

# Die wissenschaftliche Methode als Hermeneutik des Selbst

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## Zusammenfassung

Die Wissenschaft ist kein völlig klares Gebilde, und Wissenschaftler sind keine durch und durch rationalen Wesen, bei denen jede Voraussetzung vollkommen bekannt und eindeutig und ihre Methoden transparent sein müssen. Sie vermitteln - ob sie es wollen oder nicht, ob sie es wissen oder nicht - ihre Überzeugungen, ihre sozialen und psychischen Festlegungen, die den von ihnen angewandten Methoden zugrunde liegen. Der Artikel versucht, die gewählten wissenschaftlichen Methoden zu beleuchten, indem er einen schattenhaften Teil der Forscher in Aktion setzt, der hier in der Wahl ihrer Objekte, Konzepte und Methoden ebenso wie in der Art und Weise, wie sie wissenschaftliche Fragen stellen, in eine biografische Erzählung umgesetzt wird. Was wäre, wenn die Forschungstätigkeit zunächst in einen Abgrund führen würde und die verwendete Methode eine Hermeneutik des Selbst wäre?

## La méthode scientifique comme herméneutique du soi

### Résumé

La science n'est pas un édifice totalement limpide et les scientifiques ne sont pas des êtres rationnels de part en part dont tous les présupposés sont censés être parfaitement connus et explicites et les méthodes transparentes. Ils véhiculent - qu'ils le veuillent ou non, qu'ils le sachent ou non - leurs croyances, leurs déterminations sociales et psychiques qui sont à la base même des méthodes qu'ils emploient. L'article vise à mettre en lumière les méthodes scientifiques choisies, en mettant en action une part d'ombre des chercheurs, ici mis en récit biographique dans leur choix d'objets, de concepts et de méthodes autant que dans leur manière de poser des questions scientifiques. Et si l'activité de recherche était d'abord mise en abîme et que la méthode utilisée était une herméneutique du moi ?

## The scientific method as hermeneutics of the self

### Abstract

Science is not a totally limpid edifice and scientists are not rational beings through and through whose every presupposition is meant to be perfectly known and explicit and their methods transparent. They convey – whether they want to or not, whether they know it or not – their beliefs, their social and psychic determinations lying at the very basis of the methods they employ. The article aims to shed light on the scientific methods chosen, putting into action a shadowy part of researchers, here put into biographical narrative in their choice of objects, concepts and methods as much as in their way of asking scientific questions. What if research activity was first put into an abyss and the method used was a hermeneutic of the self?

## 1 Introduction

“By method,” wrote Descartes (1953, 4), “I mean sure and easy rules, by which all people who observed them exactly will never think true what is right, and will succeed without tiring themselves out in useless effort but by gradually increasing their science, to the true knowledge of all they can attain.”<sup>1</sup> The dominant image of scientific rationality still presupposes that scientists have their own methods of apprehending reality that guarantees the scientificity of their approach. Consequently, is a science defined by its method or methods? No taxonomy is neutral. A scientific discipline can be defined neither by a central object (because of the interdisciplinary competition over objects as well as the historical variability of the objects of each discipline), nor by a specific method or a dominant paradigm (the nomadic nature of the concepts that make up the history of science denies the idea of intangible and intrinsic disciplinary boundaries (Schlanger and Stengers, 1988). A science is therefore defined by what it does (Mabilon-Bonfils, 1998) and therefore by the power relationships that define legitimate method(s) in a given field. Conflicts and consensus are thus articulated in order to develop a scientific discipline and its legitimate methods. The result of conflicts in the scientific field determines a temporary consensus at a given moment in the state of knowledge: both within itself and in relation to its place vis-à-vis other disciplines. The scientific method is therefore a strategy of legitimacy in a field, as the sociological sciences have amply demonstrated. A scientific theory can only exist if it is “interesting” (Stengers, 1988). However, not every society can build any science. Legitimate theories and methods are built up day by day in laboratories, articulating power effects, stakes in fame, diffusion credit (Latour, 1989): the scientific enterprise is also a conflicting enterprise whose aim is to produce scientific facts and to build a reality capable of resisting the strongest objections of other scientists. Scientists entrenched in their laboratories manage to arouse interest or create a demand for the esoteric knowledge they produce, thanks in particular to the construction of knowledge networks, scientists not only have to produce theories that resist as long as possible the refutations of other scientists, thus resisting controversies (making them talk), but also to build a favourable environment, populated by actors interested in this knowledge (Callon, 1989). Mistrust, rivalry and competition organize the sciences that bet on the power of concepts. The power of concepts does not therefore refer to an inherent quality but to the capacity of their producers to overcome scientific criticism (Stengers and Judith Schlanger, 1988). Method is also a demarcation criterion between sciences, i.e. between the hardest sciences – the sciences of the Centre, claiming to be different from other sciences and managing to have their methods recognized as canonical (Stengers, 1993) – and the peripheral sciences (Mabilon-Bonfils, 1998).

Our hypothesis is that the scientific method – never “sure and easy” – is a path that can also be thought of as a hermeneutic of the self.

Describing the process and the constitutive stages of a research path is always an *a posteriori* retraction of an extra-linguistic world into a so-called scientific discourse, ordering and justifying the coherence of a theoretical corpus produced. It’s a narration. This reconstruction can be likened to a scenography of research which, as such, sets in motion unconscious

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<sup>1</sup> Dans son Discours de la méthode, René Descartes définit les méthodes comme « des règles certaines et faciles grâce auxquelles tous ceux qui les observent scrupuleusement ne supposeront jamais vrai ce qui est faux, et parviendront, sans se fatiguer en efforts inutiles mais en accroissant régulièrement leur savoir, à la connaissance exacte de ce qu’ils peuvent atteindre ».

processes and strategies that value some aspects of the process by concealing others in accordance with a rhetoric of knowledge and research. Such elucidation necessarily postulates a meaning for the described itinerary and inclines the seeker to become the ideologist of their own life by selecting significant events and establishing connections between them to justify their existence and to give them coherence, creating partially artificial meaning. But it can also have a different meaning. It is the wager of this article to put biographical narration into action and use it to show how methods are forms of personal determinations. Because, if the work of research can't be reduced to mere production but is also creation, as such, it is driven by fixed ideas, "themata" (Holton, 1981), fertile dreams or otherwise, and intellectual obsessions that can't be rationalized, at least not entirely.

Yet we suggest that a research route should not be random. It has an internal necessity that we seek here to deconstruct by the yardstick of "unusual" knowledge illuminated by long – albeit evidently partial – psychoanalysis. Obviously, the ambition of total transparency to oneself is only an avatar of omnipotence. While it is usual for literature to Mabilon-Bonfils ize itself to reflect on the autobiographical dimension of all writing (Baroni, 2008) – even though some have called for the Mabilon-Bonfils's "share" to create the reader, especially Barthes (1984, pp. 61-67) – this has been little practised in the social sciences and even less so in the so-called sciences of nature.

We want to identify the historical, internal and especially intimate golden thread which will give meaning both to the objects of research and to the very subject who thought them, and thus to the "subjective roots of a researcher's position" as Florence Giust-Desprairies states (2003, 181).

Of course, as such, this elucidation is of interest only to the subject herself. But we suggest that it can go beyond the singular experience by illustrating how scientific activity (and its methods) is never as transparent as it claims, a sort of "noiseless" speech. Rather, it involves much more than what the "Social Science Methods" courses teach students in the classical description of what "techniques make to the object" or what is the subject-object relationships.

As early as the 1970s, Edgar Morin had his research accompanied by a daily journal (Morin, 1983), the object of which was precisely to show the retroactive relationship between objects and subjects through the example of his various volumes of "The Method" or the work of institutional analysis and thereby show how these implications largely escape a researcher's awareness. This implication is defined by Lapassade and Lourau as "the set of relations which the intellectual refuses, consciously or not, to analyse in his practice, whether it concerns relations to his objects of study, cultural institutions, family, money, power, libido and in general the society to which he belongs While the intellectual believes himself to be capable of analysing and objectifying what happens to others, including, at times, categories of intellectuals from whom he excludes himself (Lapassade, Lourau, 1971, 200).

In fact, twenty years of psychoanalysis brought me to work in an intimate way on the question of the power and psychic mechanisms of submission, that was thought not so much as voluntary servitude as a complex result of the internalization of social and mental mechanisms. These mechanisms have emerged in the aftermath of the singular transference / counter-transferential relations of the cure, although they have unconsciously nurtured my scientific work. How can what is played out in the dual relation of the cure – that is, a device with a clinical aim – produce effects as well as generate my analyses of the institution (academic as well as scientific) and my singular place in it? First of all, starting from a classic cure device, I

have to think about the psychic and political implications of my plural belongings in and outside the institution (Devereux, 2012) that defines the methods I use.

It is not so much that the psychoanalytic path has influenced my disciplinary choices and relation to knowledge as it produces a reflexive analysis of my singular and collective implications in the institution which are probably not without effect. "What is useful or necessary for ethics, research and the ethics of research is not the implication, always already there, but the analysis of the implication already in our belongings and non-belongings, our references and non-references, our participations and non-participations, our overmotivations and demotivations, our libidinal investments or non-investments..." (Lourau, 2013, 112). It is the encounter between biographical research and the temporal dimension that allowed me to go beyond the alternative of an individual grasped in their social structures and determinations and of an individual grasped in their affects and psychic functioning, in a quest for the writing of life and therefore in an incessant activity of biographical interpretation and configuration and thus in a putting of narrative reason into practice. Biography is a decisive framework in relation to training and knowledge, but also in the learning process itself and in the process of scientific research. This can also be defined as self-referential activity (Delory-Momberger, 2010).

This text is therefore the biographical narration of the inner world of the external world of scientific methods. For me, therefore, it will be a matter of taking a social as much as personal risk of putting into words and questioning, in a self-analytical text, my own research objects and methods as well as my career as a researcher, resistant to assignments and yet unconsciously caught like everyone else. If it is an instance of "*passage à l'acte*", it is in the sense that carrying out an act of mature reflection becomes a life necessity.

I have been a professor of sociology for many years. I have been leading a multidisciplinary and specialized research unit dealing with the educational question for the past nine years, but my path has not been linear, although it may be retrospectively deconstructed. Then I set up another research unit working on happiness in education and the methods for reaching happiness. Unfolding the same furrow in order to put in resonance the memory traces of a past is probably the common ground of all creative activity (cultural, literary, musical, and scientific) as if the same novel was always written, the same song always sung, the same picture always painted, the same research always constructed, but with small shades (Balibar, 2011) which testify to the way that it is constructed while walking - the devil - or - God - nestling in the details.

In my works, two main sequences echo themselves and go along in concert: a political Sociology of the School, born of a paradoxical injunction, and a sociology of the subject's response to the grip of the family system. Both have focused on a method of decrypting power as a mode of analysing reality.

## **2 From a paradoxical injunction to a Political Sociology**

The specificity of paradoxical injunctions or double-bind messages is that they are not lived as injunctions nor as ambivalences, and that they place individuals in a system of irreducible constraints and tensions from which they cannot emerge without being aware of the gap between what they believe to be rising from their own will and what binds them to an unconscious pact sealed in spite of themselves. For every organization is built on repressions and unresolved situations that have been left aside. "In couples, families, groups and institutions, alliances,

contracts and unconscious pacts support, above all, the destiny of repression and repetition” (Kaës, 2000). I therefore intend to show how my research work on knowledge as well as on power in schools and research institutions, and more broadly my particular relationship to institutions are due to an unthought – which is now partly being deconstructed – based on a paradoxical injunction.

## 2.1 The knowledge-power relation or the putting of the self into play

“Every individual social trajectory is part of a collective social trajectory, that of the social group to which the individual belongs through family mediation, so that to take cognizance of the first one would firstly involve reintegrating it into the second” (Legrand, 1993, 60).

As the product of the alliance between a family of teachers and a family of peasants, I was a very good pupil for whom School provided material for “resilience” or, more prosaically, a breathing space away from the constraints of domestic violence and subjections. My great-grandmother Germaine<sup>2</sup> was a teacher from 1910 to 1950, a sort of protective and affective referent for my mother, whom she taught to read and who herself became a teacher.

My maternal grandfather Candide, who was born in a very poor family of Valaisan peasants and immigrated to France in the 1910s, was a self-taught polyglot who had an encyclopaedic knowledge and built a relationship with knowledge mediated by affection and pleasure. He taught me the first rudiments of a foreign language using his own pedagogical methods, developed in this intimate relationship that autodidacts have with knowledge. All his life my father – a brilliant pupil of the segregating school of the pre-war and post-war years – carried the burden of the stigma of a schooling that could not be completed because of a lack of financial resources. High school was reserved for a social elite. Obviously, he lived it as a social injustice (that it is really ...) which he was never able to get over, even though he had only ceased to attain the social status and the recognition it confers by attending evening classes through internal promotion to administration provides. At the same time he thought of it as an enormous appetite for knowledge, reading, and a mistrust towards those who know and appropriate knowledge as a booty, or even as an instrument of domination.

My mother, who taught me to read very early<sup>3</sup> and was also my school teacher, was very proud of my good academic results and allowed me to build a relationship with knowledge based on sharing and pleasure. She accompanied my father on his long and courageous journey of compensating for his initial handicap (the lack of diplomas) he bearer of intimate cracks.

I thus inherited a paradoxical injunction of academic success: “Be successful but not too much! Study but be wary of people who think they know!” It was therefore necessary to pay for my too easy academic achievement with recurrent failures induced both by how the university institution functions as a place of power but also by my apparently rational choices. With my CAPES and external *agrégation* in social sciences in the bag, I began a thesis in the field of political science, a very “arrogant” thesis because it deviated from the rules of ordinary

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<sup>2</sup> In an inspection report dated 6 May 1936, it is stated that “Mrs Sébastiani surrounds her little world with kindness and attentive care. Several children are dressed in woollen clothes which she knitted herself during her leisure hours” (sic).

<sup>3</sup> And obviously I taught my two children to read very early – a little before they turned four years old – using a method of my own between affect and pedagogical technique, being guided by the idea that it would be more effective through game and pleasure than through school compulsion... not unlike the second research unit I manage.

academic functioning in many ways: First of all, I chose a thesis supervisor, who constantly had the right to bring to light the relations of power and connivance of the academic world. Also, the very subject of my thesis was very far removed from the academic expertise of my supervisor: an analysis of power relations in social and natural sciences research units. Furthermore, the thesis itself was completed without my belonging to any research unit since the university of that time had none in my discipline.

I proposed to conduct an epistemological-historical analysis of the construction of the scientificity of sciences which I conceptualized as peripheral sciences as opposed to central sciences. The thesis drew on data collected from science laboratories (through interviews, observations, and questionnaires). It gave a historicist definition of scientificity which showed that taxonomies constituted power issues in the sciences as well. I argued that what supported the scientificity of a discipline sprang from a twofold social and political process.

Drawing on the sociology of science from Callon to Matalon or Latour and Woolgar, and on the philosophy of science of Isabelle Stengers or Feyerabend, I argued that the scientificity of a discipline did not correspond to a state of fact but to the recognition of a conquered right and therefore relations of power(s) were involved in defining territories. The definition of disciplinary fields is built around issues, interests and political conflicts for the monopoly of scientific discourse, the definition of the legitimate objects of the discipline, and the power to ask one's own questions and assert one's own criteria of interest and scientific validity. The question of power and domination seemed to me central to thinking about the social division of knowledge. Interdisciplinary competition over objects, as well as the historical variability of the objects of each discipline, the nomadization of methods or the conceptual and methodological migrations show that the question of the subject of a discipline does not epistemologically make sense. No method is a divergent criterion. Every object is built by the discipline itself. Therefore scientific disciplines have a variable geometry because every science constructs its objects at every moment, but a scientific discipline proceeds from a disciplinary matrix widely shared by its practitioners who base its scientificity on it.

In a nutshell, I argued that science was what scientists called scientific – a less trivial tautology than it seems since I felt its effects in my career. To do this, it seemed to me central to ask the question of taxonomies and especially those that build a partition between exact sciences and social sciences. It was for me a nodal point in understanding the scientific mode of the social sciences. I used the Centre / Periphery<sup>4</sup> paradigm to designate them. Thus, I proposed calling the “hard” sciences – that is, the exact sciences and the natural sciences which are characterized by an epistemologically well-established mode of scientificity, a consensus on the stakes of the very high scientific field and a broad recognition of the necessity and effectiveness of these disciplines – the sciences of the “centre”. And I proposed calling the “soft” sciences – characterized by an apparent epistemological specificity, lesser social recognition and a more conflicting functioning of the scientific field – sciences of the “periphery”.

Contrary to Auguste Comte's ideological ideal of the evolution of scientific knowledge, the central sciences exert an ideological domination over the peripheral sciences by means of diffuse social factors (in the sense of Parsons), the ideal of knowledge being still widely and socially accepted as formalizable and/or mathematized. I rejected, with Pierre Bourdieu, the hypothesis according to which the sciences of the periphery would be characterized by an epistemological and/or methodological delay of scientificity compared to the sciences of the

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<sup>4</sup> Without being able here to detail this paradigm, its epistemological concepts of use, it is for me much more than a metaphorical analogy.

centre. The hierarchy between the sciences, which impacts their social and/or scientific legitimacy, is expressed through the relations between hard sciences and soft sciences, being an anchor for understanding the constitution of disciplinary fields and specific modes of scientificity. Thus, the central-peripheral model built to account for the disciplinary and social division of knowledge enlightens the relations of power(s), conflict(s) and consensus that determine what is central or peripheral in a given discipline. Thus, conflicts and consensus develop to define a scientific discipline. Indeed, the result of conflicts in the scientific field determines a temporary consensus at a given moment in the state of knowledge.

“It is through its internalization in the habitus of the “scientific community” that the centre-periphery model works. The model is also applicable to the social and disciplinary division of the sciences by an internalization in the representations and everyday practices at work in the sciences of the centre as in those of the periphery. If scientific practices are also social practices and if the epistemological practices of scientists, whether conscious or not, objectify interdisciplinary power relations, appropriation through conscious and unconscious internalization allows social mechanisms to function – the social effects of illusion are not illusory.” In my thesis I wrote, with the candour of a young researcher, an analysis that I myself would end up experiencing and talked about how scientific lucidity doesn’t give personal lucidity, or even so doesn’t provide the effective strategy, something which today I explain directly to my students.

My thesis further deviated from the rules because of the examiners I had: a multidisciplinary group made up, in addition to my thesis supervisor, of a political scientist, a sociologist, a lawyer and a professor of management sciences – an examining committee that was certainly in line with my first research work. –about Edgar Morin and his transdisciplinarity - but quite contrary to the disciplinary rules of university recruitment. The institution’s *petitiones principii* on the scientific fertility of multidisciplinary notwithstanding, “scientific research is characterized by a disciplinary hyperspecialization, conceived as a division of scientific work of a commercial type”, something which I did not hesitate to write in my thesis elsewhere. This thesis, as it was used in political science when the publication record permitted it, was accompanied by an HDR.

Finally, it deviated thanks to a first work co-written with my thesis supervisor, entitled “Is Political Science a Science?” and translated into Japanese and Portuguese, which ended with this naive as well as symptomatic sentence: “Let’s go, gentlemen political scientists, one last effort and you will soon be scientific!” (Bonfils-Mabilon, Etienne, 1998, p. 133). It questioned the scientificity and masculinity of the discipline, even though entry into the profession is first an act of dubbing, and what profession of political scientists don’t accept.

In fact, thanks to a CAPES and an *agrégation* in economics and social sciences, a thesis in political science critical of the discipline, and a multidisciplinary examining committee, I was therefore a UFO and blackballed from discipline to discipline (a sociologist for the political scientists and a political scientist for sociologists). It took me about fifteen years from the first CNU<sup>5</sup> qualifications to recruitment, fifteen years of a *tour de France* of examining and recruiting committees, ten qualifications in several disciplines, 3 HDRs<sup>6</sup> before finally finding a post as a lecturer in sociology. By then I had a list of publications on a level with a university professor, which is why I only spent two years as a lecturer.

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<sup>5</sup> The CNU is the authority that allows one to become a lecturer or professor.

<sup>6</sup> The HDR is the qualification that allows one to become a university professor.

These successive tests were, of course, partly the result of my choices and were enlightened by an ironic detour but not elucidated by the subject with its cracks, by the lines of force of the academic field which my thesis had brought to light, especially with regard to political science. This discipline is a peripheral science in quest of hardening (in the sense of Stengers) by means of quantification (notably in the case of electoral sociology and the induced benefits of the social demand for policy professionals in this field).

So I decided to take a second HDR in education sciences and there reiterated the fact that my works were not identifiable in any discipline: was it political science or education sciences? Or sociology? To tell the truth, to this day I still have no answer as to which discipline my works belong, unless we can also count philosophy or psychoanalysis – this is sometimes determined as much by the disciplinary areas of interest of the journals my papers are published in as by the latter's content... I even ended up writing a third HDR in sociology, which I finished but did not defend as in the meantime I had been made Professor of Sociology, even though I did not hold a degree in this discipline.

Together with other colleagues, I was behind the creation of a research unit specializing in the field of education. I proposed a horizontal governance structure whereby the decision-making council was composed of all the unit's academics and elected representatives of the other categories of members. It was an atypical organization because generally a research unit council is made up of only a few members. The aim of an academic organization by flexible workshops was to break with the mandarin practices that are still frequent and structure research around professors alone. I thought of my role as the head of the research unit as a leader of early societies as described by the anthropologist Pierre Clastres. Early society is essentially an egalitarian society and the leader only has a diplomatic role, must maintain consensus, and does not really exercise political power. He also sometimes acts as a mediator when a dispute arises between tribe members. He must be generous with his goods. This organization worked for a few years but was then overtaken by the relations of power and territory, thus failing in my quest for sharing and deliberations.

If this course belongs to me, it is because it involves this paradoxical family injunction: not competing with the father, being critical during my thesis of my "peers" without their being "peers", and neglecting the effects of power on subjects. But also shedding light on the injustices caused by schooling. In fact, all the textbooks and books that I have written to help prepare for secondary- and higher-education examinations (Mabilon-Bonfils, 1999, 2002, 2007, 2010) have only one frame: showing the implicit principles of selection and giving those who do not have the social and cultural prerequisites the keys to decrypting these unsaid rules and conventions that determine success in competitions and examinations.

This professional career therefore belongs to me and questions the scientific nature of the social sciences but also raises the question of power which I decided to put to work in schools.

## **2.2 School as a place of exclusion and powers**

Without discussing here all my works around school, they all deal with the "school question" as a twentieth-century social question.

In a non-reflective way then I wrote in “School in crisis”<sup>7</sup> *without knowing that I also spoke of me: “School is at the crossroads of individual stakes (learning, forming oneself, socializing oneself, obtaining a diploma, etc.) and political stakes (socializing the younger generation as Emile Durkheim would write, integrating citizens). We are all former pupils and the traces of this past – whether excellent, good, mediocre or bad – are often reflected in our peremptory opinions about the “school institution”, impairing our lucidity when thinking about it. Education is about trope, affect, human relationships, pleasure, suffering, boredom, relationships with the others, encounters with the other sex, the other age, the social other, the cultural other, with difference. This passage will mark our personality. A lot of meaningful encounters will take place during schooling, whether it be meetings with people, knowledge, works or experiences... So we must try to take some distance from our affects, testing the knowledge that the social sciences produce about school, questioning these commonly held and widely circulated opinions about school. We must enlighten the collective French nostalgia around the myth of the school of the French republic.”*

In many of my articles and books I tried to show how in our French political history the contribution of the modern state to the birth of the citizen has become an ideological instrument – that is, the School – for inculcating common values. The invention of the republican school is part of a project of collective citizenship: working on the unitary construction of French citizenship conceived as a denial of particular allegiances and as the founding place for neutralizing environments by homogenizing, constructing one knowledge for one nation. In short, the Other must imperatively become the Same. F. Giust-Desprairies (2003), analysing the figure of the Other in the French republican school, shows that the republican model articulated around the ideas of universalism and abstraction has induced teachers defenses, as representations, which prevent them from accepting students as they are. The subject (and, she states, especially the subject teacher) internalizes social representations until they become so distant from reality they can no longer play their role of psychic support, leading to the sideration of imagination.

In line with Bruno Etienne’s work, I questioned the forms of the domination and reduction of otherness – school being one of the perpetrators –, forms of exclusion from school, dropping out, social and ethnic discrimination, and the excesses of secularism as an instrument of minority aggression. These were all themes rooted in my singular family and intimate journey.

Like any institution, the school produces norms and rules which, of course, permit community life within the institution but largely encroach on the freedom of individuals. Entry into language (including that of the body), that is to say, into the social, is conditioned by a sacrifice, probably that of speech. Partial renunciation of enjoyment is necessary in order to live among the others, which is expressed by the desire to hold all pedagogical relations inside, starting with the sacralization of knowledge and the imposition of a power-knowledge relationship to the discourses exalting the rewarding of merit and toil (sometimes even saving boredom) as virtues of imitation.

It took me years and years to grasp the mental toil of the paradoxical injunction which gives meaning not only to my trajectory of academic success and successive professional failures, but also to my choice of research objects as well as the way I question. This paradoxical

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<sup>7</sup> AUTHOR., *Ecole en crise* (2008) éditions ellipses

injunction can be formulated in this way: be successful in School, but not to the point of being “on the side of those who know” who are so unbearable to paternal “mythology”.

An original paradoxical injunction therefore functioned as a stimulus, and it was therefore through the question of the play of desire and power that I entered the school question.

### **3 From the grip of the family system to a sociology of the subject**

Subjected to a system of powers, I was working to understand teenagers’ ways and means of escaping the grip of the school institution in order to construct a sociology of the subject.

#### **3.1 School of Social Control**

Without going into overly intimate details, my childhood was marked by psychic violence and power in a patriarchal closed-family system that organized life around a system of obligations and constraints, diktats and confinement, fears, obedience and silences that denied all otherness. There was only one word that counted, that of the father, but it was never reliable and was riddled with uncertainties. At any moment an outbreak of violence could occur and there was therefore no ontological security. The Other did not exist. This resonated/reasoned singularly with the analysis of republican monism and school as the armed branch of this monism...

But there was one discrepancy that has made putting traumas to work meaningful and probably helped me live with them, if not overcome them. School was, for the good pupil that I was, a place of resilience. *“School is an institution that has a special place in collective and individual memory. It is often the first place of separation from the mother, one of the first places of socialization, learning norms and social values, the first place of the relationship with otherness, competition and solidarity; one of the first places of suffering too.”* For me, it was the place of security, of sharing (as a pupil in the school where my mother taught), of the relation to the other (the closed family was broken), and the place of success...

Are our choices of objects (as scientists) commensurate with the choice of objects such as Freud speaks of? Unconscious determinations are necessarily at work. Obviously I had to go this way: to grasp the mechanisms of power, or at least go in search of some of these mechanisms, especially when they do not entail any physical force, but rather an intellectual displacement that questions me to this day.

My second HDR – nourished by La Boétie, Rousseau, Foucault, Castoriadis and others – is symptomatic of this interrogation of obedience. Power can be defined as a balance of powers, out of which one can get more than the other, but where the other is never totally deprived of the one. There is no power without belief. If political resources designate the means by which political power lies and strengthens its domination, no political power can operate solely on the basis of violence and coercion... Power then uses physical or symbolic violence in a legitimate way. In the conception of power which we have adopted in our work on the critical sociology of politics, power cannot be defined as an appropriation, but rather as an asymmetrical relation to which each person participates knowingly or not, and as a individually or collectively. strategy.

A whole part of my work questioned the symbolic violence of the school institution which I can’t develop here.

It is indeed the question of power that is today at the centre of my work, this power lived in my body, even in the family sphere, but displaced in the Freudian sense onto the school sphere. School in my psychic economy is at one and the same time a place of sharing (with my mother teacher), of freedom (even though intellectually, as a researcher, I analyse it as a place of social control par excellence), a place of reliance, of certainty, and of safety (for a child without any secure attachment).

A shifted reading – like the one I am trying to do now – causes vertigo (in the case of psychic violence, the mechanisms of submission, etc.) so much so that it is almost possible to argue that a shifted reading of one's own work – if not an analytical one, should this be possible even after long analysis – would be equivalent to analysing a dream or a slip of the tongue...

My work on the different ways in which the schools asserts its grip is therefore a continuation of my own subjective path.

### **3.2 Grip, suffering and ruses: towards a sociology of the subject**

So I worked on school relations as a relationship of control, drawing on the work of R. Dorey (1981) in which the grip takes on meaning only in the field of intersubjectivity as a “relationship of influence”. Dorey distinguishes three semantic meanings: The first, in line with the term *Bemächtigung*, evokes the idea of capture, of taking. The second is, from an interpersonal point of view, an act of appropriation by dispossessing the other, of dominance, an exercise of a power by which the other is subjugated, controlled, manipulated. Finally, the third is the consequence of the double action of appropriation-domination: it is the inscription of a trace, the impression of a mark. The one who exercises one's grip leaves one's imprint on the other that is marked physically and psychically. The relation of hold thus refers to the fundamental impossibility of accepting the other in its difference.

The other is denied as a desiring subject, which as such is characterized by its singularity, its own specificity, and the very idea of its desire is unbearable. The grip therefore reflects a very fundamental tendency towards neutralizing the desire of others, that is to say, reducing all otherness, all differences, to the point of abolishing all specificity. The aim is to bring the other to the function and status of an entirely assimilable object. Thus the relation of control, whatever shape it takes, represents a true defensive formation make it possible to conceal the lack revealed by the encounter with the other. This scenario gives rise to rules shared by all relations: the instrumentalization of the other and the impossibility for it to break the cycle of exchange in which it gives more than it takes. Rupture is constructed as of the order of the unjustifiable and assumes an act of forcing things through, of rebellion or violence.

The concrete manifestations of the relation of control are expressed in the sacralization of knowledge by the School and by the knowledge-power relation (Mabilon-Bonfils, 1998) through discourses exalting requirement, merit and toil, constructing a particular form of academic excellence which aims to change the other through submission or through relations that are often of the transferential type when the “pédagogue” – the one who shows the way – is experienced in being a model to be imitated.

No institution, however regulated, can function without leaving its actors room for freedom and improvisation. Between subjectivization and control, School is Mabilon-Bonfilsized by many ordinary arrangements to which the pupils respond by means of resistance, adaptive ruses, veritable strategies of surviving a school in tensions and movements. I proposed to construct a sociology of the subject or a sociology of affects (Mabilon-Bonfils, 2015).

I conducted field surveys on the plurivocity of school rules as well as on adaptive ruses, resistances and survival strategies (Mabilon-Bonfils, 2013) and lateral tactics (Mabilon-Bonfils, 2012) used by adolescents in school as well as by “ordinary” suffering pupils (Mabilon-Bonfils, 2011), on how they cannot express themselves and the mechanisms of this denial that were mine in a different way as well as the daily inventiveness of tactics used to bypass rules and escape the grip of the family.

So, two years ago, together with my colleagues, I created a research unit whose central question is how to promote happiness in education, what methods are used to achieve it, and what is the best way to achieve it. Its objective is to come up with scientific methods that can be used to test the ingenuousness of happiness in education.

Individual biographical constructs are closely dependent on socio-historical and cultural contexts and on the biographical models and programmes they carry (Delory-Momberger, 2005). Every researcher is anchored in an age, an institution, and relations of power which affect their choice of object – almost in the sense in which Freud speaks of the choice of objects of desire – and model the methods which they think valid and circumscribe their territory. But to this banal formulation of subject-object relations it must be added that, as singular subjects, we all have a history, a biography that we will put into operation whether we know it or not, whether we want to or not, in our most “hardcore” works, that is to say, those that use the most quantitative methods. In other words, any research informs as much about the society that produces it and the researcher that sets it to music as about the precise object of that research. “We, observers,” writes Watzlawick, “distinguish ourselves precisely by distinguishing what apparently we are not, that is, the world” (Watzlawick, 1988, p. 356), but of which we are undoubtedly a part.

What if behind the blinding proximity of the real, research activity was at first abyssal?

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