Integrity or academic heuristics? : a journey through social sciences

8

INTEGRITY OR ACADEMIC HEURISTICS? A JOURNEY THROUGH SOCIAL SCIENCES

François Vatin

1. Introduction

What is integrity?[*] "The state of a thing that is whole", as *Littré* put it in 1863. In the deepest sense of the term, it means being true to who you are, that is, to your nature. Respect for academic integrity therefore consists in conforming to one's "academic nature". If we do not want to give such an idea an overly metaphysical meaning, in the pursuit of an ontological definition of the academy, we must understand the idea of "nature" as the "characteristic property", in the mathematical sense of the expression, of the object we are examining.

In this paper, I will argue that the characteristic property of the academic institution is heuristics, the active pursuit of new truths—in short,

---

[*] François Vatin is Professor for the University of Paris-Nanterre.

How to quote this chapter: Vatin, Francois, "Integrity or academic heuristics? A journey through social sciences", in: Bergadaà, Michelle, and Peixoto, Paulo (Eds.), *The New Boundaries of Academic Integrity*, Geneva: Globethics Publications, 2024, pp. 169-187 DOI: 10.58863/20.500.12424/4307055.

research—and that it is against this yardstick that the various dimensions of this institution must be viewed, including the teaching mission that appears to dominate it. It is in this light that we can understand the crisis of academic integrity that many professionals seem to feel exists. But I conclude from this analysis that the solutions that are generally promoted to remedy this crisis are not only inappropriate but tend to reinforce the evil that they purport to combat. They contribute to a growing bureaucratization of the academic institution, which runs counter to heuristics.

In this highly allusive essay, I will proceed in two stages. First, I will outline a professional sociology of the world of research in order to justify the definition of the academic institution that I proposed above. Second, due to a lack of space, I shall confine myself to a single example presenting one dimension of the bureaucratization of today's academic institution: the development of the "laboratory" model, the evolution of which I shall examine in French universities from the end of the 1970s, when I started out, to the present day.

This chapter does not claim to be systematic. It aims to provoke thought and—why not?—reaction. It is written very freely, without any critical apparatus, and is largely based on my own experience. There is a bias in this approach, which is unfortunately present in all works on the French academic world. For several decades now, we have been witnessing a worldwide transformation of university organization and practice, which has been much studied and much deplored. This has had an undeniable impact on French universities. However, the French academic world has unique characteristics and shortcomings that are not rooted in this global movement. Nowhere else is there such a tripartition between "universities", *"grandes écoles"*, and "major research organizations". A singular history, dating back to the nineteenth century, has led to universities in France becoming the poor cousins of the academic system.

The existence of full-time bodies of researchers has pushed academics back towards the ambiguous status of "teacher-researcher", to which I will return later. At the same time, the obligation for French universities to admit all young baccalaureate holders without discrimination, on the grounds that the baccalaureate is historically the first university degree, has led universities to become the "sweep vehicle" of higher education, responsible for taking in students who have not found a place elsewhere.[58]

It is impossible to study the development of French academic institutions without placing them in a global context, and in this respect, studies which combine different national experiences, as this collection does, are useful. But it is a mistake to consider academic situation as the ordinary expression of a more general trend. The specific characteristics of the French university, which are briefly mentioned here, give a particular colouring to the crisis of academic integrity, as we shall see throughout this chapter.

2. Researchers and their Research

Research is a professional field like any other: it is made up of men and women whose lives are shaped by a system of norms and sanctions and a culture forged over time. As in any professional space, these norms are multiple, both formal and informal, hierarchically stacked, from the level of actual research collaboration spaces (often informal and multiple for a given researcher) to the law, through different structures that vary over time and from country to country, such as laboratories, universities, major research organizations, etc. These multiple social spaces are riddled with interpersonal tensions. First of all, researchers are competitors to some extent in a hierarchical system: recruitment and promotion hurdles have to be overcome; there are positions to be filled at the head of

[58] From my many publications on the subject, I shall cite only Vatin (2016) here.

the various bodies; researchers also have "offspring" to look after —their doctoral students, for whom positions must be found...

But, as in geopolitics, it would be a mistake to think that "interests" alone can explain conflicts. The paradox of competition, in this professional field as in any other, is that the closer two people are, the more intensely they compete with each other. These two contradictory tendencies (proximity and competition) can result in alliances as well as conflicts, and often both in turn. In this respect, we need to ask ourselves what "proximity" means in the world of research. Two people are "close" because they share a common history, common points of reference, and a convergence of scientific interests and questions. Hence the importance of generational effects in particular. It is with colleagues of your own generation that you are most immediately in competition. But generations can also form informal bodies that combat each other.

I was born in 1957 and took up my current position in 1982. I belong to a special generation in the French university system, caught between two phases of its "massification": that of the late 1960s (before and after 1968) and that which began in the mid-1980s (after the Savary law of 1984). Twenty years ago, when the large numbers of academics recruited between 1965 and 1975 had not yet retired, the age pyramid of French universities showed a great divide like the one that marked the French population after the 1914–1918 war. As an isolated member of my generation, I spent most of my career in the company of colleagues ten years older than me, who kindly treated me like a "little brother". Their dominance in the institutions was guaranteed by the numerical weakness of the generation that immediately followed them and whom they had no reason to mistreat. When this population retired en masse in the early 2000s, the generations recruited in the 1990s and 2000s had gained confidence. They intended to occupy the positions of power, and the meagre battalions of older people still present were unable to resist them.

A symbolic event took place for me in 2012, when, for the first time, the elected president of my university was younger than me. At 55, I had crossed over an invisible bar.

If you think about it, this is a very common occurrence, because it is at this age, in their fifties, that managers in large private companies retire when they are seen to be "in the way". In a pyramid system, the number of available positions decreases with the length of a career. If you can no longer climb the ladder, you become "dead wood". In private companies, there are means to prune the dead wood, but sometimes there are also "side tracks" where this population can be shunted aside. The protection provided by the civil service prohibits this type of treatment, and the French universities are also short of "side tracks". (What do you do, for example, with a colleague who is no longer able to fulfil their teaching duties, for example?) As a result, the ostracism is more discreet, but no less real.

In just three paragraphs, I have discussed the academic profession, its history in France specifically, and my own career in this context, in the ordinary terms of the sociology of professions. In so doing, I have neglected the specific characteristics of this profession and its purpose: the production, preservation, and transmission of knowledge. This is because I intend to approach the profession without *a priori* consideration of its professional ideology. Every profession develops its own ideology, and this may even feature in charters that have legal force, as is the case with doctors, who have served as an archetype for the sociology of professions. But the sociologist must study the activity of doctors, lawyers, or police officers for what it is, and not by thinking of it as the implementation of their declared professions of faith. This is where regularities emerge that cut across the different professions. There are, however, determinations that are specific to each professional community, because they depend on the nature of their activity.

In my view, the purpose of academic activity is research, and not, as people tend to say today, research and teaching (Beaud & Vatin, 2020, 2021; Vatin, 2020). Not that I am neglecting the teaching function—and I believe that I have been a fully-fledged professor, fulfilling my teaching duties for 40 years—but because university teaching is "backed up" by research. I could be criticized for changing the register here, by considering what the university should be and not what it often is in France today: there are academics who don't do research and, even among those who do, many consider their teaching mission to be independent of their research mission. This is a state of affairs that has become increasingly apparent over the last few decades as a result of the role that has been assigned to universities in France: that, as I have said, of the "sweep vehicle" of higher education, responsible for taking in the baccalaureate holders who are least able and least willing to follow an educational programme based on research. A poisonous status quo has developed between academics and public authorities: the latter allow the former to devote themselves to research, thanks to the benefit of a limited teaching service, provided that they agree to accommodate, whether well or badly—which is hardly considered—the population that is sent to them. So, despite the growing gap between teaching and research functions, we have not yet formally touched on the definition of the academic profession.[59]

"Research", which defines the academic profession, appears in a dual register: either punitive or a leisure activity—either a chore or a hobby.

It has retained its symbolic value for careers—recruitment, tenure, promotion—and this is the subject of recurrent criticism from those who consider that it is taken into account excessively to the detriment of "pedagogy" and the administration of the institution. Those who take this

[59] One might wonder how long such a flawed compromise can be maintained, but that is not the purpose of this paper.

line feel they are being "punished", hindered in their careers, even though they see themselves as the true professionals who are of value to the university, as opposed to the egotistical careerists who abandon the thankless tasks of teaching and administration to focus on their careers, secured by publications in "good journals". They see research as irrelevant to real academic work, as nothing more than an artificial means to achieve distinction, like the calligraphy of Chinese mandarins. As a result, measures are regularly put in place to calm the professional dissatisfaction of academics who are hostile to research, particularly those who have opted for a career as an administrator, which brings them into close contact with the political and administrative authorities in higher education.

However, a different stance has been adopted by academics who have accepted the division of their time between research, teaching, and administrative duties, and who are aware that research is not the royal road to a career that previous generations have denounced. They regard the time they devote to it as private and personal. The great freedom given to French academics means that they can do many things: supplement their income, which those who have the opportunity to do, for example in law or management, are more than happy to engage in; "cultivate their garden"; or, if they wish, do research. This is how it is understood by the ordinary public who regularly ask academics about their "working time". As soon as we try to explain that it is not limited to the time spent in front of students, the response is, "yes, but you are working for yourself". It is difficult to make people understand that the academic vocation does not fit easily into the ordinary salary system, since it is based on a principle of freedom that is a constituent part of research work. Historically, it has been a "burden", not a job.

With this in mind, we need to examine the increasingly restrictive bodies and procedures that govern research activity, which have often been introduced on the grounds of academic "deontology". The aim of these mechanisms is to manage the academic profession in its formal

relationship with research, which reinforces their careerist and punitive nature: "professionalize" research by imposing common standards on the way it is funded, produced, and disseminated; determine the duties of researchers; set up arbitration procedures in the event of conflict, particularly between doctoral students and research supervisors; etc. The only way to escape is to ignore the professional stakes of research and rediscover the simple pleasures of the hobby.

In this institutional arsenal, the question of the vocation of research, namely heuristics, is ignored. The essential thing is to ensure that researchers comply with procedures and that their time has all the characteristics of salaried, constrained time. Nobody really cares about *what* they produce. Here, as elsewhere, work is seen as a penalty, not a product (Vatin, 2014). This state of affairs is a feature of procedural systems, which are based on the belief that a system of standards, obligations, controls, and sometimes incentives can improve the service provided. Of course, this is by no means self-evident, because every new standard produces new deadweight effects and incites new ways of getting round it, and because, however well-intentioned it may be (and it isn't always), making the regulatory system more cumbersome has a disincentive effect in itself and, which we cannot reproach it for, encourages conformism, which is contrary to the very spirit of research, that is, innovation. The result is that "real" research, the kind that is carried out freely and for pleasure, is often done on the margins, or even against established, funded, and controlled research. Anyone who has read Alexander Zinoviev's *The Radiant Future*, a cruel satire of social science research in the late USSR, would not be surprised.

3. The Laboratory Model: Its Successes and Its Perverse Effects

When I started postgraduate studies in economics at Aix-en-Provence in 1978, laboratories were still in their infancy in the social sciences. But there was one in Aix: *Laboratoire d'économie et de sociologie du travail* (Laboratory of the Economics and Sociology of Work; LEST), a laboratory owned by the CNRS (the National Centre for Scientific Research) itself; that state of affairs has now virtually disappeared. They didn't want me there, despite my academic achievements, because politically I was not in good odour; it's funny when you think that political radicalism is favoured nowadays for a career in the social sciences in France. For the rest, the creation of small teams meant that every teacher had a secretary. I joined one of these teams, where my very relative "radicalism" (my Marxist culture, forged on the fringes of official education) was welcome: that of Jean-Paul de Gaudemar, a Polytechnique graduate who was then a Foucauldian-Marxian, and who went on to a career as a senior civil servant, at DATAR (the Interministerial Delegation of Land Planning and Regional Attractiveness), and then as a university rector.

Was this a "laboratory"? Around the young professor (he was 31 at the time), apart from the secretary, there were only two statutory staff members, both of whom had transferred from LEST: a CNRS researcher who soon resigned to work in the private sector and a research engineer, a great but isolated scholar, who had published practically nothing. The team was completed by a number of doctoral students who, like me, were in fact affiliated with the École des hautes études en sciences sociales through a partnership. The intellectual cement of the team was publicly funded research into development of the Fos-étang de Berre area. The work of the doctoral students revolved more or less around this theme. This was the case for me, as I was working on the oil-refining

industry at the time with funding from the *Commissariat Général du Plan* (General Planning Commission). This research was managed by a private consultancy, of which there were many at the time, with the consent of the public authorities. It was run from Bandol by a professor of economics in Paris, who was also the Socialist mayor of that village in the Var. I then benefited from one of the first waves of "research grants", which have since become "doctoral contracts". Those who deplore the "casualization" of young researchers should note that these contracts only lasted two years, that the compensation was less than the minimum wage, and that there was no unemployment benefit after they ended. Forty years later, the Ministry still refuses to count those two years as part of my career!

The true scientific space was in fact the "seminar", a ritual ceremony held every fortnight, which brought together, around a large table, sometimes in double rows, in addition to the small team described above, some dozen researchers from different disciplines (economics, sociology, philosophy, law), either in academic positions or *hors-statut* (unofficial), as we used to say at the time to designate researchers who worked, like me, under a public contract but with private status. The presentations were given by the regular participants or by guest researchers from Paris or elsewhere. A document was sent out beforehand and the professor had instituted a ritual, taken, he told us, from the Keynes seminar: everyone had to react in turn, following the seating plan, starting at a particular point, but I no longer remember how that was determined. The exercise became increasingly difficult as the session progressed.

Although I can't say that, strictly speaking, Jean-Paul de Gaudemar "directed my work", I learned a great deal from this seminar, in terms of both content and form, which I have more or less reproduced throughout my career. The idea of a master, with their journeypersons and apprentices, is an "establishment" type format, as the traditional organization of

the "profession" is pejoratively referred to in the university. We often had small meetings in the morning devoted to collective research and we lunched together. We were all quite young—the professor was no more than 10 years older than me, and I was one of the youngest in the group—and we addressed each other informally. This was a very classic academic way of working, a far cry from the "laboratory" approach that was later prescribed and to whose development I contributed in the two establishments to which I belonged.

In 1982, I was elected assistant professor of economics at what was then called the Université de Haute Bretagne (UHB), which later became the Université de Rennes 2. It was in fact the former Faculty of Arts, without its philosophy department, which had joined the sciences, medicine, law, and economics at the Université de Rennes 1. At the UHB, an education and research unit (UFR) was set up around the "economic and social administration" programme, which mainly brought together academics who had transferred from Rennes 1. They were economists, political scientists, lawyers, and even statisticians and mathematicians; they had fled the academic order in their home faculties, particularly those who belonged to the *agrégation du supérieur* (higher education certificate) disciplines (law, political science, economics, and management) but had decided not to take this competitive examination, which was then the only route to a professorship in these disciplines. For this reason, the idea of research was perceived within this small world in the strictly punitive sense I mentioned earlier. In our departmental meetings, the word "research" was explicitly forbidden, although this did not prevent some people from indulging in this vice in private.

It was, however, to set up a research team on the theme of work that I was recruited to this university at the age of 25. The small group of economists who dominated the UFR had felt the need to revitalize their community through research. I already had a substantial publication record (more in sociology than in economics) and experience with contract

research. What's more, I was fully satisfied with the project. I had to "feed" the team by responding to local and national invitations to tender on the subjects of unemployment, vocational training and poverty, which the public authorities were concerned with at the time. Later I was able to develop a more personal research programme on the history and transformations of the dairy industry, from Brittany to sub-Saharan Africa. A regular seminar cemented the group's activities.

The group grew in size and attracted lawyers, sociologists, and psychologists from beyond the field of economics. A merger was arranged with another team of sociologists organized around a recently recruited professor. We set up a graduate studies programme and began to supervise theses (I supervised two before leaving Rennes in 1992). The Ministry, anxious to organize social science research in Rennes as part of the creation of a *Maison des Sciences de l'Homme* (a research organization with laboratories, etc., focusing on the social sciences and humanities), asked the other two sociology teams at the university, each organized around a professor, to join the large laboratory that was being set up under my direction, so we could pool resources. This provoked a conflict that I was not involved in, since it was between sociologists and I wasn't one at the time. But it did lead to my leaving Rennes, because, even though I was prohibited from becoming a professor of sociology in Rennes, I was appointed as exactly that at Nanterre in 1992. The Maison des Sciences de l'Homme was finally set up, without me and without sociology.

I believed, and still believe, in the dynamics of collective research. In Rennes, I was able to set up a fruitful embryonic "laboratory": shared premises, secretarial services, the purchase of the first computers, the creation of a library, a regular seminar, annual study days leading to a publication, research in line with "social demand", the possibility of bringing together young researchers by funding their theses. But I was also able to

observe, without fully realizing it at the time, how toxic the desire for bureaucratic standardization of research ultimately was. For what is commonly referred to as "the Ministry", which in fact means those of our colleagues who hold bureaucratic responsibilities, the issue was that everyone should have their place in a box and that each box should be neatly arranged next to the others. I say this with less irony than you might think because these "bureaucrats" supported me and at the time I generally shared their vision of the world. It simply seemed difficult to do social science research without such an instrument. The French university is a curious institution. It recruits staff, meaning that it commits to paying salaries for several decades, without providing any resources whatsoever, not even an office or a telephone. At Rennes 2, we had instructors' offices, but many Parisian academics still don't have offices. Without specific funding for research, it was impossible to buy computer equipment, carry out fieldwork, etc. The establishment of solid funding agencies was a necessity in this context, and it seemed to me that they ought to be associated with a shared intellectual life: a laboratory, in short. It took me a long time to realize the extent to which these institutions could be not only useless (in the form they took), but even toxic because, by their very nature, they are "hotbeds of conflict".

When I arrived as a professor of sociology at Nanterre in 1992, the organization of laboratories was still in its infancy, which led to major disparities between academics. Those who worked in teams recognized by the CNRS had a minimum of working resources: not a personal office, but the possibility of access to a telephone, a photocopier, secretarial services, and some budgetary resources, which the others did not. The enrolment of academics in laboratories, combined with an undeniable improvement in the availability of premises thanks to new buildings on campus, has made it possible to reduce these disparities, at least in

part.[60] In recent decades, French academics have gained access, albeit imperfectly, to the logistical services that should have been associated with their recruitment, via the laboratories, which are often placed under several supervisory authorities and therefore benefit from several sources of funding. But I am sure that there are still many academics in France for whom the classroom and the secretarial staff are their only material ties to the institution.

At Nanterre, as before at Rennes, I have contributed to the structuring of the professional space for academics, in the teams to which I belonged and which I directed, but also as a member of the scientific councils and their offices, as director of a doctoral school, and as a member and occasionally chairman of evaluation committees for the High Council for Evaluation of Research and Higher Education. I honestly saw in this dynamic an instrument to encourage the development of research, to enable its "steering", which I envisaged as very "liberal", and to ensure greater equity between colleagues. It was only gradually that I became aware of the perverse effects of this excessively rigid organization: tension within laboratories, tension between laboratories, universities closing in on themselves or in groups within the confusing governance structures of the Communities of universities and institutions.

Experience has shown that laboratories are first and foremost places of confrontation between bodies and, in particular, of exacerbation of conflicts between CNRS researchers and faculty members. There's a lot to be said for this, but I'll just mention one essential difference between the status of the two types of staff members: a CNRS researcher "walks around" freely with their position. Of course, they have to be accepted by

[60] These disparities have only been partially eliminated, because the CNRS has sought to concentrate its staff in a smaller number of teams, increasingly located in a smaller number of establishments, and the universities are not in a position to compensate their own teams for the lack of this material support.

the laboratory where they want to go, but the lab will generally be happy to have "captured" a new researcher. Faculty members, on the other hand, are confined to their own university and have very little freedom of "movement". Laboratories also polarize conflicts within disciplines, as they compete to attract new members; in fact, sometimes their very survival depends on it. Finally, the budgetary autonomy of universities, following the law on Liberties and Responsibilities of Universities of 2008, has generated conflicts between disciplines over the allocation of resources, as it is extremely difficult to establish criteria common to all disciplines. As a member of the Scientific Council at Nanterre when this affair played out, I was in the midst of this turmoil.

Organization into laboratories creates the fiction that active scientific communities exist, the contours of which are also invoked in connection with "conflicts of interest" in the various committees (recruitment, promotion, funding). This is, of course, absurd, because the real similarities, including in terms of scientific collaboration, tend to cut across the boundaries of the laboratories. And this is a healthy situation, because these areas of collaboration are constantly being created and dismantled over the course of an intellectual career. For a long time, I thought that this was specific to the social sciences, but my discussions with colleagues from the "hard" sciences and the work I have read on the sociology of scientific work have shown me that this is not the case. On the whole, except when they are first set up, laboratories are intellectually empty shells, mere bureaucratic nodes dispensing meagre resources. Their fictitious nature is of course all the more striking when they are expanded, in accordance with the wishes of the bureaucratic institutions, since it simplifies their regulatory work.

Another perverse effect of laboratories and the institutions that have been grafted onto them in a bureaucratic pileup—doctoral schools and doctoral colleges—is the perversion of doctoral work and its supervision. This is rooted in the denunciation of the "establishment", which was

asserted in 1968 in a "revolutionary" spirit and then maintained within a bureaucratic framework. What is the "establishment" if not, as I argued above, the expression in the academic world of the logic of the "profession", that is to say, the power, based on experience, of the master. There are undoubtedly incompetent instructors, and others who are obstructive, but the principle of the profession has the essential function of organizing knowledge transmission. By abolishing the profession, we do not abolish the hierarchy, but we replace a traditional hierarchy, which guarantees transmission, with a new, rationalized hierarchy: in a word, a bureaucratic hierarchy. Generally speaking, the latter has its merits, just as the former has its faults, but it applies very poorly to research work for the reasons I gave earlier.

In my opinion, the development of bureaucratic organization has been most toxic in terms of thesis supervision: dilution of responsibilities in co-supervision; collective control within the laboratory, and then the doctoral school, over the progress of the thesis; setting up of monitoring committees; and so on. It is difficult to go into detail about all this. It is difficult to go into all this in detail. The aim is to encourage the "production", in the industrial sense of the word, of theses, that is, works that are standardized in form and in the time taken to complete them. We were fighting against very real abuses (when I took up my position as director of the doctoral school, I found in my files doctoral students who had been registered for more than 10 years—the record was 20 years). But once again we were confusing the ends with the means. A doctorate is a piece of research, and the only relevant judgement at the end of the day is whether it has advanced knowledge. "Producing" theses is of no interest in itself.

4. A Step Forward

This brief account shows just how far French universities have come since I started working there in the late 1970s. The profession has undoubtedly become more professional, the material environment has just as undoubtedly improved, and the institutions have been developed, systematized, and rationalized. Has science benefited? The answer is probably no. Similar conclusions could be drawn from an analysis of the change in the publication regime and, in particular, the evolution of academic journals. Bureaucracy has flourished everywhere, which is undoubtedly the price of massification, because, at a certain scale, informal arrangements are no longer possible. But this massification was desired at all levels, as I have just shown with regard to the size of laboratories, because it was seen as a guarantee of efficiency. That's not necessarily wrong, but we need to ask ourselves what kind of efficiency we're talking about here and how it can be measured.[61] Degrees can be awarded without training students and theses can be defended without producing knowledge.[62] Similarly, we publish articles that are no more than "CV fodder" to organize the careers of young researchers. The means have become the ends.

It is in this context, and precisely to combat the phenomenon I am denouncing here, that the problems of academic ethics have arisen: controlling plagiarism, the strengthening the rules relating to the composition of various committees for recruitment and promotion, protecting doctoral

---

[61] On this topic, I would like to refer you to the book I edited (Vatin, 2013).

[62] This is nothing new. For a long time in France, law theses were pure exercises designed to obtain a title, and this is still the case for medical theses. But these were the conditions for the reproduction of professional bodies of doctors and lawyers, who were not presumed to be 'researchers'. With the exception of medicine, today's theses are supposed to represent research work and, in France, have no meaning outside the academic world, since they are not related to professional accreditation.

students against the risk of a supervisor's malign influence, having peers monitor the work of supervising a thesis: in other words, the many mechanisms for monitoring and standardizing academic conduct. The reason why plagiarism, on which so much attention has been focused, has become such a problem is that publication, from the dissertation to the scientific article, has become an end in itself, "CV fodder", irrespective of its content. To compensate for the perversion of the academic system, which consists in publishing articles that no one reads, metrological systems have been invented to count citations, which should enhance the value of publications that "count". But these, as we know, can also be exploited.

We are therefore repaying evil with evil, in a downward spiral, because we are refusing to think of academic activity itself in its primary meaning, what I have called its *heuristics*. We cannot respond to bureaucracy with bureaucracy, because second-tier bureaucracy legitimizes first-tier bureaucracy. Think of this curious recent invention of the press: the function of "decoder" journalists. Briefly, journalists might say stupid things, but super-journalists would be there to correct them. But who will be the decoders of the decoders? So it is only by returning to academic activity, here and now, and reflecting on its heuristics on a daily basis, that we will be able to attempt to resolve the crisis of academic integrity, and certainly not by piling institution upon institution in a grand and teetering structure. This is a collective task, which each of us must undertake, to appreciate what is valuable in our day-to-day practices, without seeking to hunt down turpitude at all costs, at the risk of giving it new grounds in which it can flourish.

References

Beaud, O., & Vatin, F. (2020). La profession universitaire. Une expression malheureuse, 'enseignant-chercheur', et un statut dégradé (I). *Commentaire, 172,* 859–870.

Beaud, O., & Vatin, F. (2021). La profession universitaire. Une expression malheureuse, 'enseignant-chercheur', et un statut dégradé (II). *Commentaire, 173,* 141–152.

Vatin, F. (2013). *Évaluer et valoriser. Une sociologie économique de la mesure.* Presses universitaires du Mirail.

Vatin, F. (2014). *Le travail. Activité productive et ordre social.* Presses universitaires de Paris Ouest.

Vatin, F. (2016). Une crise sans fin ? L'État, l'enseignement supérieur et les étudiants. *Le Débat, 192*(Nov.–Dec.), 154–172.

Vatin, F. (2020). Enseignant/chercheur : un métier ou deux ? *Revue française de pédagogie, 207,* 87–94.