

Chapter 14. “A laboratory curiosity”

“That is an impressive list”

To prevent the negative consequences of the investigation of *Nature*, J. Benveniste suggested to the administration of Inserm the immediate organization of a counter-inquiry:

“[...] Philippe Lazar, the Director General of INSERM, with whom I did not really get along, seems to want to let things rest and send back the evaluation of the research of the Unit 200 to the "legal" deadline of the four-year examination planned at the beginning of year 1989. But to counter effectively the devastating effects of the botched investigation published by *Nature*, it would have been necessary that another inquiry commission – a serious one – be immediately appointed. Its composition could have been established in dialogue between Inserm, my team and possibly other partners such as the CNRS [*National Center for Scientific Research*] and the Academy of Science. A rigorous protocol of check of my experiments and the observations performed in the foreign laboratories would have allowed rebalancing the situation. But, in total convergence with the mandarins of the French research whom I met a few weeks before at the Minister of Research, the administration of Inserm refuses the immediate creation of such a commission. *Business as usual*, as if the intrusion of a bunch of bounty hunters in a state laboratory was commonplace.”¹

In spring 1989, J. Benveniste knew that many of his colleagues sitting in Inserm commissions where the renewal of the units of the Institute were decided, waited to catch him out. Numerous members of the commissions of Inserm wished an exemplary penalty because they considered that the credibility of Inserm and French research was at stake. But both the commission and the administration of Inserm faced with a difficult problem. Indeed, how could J. Benveniste be punished when the production of his laboratory was unanimously acknowledged and while he wore the halo of the discoverer of the paf-acether?

Furthermore, after the turmoil of the summer 1988, the *Current Contents* of Philadelphia – an independent organism that broadcasts databases concerning scientific articles and performs bibliometric studies – wrote: “Professor

Benveniste has a substantial scientific reputation as judged by his publication and citation record”:

“A check of the *Science Citation Index* revealed that Benveniste has written dozens of papers, including at least 13 that are cited more than 100 times [...]. That is an impressive list. He has written the second most-cited paper ever published in *Comptes Rendus de l’Académie des Sciences*. And certainly a paper from the *Journal of Experimental Medicine* cited more than 640 times is an outstanding achievement.”²

These comments made the task of the “peers” of J. Benveniste particularly difficult for assessing his scientific activity. Their work would have been largely facilitated if J. Benveniste was an unknown researcher without a prestigious past, without a productive laboratory around him and without scientific and political networks. What arguments the Commission could put forward without giving the impression of an “official” scientific censorship. Only the ability and the skill of P. Lazar allowed protecting the institution and not making a martyr of science at the same time. But what scientific arguments were used for this purpose?

*The report of the Specialized commission*³

In April, 1989, a delegation of the commission of Inserm (*Specialized scientific commission n°2* or *CSS2*) attended a presentation in Clamart made by the scientists of the Unit. Then, the members of the commission visited the premises. A report was available on April 25th, 1989 in which one can read the following extract:

“Concerning the controversial domain of high dilutions, the CSS2 recommends:

- Stop as fast as possible (or get rid of it) the activity concerning the pharmacological effects at high dilutions. The reasons of this last recommendation are the following ones:

- 1) This activity represents, according to Mr Benveniste, only a quite small fraction of the total activity of the Unit and employs no researcher with permanent position;
- 2) At present, the team does not seem to be ready to use biological models other than basophil degranulation;
- 3) The possible biophysical interpretations of the experimental observations exceed at present the skills of the current team.

For all these reasons, it is obvious that this issue can only move forward at a snail’s pace. It seems thus urgent that the problem changes hands.

Chapter 14. "A laboratory curiosity"

- Any relation with the media concerning high dilutions must be immediately stopped. It is clear, in the opinion of Mr Benveniste himself, that the controversial facts cannot be considered as definitively established. The considerable importance given in media to these results, as well as the permanent polemic associated to the problem:

- 1) damages the necessary outside collaborations that the team needs to preserve in other scientific domains constituting the key point of its activity (paf-acether)
- 2) will probably damage recruitment of researchers for this team from public agencies and will make the search of an industrial employment for the young PhD students more difficult ⁴
- 3) probably damages the scientific reputation of the strong part of the team (paf-acéther)
- 4) damages the image of Inserm and more generally the image of the French scientific community."

The vote of the Commission n°2 on June 6th reflected well the perplexity of its members in front of the administrative management of the issue: for the theme of the paf-acéther, the votes were widely favorable (22 favorable votes and 1 reserved); for the theme of "high dilutions" the ratio was inverted (1 favorable, 3 reserved, 16 unfavorable and 3 abstentions). The global vote concerning the scientific activity of the unit led to 3 yes and 20 abstentions. And for the question of the renewal of the mandate of director, the rate of abstention was also massive: 1 yes, 6 no and 13 abstentions.

Bis repetita

In front of this division of the votes, a second visit of the unit was decided for June 27th. But this time, it was a delegation of the Scientific Council of Inserm, the supreme scientific authority of the Institute, which went to Clamart. It was unusual, but two foreign experts joined the commission. One of these experts was the American H. Metzger which was an eminent member of National Institute of Health. He was the same who evaluated the first manuscript for *Nature* on high dilutions. Once the article was published in June 1988, he wrote to *Nature* that it was shameful to publish such absurdities. The other expert was the English A.B. Kay of National Heart and Lung Institute of London.

The day before the coming of this commission, J. Benveniste sent a letter to P. Lazar to express his fears about "the purpose of this visit and its conditions of organization". Thus, he wrote about the foreign experts:

"We received no official notification of the name of the experts chosen by the Council. These names are circulating all over Paris

and came back to us by numerous sources, but we had no possibility of discussing this choice and, possibly, of suggesting other experts for a second opinion. One of them, Barry Kay, is not one of our friends for strictly scientific reasons. A reliable source, since he is the personal doctor of Queen Elisabeth (*sic*), recently told me that Kay was ideologically opposed against any research on high dilutions. The other one, Henry Metzger, scientist of much better quality, became famous for sending a letter to Nature [...] describing a single experiment, performed in a system totally different from our own, allowing him to categorically deny the existence of any effect at high dilutions. It is to say the impartiality of the chosen experts and the impartiality of the choice.”⁵

About the theme of high dilutions, he pursued:

“Mr Lhoste who manages the delegation of the Scientific Council pointed out to me that the research on high dilutions would not be examined during this visit. This seems very inconsistent to me. Indeed the C.S.S. N°2 "emitted a favorable vote on the activity concerning the paf-acether". The unfavorable vote is only for "the activity which concerns the pharmacological effects of high dilutions". A visit of the unit by the Scientific Council is necessary only if there is an unfavorable vote of the commission and it is exactly the part that was the object of an unfavorable vote which would not be examined.”

J. Benveniste then considered the possibility of giving up the study of high dilutions:

“I remind you my position which is the one of the laboratory council of U.200: if deemed necessary, I agree to give up, within the framework of my activity of research in Inserm, any research on high dilutions. Things being what they are, any decision which could appear as a penalty to a laboratory of Inserm which published 10 articles in the Journal of Immunology during the last four years would provoke an enormous national or international scandal in which nobody has interest, self-destructing in passing the system of evaluation of Inserm. I am counting on your wisdom, to avoid such turbulences in U.200 and in Inserm. However, the appointment of these "experts" and the fuzziness of tomorrow's visit worry me. Obviously, an operation gets ready, but which one?”

Chapter 14. "A laboratory curiosity"

The report of the Scientific council

Contrary to what J. Benveniste anticipated, the Scientific council was not as negative as he had imagined. In its conclusions, the Scientific council indicated that "the scientific activity of Unit 200 remained at a high level, in an original and important domain, which is inflammation. [...] We are certainly not in favor of depriving it from the means necessary to pursue most of the studies, even less to scatter it". Then, the main subject of the report was finally reached:

"Without entering the debate, the delegation wished to analyze with lucidity the place and the effects of the research on the theme of high dilutions led in the Unit, or in the immediate neighborhood, for several years. Let us remind first that they constitute only a small portion (5%?) of the global activity of the group. Well informed about various aspects, scientific or not, about this activity, the members of the delegation were unanimous to underline the disproportion of the facts with their interpretation and judgments expressed in all circles. As they were described, with the recent developments which were presented to us, the results of the team of J. Benveniste appear only as a curiosity of laboratory to which satisfactory explanations are not yet given and the impact of which will remain limited."

The reader probably noted the expression "curiosity of laboratory" that is surprising coming from scientists to describe the experiments on high dilutions. He also noted this strange conception of the scientific research: only facts for which one has an explanation have to be the object of research. The report continued:

"These observations contradict some of the best established laws of physical chemistry; thus they require an open mind and even temper. Every experimental researcher is familiar with unexpected, even unusual, observations and the signature of a creative and responsible scientist is to know how to distinguish the facts that are significant among those who finally appear trivial, what is sometimes difficult. The observation of this group, the sincerity of whom we will not question, can correspond to the one or the other one of these categories. One could hope, even demand, that this team will make an effort of analysis in order to conclude with certainty on the meaning and the importance of these observations. For several years, the team supports its conclusions on a type of cells and a test, which is disputed to say the least. Only the extension to other simpler cellular or biochemical

systems would allow generalizing these curious results before asserting that certain phenomena escaped 200 years of research in chemistry. The director of the laboratory did not answer all these objections in a satisfactory manner, so the relevance of the facts could not be considered as established [...].”

During its meeting from July 4th to 6th, 1989, the scientific council recommended to maintain the Unit 200, but to postpone the renewal of director's mandate of J. Benveniste “because the program and the scientific perspectives were insufficiently structured and because the answers to the serious scientific objections were insufficient.”

“The refusal of any ideological censorship as guarantee of any creativity”

In an open letter, which he gave directly to J. Benveniste after having met him, P. Lazar explained the reasons of his decision, first of all the preservation of the Inserm’s laboratory:

“The convergent opinions of the Specialized scientific commission and the Scientific council on the internationally recognized quality of most of the work made in your laboratory lead me quite naturally to this decision.”⁶

Then P. Lazar expressed his wish to maintain J. Benveniste at the head of the laboratory but “to postpone the official confirmation” of this new mandate for the following reasons:

“At first I think necessary to reaffirm clearly that, subject to the scientific quality of their works, the freedom of the researchers in the choice of their hypotheses and their working methods could be limited only by the rules of the common law and by the respect for the ethics and the moral code. Consequently, we must accept the possible consequences of this deliberate refusal of any ideological censorship, irreplaceable guarantee of any real creativity”.

A declaration of principles which one would like to see engraved above the front door of any laboratory! But – indeed this type of introduction is often followed by one “but” – this freedom has for consequence, P. Lazar continued, the exercise of the responsibility of a laboratory director who, because of the public character of the research institute, “also commits the scientific community to whom he belongs”. Besides, the Director of Inserm added on, it was necessary “to consider with the biggest attention the criticisms and the convergent recommendations of both authorities of evaluation of Inserm during the *a posteriori* four-year examination of their activity [...] which

guarantees the good employment of the means granted by the nation to his researchers". He then added:

"It is clear that the two scientific authorities that successively examined the work of the Unit 200 with – for the second one – the help of foreign experts, have expressed strong reservations about your works concerning "high dilutions".

These reservations are related to the content of these works, an analysis of the results which was insufficiently critic, their adventurous interpretation, the conditions of their public expression and the worrisome consequences of the publicity which was then given to them regarding the unfounded strengthening of the credibility of some forms of therapeutic practices".

Then P. Lazar came to the role of sound box played there by *Nature* – which was not directly named – in the affair:

"The conditional publishing, by a high-level international journal, of an insufficiently supported article and the surprising behavior of this journal, to say the least, after this publication – the unprecedented decision to organize a visit of the laboratory by representatives of the journal, the strangeness of the composition of the committee of visitors, the unfriendly contents of the report published as a result of this visit, the doubtful justifications of the journal on its real motivations – constitute extenuating circumstances towards the team of Unit 200. They do not however absolve their own responsibility."

This quite clear criticism of the attitude of *Nature*, presented as an "attenuating circumstance", is the first public position of Inserm on the "affair in the affair"⁷. That was in stark contrast with the press releases of Inserm which seemed to contemplate at a distance the "scientific debate" in spite of the involvement of some of its members.

Having expressed the necessary "duty of confidentiality" of the researchers towards the population and the "rational doubt" and the critical mind which the latter should permanently exercise, P. Lazar proposed to J. Benveniste a kind of road map for the next six months at the end of which the renewal of the mandate of the director of the Unit 200 would be decided or not. First of all, about his scientific activity towards the high dilutions:

"On the basis of the scientific authority which is recognized to you, I ask you to work, during the period which opens, to completely resume your role in a scientific community which does

not, in principle, try to reject you – as shown by the views emitted by our authorities of evaluation – but which rightfully expects from you the proofs of your desire not to deliberately marginalize from it. I do not ask you to give up your ideas and the studies which result from it. Such an act of authority would seriously violate the principle of freedom which I expressed above. I perfectly understand moreover that a researcher who thinks he has highlighted a new phenomenon cannot agree to classify the file without clearing up the reasons of these observations. But if you really want to achieve this clarification, please agree to dedicate as a priority your reflection to systematically look for the experimental biases which might have escaped until now and which can, in all likelihood, explain your unusual observations: you will thus find again a behavior which could not be criticized by your peers because in compliance with the true essence of the scientific thought. It is not excluded in fact that the highlighting of such a bias could present in itself a scientific interest.”

Finally, concerning the attitude of J. Benveniste towards the media, P. Lazar added:

“The code of good practice which I recommend to you presumes in particular that you give up, for a while, expressing yourselves on this subject except in high-level scientific journals – the necessary time for reconstituting the capital of confidence which you have today, you may or may not admit it, largely dissipated in the eyes of your colleagues.

I hope very sincerely to be understood by you, to observe the next signs of a significant change of attitude, in order to follow up the intention which I announced you at the beginning of this letter as regards the renewal of your mandate before December 31st, and thereby to assure the viability of your laboratory. I will be very sorry to give this up.”

It was not thus strictly speaking about a penalty but of a kind of testing that allowed the administration of Inserm to let time take its course and to calm things down by maintaining the media at a distance.

“Certainly, I was dreaming sometimes”

In an “opinion” in the journal *Le Monde* entitled “The forbidden dream”, J. Benveniste expressed his satisfaction after this decision and commented on the conditions which were imposed on him:

"As one could foresee, the wisdom and the courage have prevailed. The final decision maintains the U200 in its integrity. It leaves to the researchers their most fundamental right, the freedom to search, without which discovery is not possible. I did not doubt Philippe Lazar's attitude on this point, even if it led him to implicitly deny the conclusions, admittedly arbitrary, of the CSS2 (and, it seems, of the Scientific council) forbidding without reason a research subject.

This was merely very normal. However, there are two conditions. Firstly, I must dedicate as my priority a reflection to look for experimental biases... What else did I do the previous years by informing the Scientific council and the administration of Inserm about these strange results? Did I do anything else when I asked the most famous French scientists for help and when I submitted, after seven working years and after check by five laboratories around the world, these results to the journal *Nature*? Did I do anything when I accepted the only French scientist who presented himself, showing him my books, making with him the necessary checks with, naturally, the same positive results? ⁸

The public opinion must know that, among the numerous scientists who shout that French research was dishonored, no one came in the laboratory to comment scientifically on these scientific results. It indicates that the debate is not, has never been, scientific; it is partisan, personal, maybe economic and especially theological." [...] ⁹

Then he addressed the recommendation not to express himself in the media any more:

"The second condition which is imposed upon me is the absence of communication with the media. I point out that, from 1985 (first disclosure in the press) to 1988, I remained silent under, sometimes, a torrent of insults. I published first before speaking, and it is *Nature* that, instead of doing its work of editor, gave a considerable impact inside and outside the journal and continued after this publicity. I followed up. What would have been said if I had refused to explain? I always made it, I believe, in the dignity and by specifying every time: "if it is true".

Certainly, I sometimes dreamed: the key in the Seine, the electromagnetic fish. I did not know at that time that the physicists who touch the infinity have the right to dream and not those soft scientists who are the biologists! Now I know it."

“A little more humor would be needed in this story”

Even if P. Lazar was not convinced of the interest of high dilutions – it is an euphemism – nevertheless the delicate management of the affair probably fed his reflection on the role and the functioning of the administration of research as well as the role of scientific journals. Thus, in a book published in September 1989, he wrote that “when one gets off the beaten track, one takes real risks, starting with the risk of not being able to quickly publish results”. In order to avoid innovative ideas slip away, he proposed “to try to set up a procedure which would, experimentally, allow financing every year a small number of projects recognized as at "high risk", out of quota.” With a scoffing attitude, one could interpret this proposition as the implicit recognition that the projects that are usually financed by Inserm are neither risked nor very innovative...

In the same book, he confirmed his reproaches concerning the behavior of *Nature* by naming the journal directly this time:

“The journal *Nature* should never have agreed to publish an article by having the intimate conviction, as its director will explain later, that it deliberately released an example of “second-category research”! In reality, there is every reason to think that it imagined having to deal, in one way or another, with a deliberate deceit: otherwise how could one explain that the journal took the deliberate risk to be deeply criticized for this choice, obviously inconvenient if the existence of trickery could not be demonstrated? It had to be really sure of itself and of the trick it played, in passing, to the French science.”¹⁰

In a less formal way, at the same time, P. Lazar shared his thoughts with the journalist M. de Pracontal about “the affair”:

“[...] Philippe Lazar points out that there are not two Benveniste, "One Doctor Jeckyll who would do wonderful works on paf during the day and Mister Hyde who would devote to obscure research on the memory of water during the night". Lazar does not personally believe to the memory of water: "I think that there is an artifact, something like a "magic trick". Nevertheless, I think that Benveniste is in a situation which deserves respect: he thinks he has pinpointed out something. My message is not easy to pass through. In this affair, my wish is that Benveniste does not drown himself in his own bowl of water. I cannot change his character nor his way of overestimating his own work and depreciating those of others. I cannot blunt Jacques Benveniste. There are no solutions to all the problems on Earth when we refuse censorship.

Chapter 14. "A laboratory curiosity"

The ideal would be that somebody explains what is happening in these experiments and discovers the experimental bias. But a little more humor would be needed in this story. A number of my colleagues have no sense of humour."¹¹

In 2004, p. Lazar reaffirmed his opinion on the work of J. Benveniste:

"Philippe Lazar [...] sees, above all, in Jacques Benveniste a first-rank scientist who remained honest but who was a victim of a murky affair. He also considers that the man "was not critical enough in the interpretation of his results." "The phenomenon which he noticed, he judges, could result from another cause than from a dilution of the studied substances, for example a repetitive contamination from tube to tube." "¹²

It is important to note that these comments dated October 2004 did not take into account the later developments when contamination was no longer an issue (see second part). If there was an artifact, its highlighting would have to be much more subtle and more original than a simple "repetitive contamination from tube to tube". We will nevertheless examine in the next chapter the arguments in favor of possible artifacts.

Notes of end of chapter

¹ J. Benveniste. Ma vérité sur la mémoire de l'eau, p. 86.

² E. Garfield. Citation Perspective on Jacques Benveniste - Dew Process at Last? *Current Contents*, March 27th, 1989, p. 3–7.

³ Members of the Specialized Scientific Commission n°2 : M. Richard Rips (President), M. Jean-Paul Tillement (Vice-President), M. Raymond Bazin, M. Pierre Bechtel, M. Emile Bisagni, M. Denis Blache, Mme Francine Bourgeois, Mme Marie-Françoise Cachera, M. Etienne Delain, M. René Devilliers, M. Guy Dirheimer, Mme Evelyne Eschwege, M. Jean-Charles Fruchart, M. Yves Guidicelli, M. Jean-Pierre Henichart, M. Jean-Pierre Kantelip, M. Pierre Laduron, M. Michel Lagarde, M. Michel Ladzunski, M. Gérard Leclerc, M. Jacques Robert, M. Jean-Michel Schermann, M. Gérard Siest, M. Camille-Georges Wermuth.

⁴ About this issue, J. Benveniste pointed out: "Other inconvenience that the committee would dread was the impact of the media debate about the memory of water "will probably damage recruitment of researchers for this team from public agencies and will make more difficult the search of an industrial employment for the young PhD students." On the question of the recruitment of PhD students coming from my laboratory, the future, as we will see, will show that the commission was right. Retrospectively, such prescience seems to me admirable". (J. Benveniste. Ma vérité sur la mémoire de l'eau, p. 90).

⁵ Letter of J. Benveniste to P. Lazar of June 26th, 1989.

⁶ Letter of P. Lazar to J. Benveniste of July 11th, 1989.

⁷ J. Maddox answered the critics of P. Lazar about the attitude of *Nature* in *Le Monde* of July 26th, 1989 (J. Maddox. Plus vrai que « Nature »). He pointed out, about the works of J. Benveniste, that in spite of the criticisms of P. Lazar on the attitude of *Nature*: "both committees of Inserm criticized his works on high dilutions with more or less the same arguments as ours".

⁸ This scientist was Alfred Spira (see Chapter 16).

⁹ J. Benveniste. Le rêve interdit. *Le Monde*, July 12th, 1989.

¹⁰ P. Lazar. Les explorateurs de la santé. *Odile Jacob* (1989), p. 160.

¹¹ M. de Pracontal. Les mystères de la mémoire de l'eau, p. 140.

¹² M. Albertganti and J.Y. Nau. Jacques Benveniste. Un biologiste hors norme. *Le Monde*, October 6th, 2004.