosophy*. It continues to this day. Curiously you will find it in the work of sociologists of the Columbia school around Lazarsfeld, among political scientists and in general in all macrological studies that focus on predicting the evolution of phenomena without necessarily worrying about the micrological foundations of macrology.

I have identified the founding fathers of explanation by mechanism such as Claude Bernard, followed later by some particle physicists of the early 1900s, still later linguists such as Chomsky. Sociologists have only really taken this route rather late in the day, with Boudon, Schelling, and Fararo for instance, although there were attempts by Durkheim, Weber and Tarde. Any explanation by generative mechanisms is necessarily demiurgical in the sense that Theaetetus intended. You can build

purely macroscopic generating mechanisms. They are easily found in the work of several economists and sociologists. In sociology, for example, normative theories enable the movement from macro to micro; they can link the macro to the macro, but not the transition from micro to macro.

In fact, the discovery of the role of generative mechanisms preceded the solutions of the problem of movement from micro to macro and vice versa. To be convinced, just read the book by Hedström and Swedberg on *Social Mechanisms*.

Thus it was necessary to solve the two problems simultaneously. This is what, modestly, I tried to do in a clumsy way in my publications, and especially in *Invisible Codes* which is the conclusion to the earlier work. I also tried to deepen my thinking on the explanation of emergent phenomena in *Good Intentions* and *Crise de l'université*.

The ambition of some colleagues to define sociology as analytical is not a problem for me. Is it, however, a new theory and a new approach? I do not think so, because the leaders of this orientation are merely continuing a strong tradition that has always regarded sociology as a scientific discipline.

The concept of methodological individualism is still controversial in sociology. Would you define yourself as a methodological individualist and, if so, in what sense?

In all scientific disciplines, controversies are desirable provided that we are not spending too much time on them. We are all methodological individualists at one time or another in our research work. We may cease to be so when our means of investigation and explanation impose limits on our creative imagination. Even the anti-individualist who believes only in the effects of structures and conceives the individual as a moldable dough, is required at some point in his work to stop believing this fiction. Durkheim noted somewhere that even the cat can tell the difference between a toy mouse and a live mouse as an animal endowed with a certain autonomy. The cat is not fooled. The eternal question of the programming of the human being will never cease to haunt us. It will continue to do so with the new cognitive sciences.

Do you feel that your epistemological standpoint leads to special resistance within French sociology?

Assuredly: I would only need to offer as proof the fact that some French researchers devote their time and energy to their wish to demolish methodological individualism. But let's be generous: the most demanding methodological individualist will violate certain rules of this epistemological orientation and take refuge in universals and reifications that even Weber had tried to fight throughout his intellectual life. In economics, you will find some fine passages about this in the articles of Carl Menger.

Things change over time; but it is difficult to get rid of certain intellectual reflexes.

MOROCCAN SOCIETY

It seems that your first published article on Morocco appeared in 1994. Was your interest in Morocco earlier than that?

I wrote that article on the occasion of a thesis about marriage in Morocco. In fact, I had already carried out some unpublished work on the effects of the structural adjustment policies that the International Monetary Fund had imposed on Morocco in the early 1980s. At the time, I took part in some projects. Among the oldest, I would count the thesis of my late friend Remy Leveau, who had in the mid-1970s asked for my assistance in his analysis of Moroccan electoral data.

Your published work on Morocco now seems more regular than before. Why?

You are right. Since 2003, after many requests from my Moroccan friends, I have increased my research concerned with Morocco and become more involved as an advisor to senior civil servants and political authorities. It was firstly the Senior Commissioner of the National Plan who invited me to prepare a conference on the future of Moroccan society in 2025. I had to write a long note on the subject that would be

the plan and direction for the conference. My geopolitical and sociological work on the Sahara was a response to questions that a dear friend had asked me and this took me a long time because I had to undertake very large scale field surveys which were extremely difficult to carry out. Then there was the major survey on teaching and research in the humanities and social sciences which lasted from 2005 to 2009, and was commissioned by an interministerial committee chaired by the Prime Minister. These are the reports that I wrote on trust in institutions and social bonds. My late friend Meziane-Belfqih, adviser to the King, asked me to carry out a major survey of the social bond for a new institute that he had established. This was the work that I published on the "Arab Spring". At the moment I am working on a major sociological synthesis on Morocco, for which I am using all the available data and surveys from the beginning of the twentieth century until today.

In truth, I have finished the first chapter of the book. The text is somewhat long since it is almost 100 pages. I took this opportunity to ask numerous sociological questions about Islam and its amazing spread in societies where the faithful did not practice a lot. I compared about 40 Islamic and non-Islamic societies. I used all the national and international data on the subject including those of the *World Values Survey* and those of the Moroccan High Commission for Planning. I hope to include other data from other sources. I am frankly surprised that such important issues had not hitherto been seriously studied by sociologists of religion who are of course aware of these tidal waves. I use all possible assumptions, including those I had advanced in my *Good Intentions*, to offer some explanation. Inglehart and his colleagues have been obsessed with the relationship between Islam and democracy or gender equality in Islamic societies. They also wished to test the hypotheses about Huntington's clash of civilizations. They have not faced what seems to be the real issue.

What was your personal and institutional involvement in Moroccan academic and intellectual life during your career?

In one way or another, I have never stopped playing a part in the intellectual and institutional life of Morocco. My own family has had many

religious, intellectual and political roles since at least the fifteenth century. Arriving from the East with the first waves of the Banu Hilal in the eleventh century, my family settled in the southern region of Morocco before moving to the central plains and founding religious centres of which the most important is Boujad. Its privileged relationship with the royal courts and with the Arab and Berber tribes had earned it a special status. Many of its members were ministers, ambassadors, counsellors, judges etc, but mostly intellectuals who have left behind many writings, and mystics, however, committed to alleviating the suffering of their protégés. A number of works in anthropology, history and political sociology have been devoted to it.

Whilst for my own part I decided to take up a career as a researcher in France rather than one as a senior civil servant or in Morocco's political life, I have never broken away from my Moroccan connections. And it has always been as a researcher that I have accepted occasional assignments whether in France, Morocco or elsewhere. In the mid-1980s, I agreed to write a report on the social consequences of structural adjustment policies in Morocco. I also agreed to provide a seminar at the University of Rabat from the mid-1980s when a group of students from the Royal College were studying for the Diploma in Advanced Studies. The seminar included a presentation of game theory and the application of models of this theory to the relations between Morocco and Algeria. Twenty years later, I returned to this early thinking and it would form part of my first study in geopolitics.

But it is has been since 2003 that my activities in Morocco have greatly intensified. My personal and family relationships led me to respond to some informal and official requests for advice and reports on various topics. I cannot mention them all, but here are some examples. I was appointed by His Majesty the King to be a member of the Royal Commission on Regionalization. I was given the rank of lifetime professor, and act as unpaid advisor to some of the institutions of civil society.

Sometimes I have declined requests where I found them unreasonable or doomed to failure. For example, about twenty years ago, I was approached by the Moroccan Prime Minister to be on the list of a group of persons who were proposing educational reform. I declined

the offer because of the working conditions and the very large number of participants. I was right, of course, since the so-called reform has worsened the situation. A year ago I was again asked to be a member of a committee on reform of the education system. I explained publicly this time why I refused to take part. I thought, as I always think, that the mode of work and functioning of this committee means it was inevitably doomed to failure.

DIFFUSION OF KNOWLEDGE AND TEACHING

You devoted considerable energy to the diffusion of sociological knowledge through editing, and/or contributing to, dictionaries, encyclopedias, textbooks, and collections of texts. What was your motivation in undertaking such a great amount of editorial activity?

Anybody who has devoted some of their time to editing or writing dictionaries or series of collective works knows that they are providing a service to the scientific community. Where I was concerned I thought that my research activities were too focused on my own goals and not enough on the dissemination of knowledge. You will be aware that my teaching at several universities was always quite limited and did not involve many students. In truth, I was always been put off by teaching except when it came to seminars where I could present my research and the results to which it had led.

Would you say that this has had a substantial impact on French sociology?

I hope so. In any case, if I judge by the number of editions and translations, the dictionaries, encyclopedias and anthologies may have been useful in the dissemination of knowledge in France and elsewhere. I am afraid that this has been more the case abroad than in France, where sociology has not yet acquired the status of a scientific discipline that has gone beyond petty politicking.

Apparently in contrast to your will to diffuse a certain kind of sociology through general and pedagogic writings, only rarely have you agreed to supervise PhD students. Why?

I am a little ashamed to admit that I have always refused to supervise theses. Throughout my career in France I had only one doctoral student: Gianluca Manzo. He began by preparing a thesis under my direction on Weber for his Diploma of Advanced Studies at the University of Paris IV Sorbonne. I had never read such a remarkable piece of work. When Manzo came to ask me to direct his doctoral thesis, I gladly accepted because I knew in advance that it would be a success. I was right. During the defence of his thesis, all the members of the jury warmly congratulated him for this exemplary work.

It is true however that I once agreed to supervise another student to please Boudon. I was sure that we would not get on. This proved to be the case.

In Morocco, I also agreed to supervise the preparation of the thesis of a friend, Bensouda, former director general of taxes and the current Treasurer General of the Kingdom, on the limited rationality of decisions about finance legislation and tax institutions in Morocco. Not being an expert in public finance, I learned more about what really directed the content of the thesis. However, I have never refused to advise dozens of doctoral students in various disciplines who know that my office and my house are always open to them.

WORK RECEPTION

Were you to perform the difficult task of retrospectively assessing your intellectual trajectory, who are the authors who you would say have influenced you the most?

Boudon was a master and a dear friend whose loss is unbearable to me. My debt to him is enormous. How can a man of his intelligence and modesty with whom I shared forty years of intellectual life, and who had agreed to read my writings and all of whose manuscripts I had read,

with whom I had almost daily discussion on all and any subject, how could this not have had a profound influence on me?

I would also like to acknowledge my intellectual debt to other sociologists whom I never had occasion to meet such as Lazarsfeld, Simon, Fararo, Schelling, Peter Blau, Merton and Coleman.

Several of your books and articles have been translated into various languages. Are you be able to assess how your work was received in different countries?

I confess that I have not sought out all the reviews of my work or how many citations there have been of my articles. This flatters amour-propre; but to me it seems unimportant. Sometimes articles rejected by journals prove to be innovative. All I know is that every book I published has attracted the attention of some colleagues who have been kind enough to discuss them in journals or even in more general publications such as newspapers and weekly magazines. They have had the courtesy to send me copies. This covers France as well as the United States, England, Germany, Spain, Italy, Japan, Portugal, Peru, Chile and Morocco. I do not know the fate of my writings in other countries.

Apart from rare successes, I do not believe that a researcher is able to produce revolutionary results. We contribute at most to the construction of our "common household". So we should not expect to be hailed as a genius every time we publish an article or book. Let's be modest.

Are there any debates raised by your work outside France that you consider especially important?

The previous answer gives you my opinion on the subject. But it is true that when the book is about a subject that has obvious political implications, it causes heated and passionate debate. This is the case of my writings on education systems, political institutions, and geopolitics. This meant I became *persona non grata* in one country but gained public recognition. But in this case we touch on human passion.

Which book or research article do you regard as your best, and why?

It is difficult for me to answer your question. I think it is others who are better able to give you an answer. You know that the itinerary of a researcher is neither linear nor smooth. His publications are only steps from a thought that unfolds gradually, sightings in an ocean over which he sails and attempts to cross although he cannot see the distant shore. The researcher capitalises on some publications before producing others that may be superior in quality. The least original article however, may contain, buried within it, a very successful idea that could enlighten us on a class of phenomena but which will be taken up by someone else entirely.

Thus, we guess that, should we ask you to identify your main contribution to sociological theory and epistemology, you would refrain from replying?

It would be pretentious of me to do so. It belongs to my peers to say what it might be. Maybe I have not accomplished anything important. However, I hope that my modest work will be useful to others, that it has made a contribution to the common enterprise.

Can you at least tell us if there is anything you would not rewrite or change substantially?

My answer to your previous questions leaves no doubt as to my position. Everything can be rewritten; nothing is absolutely intangible, sacred. But one should not deny anything; everything must be taken as it is. We all know that every scientific work is condemned to be antiquated. May I quote here Weber, who wrote: "That is the fate to which science is subjected; it is the very meaning of scientific work, to which it is devoted in a quite specific sense, as compared with other spheres of culture for which in general the same holds. Every scientific 'fulfilment' raises new 'questions'; it asks to be 'surpassed' and outdated. Whoever wishes to serve science has to resign himself to this fact."

INSTITUTIONAL LIFE

To conclude this interview, let us move on to a set of questions concerning your institutional activities in France and abroad. In this respect, could you start by describing your experience as member and then director of GEMAS?

Boudon was first of all asked to lead the Centre for Sociological Studies (CES: Centre d'Etudes Sociologiques), the largest research unit of CNRS sociology. In 1968–9, some events that took place at CES irritated him. They were caused by leftists who probably thought that France was entering a revolutionary era and that we had to break with all authority, be free from coercion, in short, to live in a gentle anarchy. He left the CSE and created GEMAS in the early 1970s and led it until the eve of his retirement. In 1998, he asked me to succeed him. Which is what I did after a unanimous vote of the members. As the name suggests, GEMAS was a research unit devoted to quantitative methods and a wide range of sociological and political studies. It was noted for the excellence of the publications of its members. From the late 1980s, Boudon engaged increasingly in work related to epistemology and philosophy. This approach had an effect on the recruitment of new members who came from a background in philosophy.

As soon as I could take on the leadership of the GEMAS, I redirected the group towards quantitative sociology. All new recruitment was going in that direction. It was not too difficult: the candidates had excellent records; my position at the CNRS enabled me to effectively support them and the leadership of the CNRS strongly supported me. To disseminate the research of GEMAS members internationally, I created a series of books in English with the support of my friend and accomplice Peter Hamilton without whom nothing would have been possible. We opened up this series to other highly original publications. I think we could have done more if we had had more financial support.

Boudon also asked me to take over from him at the Sorbonne as a professor. For strictly personal reasons, I could not take on this burden. He was sorry but in the end he understood my decision. I knew that

my decision would have consequences for GEMAS and the teaching of sociology at the Sorbonne. I was not mistaken.

What will be the future of GEMASS? It will largely depend on the commitment of its director, his willingness to sacrifice some of his time to the community, of his network of relationships that can be used to recruit new researchers, to be able to knock on the doors of the political and administrative leaders of research in France, to not be afraid to recruit brilliant foreign researchers, etc. GEMASS now includes talented researchers; some are less so—alas. It is essential to strengthen this potential.

You devoted a considerable amount of energy to the sociology section of the national scientific committee of the CNRS. Could you describe your activities and what you learned from this experience concerning the organization of French sociology?

I had agreed to serve in the sociology section of the National Committee of the CNRS for ten years. The work involves assessing nominations to the CNRS, and those of researchers who apply for a promotion, to grade the CNRS research units either to support the best or to eliminate the worst and create others. The committee also evaluates the scientific journals financially supported by the CNRS and allocates a budget between symposia sponsored by the institution and other work related to the organization.

Belonging to the National Committee can contribute to the defence of excellence. This is not however always the case. But with patience, diplomacy and compromise, we arrive at good results. That is why it is vital to accept this responsibility and not to leave it to the members of cliques and cults. I will be discreet and not mention the names of certain cliques that are formidable, capable of the darkest plots to help their zealots at the expense of scholars with excellent records, and hence sacrifice our scientific discipline on the altar of defence of the clan. These cynics do not believe in the search for truth but only power and exclusively power struggles. They thus support Callicles and Pontius Pilate against Socrates and Christ.

You have been a member, then, director of the Revue Française de Sociologie. *Could you describe the main historical evolutions of this journal, its impact on French sociology as well as your own personal view on, and experience with, this central scientific institution?*

In the early 1980s, members of the editorial board of the *Revue Française de Sociologie* had paid me the honour of recruiting me as one of their number. Our meetings were held as often as necessary to discuss all the manuscripts that we were all supposed to have read. Without exception, all members were present at the meetings that ended with a lunch in a restaurant. Despite our different intellectual backgrounds, our relations were very cordial. We shared the work equally without difference in status. To fulfil our commitments vis-à-vis our readers and subscribers who expected that we would publish five issues per annum, we launched the idea of publishing a special issue per year. It was much later that we had the idea of translating the best articles of the year which would be published in a special issue in English.

The RFS was undoubtedly the showcase of French sociology, insofar as the journals of narrow interest groups did not yet exist. It played a decisive role in the recruitment and promotion of sociologists at the CNRS and in the universities. Over the years, members of the RFS editorial board changed. By consensus, we recruited new members. Tradition was certainly perpetuated; but some recruitments turned out very evidently to be bad ones.

When, after the death of Philippe Besnard who was then heading the RFS, some former directors of the RFS strongly supported by the secretariat had asked me to head the RFS, I agreed on the condition that I would do it for three years. I knew that some members of the Editorial Board, unfulfilled careerists, wolves with long teeth, that I had however strongly supported to be elected to the Editorial Committee, were sickened by my application and my election. I was expecting the worst from them.

Thanks to my excellent relationship with the management of the CNRS, I obtained many advantages for the RFS. I had plans to open the journal up to an international audience. I had first discussed this with the directors of the CNRS which was quite willing to help us in terms of

personnel and financial resources. I had thus obtained the exceptional renewal of a second post for the editorial section while all the other journals were seeing their personnel melt away like snow in the sun. The CNRS committed to increasing the budget allocated to us so that we could publish every year a book taken from the exceptionally rich archives of the RFS. I had almost convinced a British publisher to be our partner in this adventure. I presented the project to the Editorial Board naively thinking that it would be accepted with only minor changes. I also knew that I would not derive any personal benefit of any kind from the realisation of such a project, since I had already given up the editorship of the RFS at the end of my term as I did not want to renew it. But as sometimes happens in scientific organizations that operate on misunderstood and sometimes misguided liberal principles, some fools had almost accused me of some indescribable plot. Was it because of ignorance or personal and treacherous calculation? This is vile and pathetic. I was delighted to see that this idea is now being used by a prestigious journal—the *Harvard Business Review*. From that point on, I was only waiting until the end of my term to leave the journal.

You have been on the board of several international scientific journals as well as an elected member of important scientific academies. On the basis of this experience, what view did you develop about the differences that exist, if any, between French sociology and the international sociological community?

My presence on the editorial boards of several international journals was very instructive for me. It was an opportunity for me to ask several francophone researchers to submit their manuscripts so that they could make themselves known internationally. Moreover, the manuscripts of analyses and reviews submitted to these journals were not a problem unlike the case in the French journals. Several reasons explain this difference. First, the number of francophone researchers and academics who agree to submit manuscripts to journals is too limited in contrast to the English-language journals. Second, it is only recently that promotion began to reflect the publication of articles. Third, the limited market of French sociology and the functioning of the institutions

paradoxically promote cults and clans outside of which there is no salvation. Fourth, this trend led to the creation of journals linked to particular schools which is detrimental to research journals. Fifth, our French colleagues do not have the Anglo-Saxon and Germanic cultures for which criticism—even where harsh—is a spur to improvement, not an *ad hominem* attack. Sixth, monolingualism, the lack of openness to other countries, the plethora of journals narrowly based on schools and cliques, the tardy hierarchisation of journals which alone would have made it possible to distinguish between the status of articles, contribute in France to incestuous relationships from which one can easily predict that they would have negative consequences on sociological output.

Scientific academies who have paid me the honour of counting myself among their members offer unique opportunities to meet colleagues from other disciplines and to interact with them. One needs to be equipped enough to understand a little of their intellectual interests and problems and to sensitize them to our own. I learned a lot from the historians, archaeologists, physicists, and mathematicians who have had the courtesy to ask me about my research and tell me about solutions to similar problems in their discipline or suggest readings. Here are some examples. At a reception, a historian of religions colleague had strongly urged me to read a series of books on monotheism that informed my reading of Weber's writings on sociology of religion. Another mathematician colleague helped me solve a problem of estimating the parameters of a function that I had stumbled over for weeks. Physicists did not find uninteresting some remarks that I made about the solutions that sociologists had brought to the problem of levels of reality, the aggregation of individual behaviour either as emergent macrophenomena or in the form of resulting phenomena.

You have experience of several international think tanks. Could you tell us more about this aspect of your professional activities and your view about the relation between research and politics?

I only started going to these international think tanks when I directed some of my research towards geopolitical issues. I admit that at the

meetings of these circles, one learns so much more in a few days than if one attended annual seminars or if one read several books that cover various topics.

In addition to the lectures usually given by external invited guests, our annual or semi-annual meetings are opportunities to meet and discuss with the political, military and economic leaders of many countries. We freely exchange remarks. This would not have been the case in other contexts.

The lectures sometimes show us the work of academics and political and economic advisers that we would not have known how to find on our own. I had the pleasure of listening to such brilliant personalities as Brezinski, advisers to Putin, Bush father and son, the leaders of several countries, former British prime ministers, central bankers, members of the Supreme Court of the United States, chiefs of staff of armies, bosses of multinationals, etc. The list is too long. It is amusing to note that these tormented eminences would be beautiful subjects for psychological analysis.

You would be surprised to learn that politicians often make decisions by ignoring the recommendations of experts, including academics. One day I asked a prominent professor from a prestigious British university about the danger of the commitment of the UK alongside the United States in the second Iraq war, despite some British experts knowing the area well. He explained to me that Tony Blair had indeed invited them to go and see him at 10 Downing Street. But at the moment when our colleagues were informing him on the situation in Iraq and the quite predictable consequences of military intervention, the Prime Minister looked out the window. Clearly, his mind was made up well before their presentations. Of course, this story should not be generalised; but it is symptomatic of the relationship between politicians and academics unless the latter are ideologues espousing political theories consistent with dogma.

Over the past decade, in addition to my purely sociological research, I devoted several works to geopolitical problems and especially to this area I call the "crisis ellipse" that runs from the Atlantic to the Indus, which includes all Arab countries, the Sahel, the Near and Middle East, Turkey, Iran, the and the former Muslim republics in Asia.

This geopolitical region has a distant relationship with the somewhat religious one in Huntington's *Clash of Civilizations* or the mythological configuration of the world of American neoconservatives, of which I do not share the diagnosis and even less their plans, although some of their insights deserve our attention once they have been separated from their messianic ideology. I confess to being a reader of the works published by the *American Enterprise Institute* and other think tanks which are numerous in Washington, works that are not limited to geopolitics but also cover urban problems, education, crime, democracy, etc.

Besides lectures that I have given on these issues in several countries and institutions, I started to publish material on the project of the Union for the Mediterranean, and the Arab Spring—which has only been the subject of journalistic articles apart from two attempts at explanation which are based on macro-data—and its foreseeable consequences on the Sahel, etc.

In the coming years or decades, and without necessarily being a soothsayer, one can bet on the emergence of new alliances and coalitions in the world. We enter a new era that, because of a lack of generosity, of myopia and through misunderstood interests, some European, Russian, Arab, and other leaders do not understand or do not want to see, it seems. The large blocs, which will each have a demographic weight of around one billion, will lead the world economy. Even the United States, which is losing its place as economic leader in favour of China, will solve the problem by partnering itself with some of its neighbours, including Mexico. All this will have consequences for politics and especially democracy as we understand it, but which it will be necessary to reinvent.

It is a pity that sociologists do not invest more in this area of research that remains theoretically underdeveloped.