

Chapter 1. "It is scandalous, you stifle the discovery of the century"

June 29th, 1988

On that day, the readers of the French daily newspaper *Le Monde* discovered an intriguing title on the front page of their favorite journal: "The memory of matter."¹ A promising comment accompanied it: "A French discovery could upset the foundations of physics." Nothing but the best.

The reading of the article could only shock every person having a minimum of scientific knowledge. Indeed, "the question is no more and no less of discovering if some of the current foundations of physics, chemistry and biology must be questioned." The bar was directly set very high!

In inner pages – an entire page was dedicated to the subject – the authors of the article Jean-Yves Nau and Franck Nouchi described the stages which led to this publication. The article of both journalists was enlightened by three texts: a text of J. Benveniste himself, an interview of Jean-Marie Lehn, French Nobel prize laureate in Chemistry, as well as a portrait of J. Benveniste qualified as "*enfant terrible* of the research world."

For the reader in a hurry, the words of J.M. Lehn – as reported by *Le Monde* – could nearly be considered as an approval. The Nobel prize laureate in Chemistry conjugated the verb to disturb in all tenses: "Disturbed, that's the least you can say. These results are disturbing, very, very disturbing." An attentive reading showed, however, that J.M. Lehn had rather mixed feelings. Indeed, even if there was a publication in *Nature* and even if "five laboratories joined to sign such a work", he cautiously added: "In the current knowledge [...] I do not see how in biology, in the absence of molecules, one can transmit information."

The text of J. Benveniste entitled "Another conceptual world" did not bother about language precautions. "As usual", those who knew him would have been tempted to say. For this latter "The change of the way of thinking is not less big when one gave up the flatness of the earth for the roundness." And pushing the metaphor to its paroxysm, he did not hesitate to state that "the used procedure is similar to shaking the key of a car in the Seine at the *Pont-Neuf* [a bridge in Paris] and then collect at Le Havre some drops of water to get the same car started and not another one". Having expressed the doubt that suits to every good scientist – "Ourselves, from the observation of the first results, throughout this research [...] felt and will feel an anxiety, a tiny quantity of

doubt present somewhere” – he described the exceptional precautions that were taken so that this doubt would be the smallest (replication of the experiments by other laboratories, blind procedures). He could thus give free rein to his innate taste of the metaphor in a kind of final bouquet. He began then to dream and wondered if one day one could not, for example “transport our electromagnetic copy at the other end of the world or in another planet? [...] with the information passing under the *Pont-Neuf*, reconstitute a diplodocus or more simply catch here an electromagnetic fish without bones?”

Without showing the slightest scoffing attitude, we cannot refrain from reminding the statement of the French humorist Pierre Dac: “When bounds are crossed, there are no limits anymore”...

On the same day, Inserm – the public institute of research on which depended the laboratory of J. Benveniste – published a rather unusual press release in which, after having briefly described the content of the publication, it reminded:

“Any real discovery inevitably arouses the temporary incredulity of the scientific community. It is this community that must select, by its usual methods of evaluation where the scientific controversy has its place, between what will be finally considered as only illusion and what will constitute a real advance of knowledge. It is clear in such a situation that the Administration of Inserm must trust in the judgment of this community. It considers that the publication, by a journal as prestigious as *Nature*, of the mentioned results constitutes an important first stage in the process of evaluation.”²

As in the very old times of the chivalry, Inserm thus invited the researchers to compete together in a tournament where God would know his own. For *Le Monde*, this text evidenced “the embarrassment of the Institute towards this publication.”

However, some details suggested to the attentive reader of *Le Monde* – regardless of any scientific concern – that there was something unusual concerning these results when he learned that “*Nature* decided not only to accompany the publication of the article by an “editorial reserve” but also to appoint a commission of inquiry which would take place early July in the laboratory of J. Benveniste.” Why to commission investigators after the publication probably wondered some readers? Wouldn’t the opposite have been more logical? But according to *Le Monde*, this sudden haste of the journal suggested that it was “worried, doubtless, not to miss a “historical” publication”.

The publication was indeed historical. However, not exactly in the meaning seemingly understood by the journalists of *Le Monde*.

A month before

In fact the information published by *Le Monde* on that day was not really new. The sensational peculiarity was the publication of the results of J. Benveniste in *Nature*, one of the most prestigious – if not the most prestigious – scientific journals. Contrary to numerous journals that publish results in a specialized area, *Nature* is one of the rare high-level journals to be multidisciplinary. In addition to the scientific studies which are reported, the journal also contains numerous sections – pleasant and easy to read – on scientific news, comments on the articles of the week, policy of research, life of laboratories as well as job offers. It is consequently much read and is present in all laboratories and university libraries. Even if they do not sometimes hesitate to criticize the journal for its bias and its taste of the scoop, most of scientists and thesis students would give much for having an article in *Nature*. It is – besides a respected trophy in a list of publications – the guarantee of a certain visibility.

A month before, the journal *Le Monde* addressed the issue of the research of J. Benveniste on high dilutions. Indeed, on May 27th, the latter presented the results of his laboratory to the National congress of homeopathy in Strasbourg. A journalist of *Le Monde* was there.

The results of J. Benveniste were then reported by the daily paper in a way of rehearsal before the edition of June 30th.³ The catch phrase of front page efficiently attracted the eye by evoking “The “ghost molecules” of homeopathy” and by being preceded by the title: “a scientific basis for a controversial discipline?” The comments of J. Benveniste already reflected the importance of the event: “either we regularly made a mistake for three years [...], or we are in front of a completely extraordinary discovery, the consequences and the upheavals of which we cannot yet measure”. In inner pages, another subtitle was written in the same vein (“a mysterious phenomenon”) and the words of J. Benveniste pronounced at this conference reinforced the rather esoteric general tone:

“We are thus led to speak of “ghost molecules”, of “molecular imprints” in water having kept the “memory” of substances with which it was in contact.”

As we see the “memory of water” was not very far. Moreover, it was on this occasion that the daily paper *Liberation* titled a short article: “Homeopathy: Pr J. Benveniste verifies the memory of water”.⁴ It was apparently the first use of this expression which became famous.

Did these articles in the press – in *Le Monde* particularly with its audience and its reputation of “leading newspaper” – played a role in the decision of John Maddox, director of *Nature*, to publish the article, which was for two years in discussion between the editorial team of the journal and the laboratory of Clamart managed by J. Benveniste? It was according to Jean-Yves Nau “too great an honor for *Le Monde*”.⁵ But Bernard Poitevin – who, as we will see, was at the origin of the introduction of this research on homeopathy in the laboratory of J. Benveniste – did not hold this view:

“It is clear that the commission of inquiry should have taken place before the publication of the article, to avoid what, one way or another, will have an air of scandal. One does not understand why the Director of *Nature* proceeded in this way. The hypothesis which I personally retain as being the most likely is that he was irritated by the publication of the information in the newspaper *Le Monde* after the congress of Strasbourg. I was personally a bit shocked by this premature "publication" of the information by journalists to whom silence had been required. But it does not reduce the responsibility of *Nature* that should have decided to refuse the publication of the article and not to set this trap to the Unit 200 and to its director.”⁶

After this article in *Le Monde* by the end of May, J. Benveniste wrote to both journalists Franck Nouchi and Jean-Yves Nau:

“I was reluctant, as you know, to make public my scientific communication, dedicated to a congress of specialists. Only the publication in an undisputed international journal will not only allow moving forward, but describing all results in detail. Having said that, the article reflects rather exactly the issues raised by these experimental facts. I thank you particularly to have reported our doubts and fears in front of phenomena so disturbing, especially as the French official research leaves us in an absolute material and mental solitude.”⁷

As we see, the reluctance was rather moderate. The friendly tone of the article seems rather to suit to J. Benveniste. Indeed, he never hated reading favorable comments on his work. Very fussy, he did not hesitate however to write to journalists if he considered that they related facts concerning him with vagueness or inaccuracy. Nevertheless, the interesting point here is the clear expression of the only purpose of J. Benveniste, namely: to publish these results in a high-level scientific journal, the only solution according to him that would allow opening a new field of research intended to study these “disturbing

phenomena”. It is this line of conduct, which he never broke, that could explain the sequence of events and could constitute a second explanation for the decision of *Nature* to let publish the “scandalous” manuscript. J. Maddox indeed declared that when he wrote to J. Benveniste that he would not publish the article in spite of the checks made by other laboratories, the French scientist called him:

“It is scandalous, you stifle the discovery of the century. You make as the Church with Galilee”. He accused me of being against the truth. I answered "why you do not propose an explanation for these results that are contrary to normal science (*sic*)?" And two or three days later, he sent me the theory of the memory of water.⁸ I was surprised that a director of a unit of Inserm could build such a theory so fast! And I was irritated. I decided to publish the paper.”^{9,10}

“The fight of his life against false science”?

According to this version of the facts, the harassment of J. Benveniste in a kind of “epistemological blackmail” would have overcome the resistance of J. Maddox who, disheartened, would have decided to publish the work. One can question such an explanation. One does not become the director of a publication such as *Nature* accidentally and one badly imagines somebody at this level of responsibility taking an important decision simply because he is “irritated”. J. Benveniste wondered about another possible interpretation¹¹:

“[...] maybe the aim of John Maddox was to let take off what he considered as a pseudo-scientific theory supporting the heretic homeopathy in order to blow up it in flight. I always wondered if Maddox did not wish to do the fight of his life, supported by the scientific establishment, against "false science." ”¹²

And in another circumstance :

“[They] had decided that it should not work. Maddox said himself in front of the cameras of the British TV: ¹³ homeopathy is dangerous and the fact that physicians are frequently encouraged to prescribe homeopathy is a very serious situation. And he adds literally: "I had examples in my own family". I really have the feeling that he identified our research with stakes related to homeopathy and that he came in our laboratory as a crusader, to extirpate this pseudo-science.”¹⁴

This third explanation – a “crusade against false sciences” – has perhaps a slight flavour of paranoia, but today it appears that this explanation is the closest

to the reality. Indeed, twelve years after the facts, in an interview about the release of his book “What remains to be discovered ¹⁵”, J. Maddox gave this explanation:

“We published the work of J. Benveniste for several reasons. At first, *we were sure that he was wrong*, but it is also an interesting example of the way the researchers could make a mistake. The inquiries which we performed in his laboratory showed how an honest scientist could persuade himself that he had made an overwhelming discovery.” ¹⁶

Hence, it was a kind of a punishment as an example administered by the director of *Nature* and in any case an effective warning for other “honest scientists”. Already in September, 1988 – just after the tumult of the summer – a journalist of the *Journal International de Médecine* questioned J. Maddox on the reason for having published before investigating:

“We did not wait until the conclusions of our investigation to publish the article of J. Benveniste, because I think that this one would not have admitted that his article would be published at the same time as a report criticizing it.” ¹⁷

J. Maddox here mistreated the logic. If the survey was unfavorable, one does not understand for which reasons the article must be published! In fact, as soon as June 30th, i.e. just at the time of the publication of the article, but before the coming of the investigators to Clamart, P. Newmark, Deputy Editor of *Nature*, explicitly admitted that – with full knowledge of the facts – his journal had published results that the editorial team considered to be forgery:

«The head researchers in this work are reputable scientists and their results have been independently confirmed by several laboratories. *We are certain that the results must be wrong*, but we have been unable to disprove them. We are sending a team of experts to Paris to observe the research at first hand, but meanwhile, because for the publicity this work has already had in France, we feel it is appropriate to publish this paper.» ¹⁸

Nature moves its pieces

What were then the reasons explaining the attitude of *Nature*? Irritation to see the results released in the press? Exasperation due to the permanent pressure of J. Benveniste? Personal fight against homoeopathy and “false sciences”? What is certain is that J. Maddox took the offensive at early June 1988. On June 3rd, a rather obscure fax from P. Newmark put a first milestone with caution,

approaching on tiptoe the question of a possible check on the site of Clamart where worked the team of J. Benveniste:

“[...] John Maddox has requested to me to contact you to ask whether it would be acceptable for Walter Stewart to spend a day or so in your laboratory observing the experimental procedure by which your data are obtained. It is Walter Stewart who is from NIH, that we had asked to write a comment on your paper should we publish it. He would probably be accompanied by James Rondi (*sic*) who has some expertise in examining extraordinary phenomena.”¹⁹

It is important to note that the issue was an inquiry *before* a possible publication. The presence of J. Randi was notified in the conditional tense with a typo. There was no additional detail on the competences of the future investigators. Internet did not still exist in the state where we know it today and J. Benveniste could only speculate. Expert in extraordinary phenomena? Not easy to find such a skill in directories of scientists or among members of scientific associations! Who were then these “Rondi” and “Stewart”? Stewart is a very common name. It was probably the same W. Stewart who previously reviewed the manuscript, but we had no additional information about him. As for “Rondi”, this name vaguely echoed something to me.

I finally found a reference about a man named Randi and not “Rondi” in the book of W. Broad and N. Wade, “Betrayers of Truth”²⁰ which I had read some time ago. Randi was a “magician”. He was a professional “skeptic”. He boasted of having unmasked Uri Geller which – according to the time-honored expression – “twisted teaspoons”.

J. Benveniste then questioned J. Maddox about the reasons of this rather disturbing and unexpected presence, possibly related to hidden intentions of the survey commission:

“Maddox answers me that our experiments require numerous manipulations, therefore a conjurer would be able to detect a possible error during the manipulations. At no time, I insist, he suggests, as he will do later, the possibility of a cheating – because in this case, I would obviously have got angry.”²¹

It was – we must admit – a brilliant exercise in the art of casuistry from J. Maddox. It was especially a white lie. Indeed, according to M. de Pracontal who questioned J. Maddox after the investigation about the incongruous presence of the magician in the laboratory of Clamart ²²:

“Maddox frankly declared to me that because he suspected a fraud he made this unusual choice: "I thought sincerely that somebody played a trick on J. Benveniste. That's why I asked Randi to come".”²³

It was only on June 30th – the day of the publication of the article on high dilutions – that J. Benveniste understood who Stewart was. As an ironic coincidence, it was indeed in an article of the same issue of *Nature* that he learnt that W. Stewart was an “investigator” in the “Baltimore case”, from the name of an American Nobel prize laureate accused of fraud (and exonerated afterward). Strangely, J. Maddox introduced then W. Stewart (as well as Feder, the colleague and boss of the latter) in rather depreciating terms:

“Feder and Stewart’s activities have been much resented on several grounds, partly because they have no substantial scientific published records, partly because they are self-appointed keepers of the scientific conscience and partly because of what often seems their nitpicking persistence.”²⁴

It was only at this moment – when the article was published after a battle of two years – that J. Benveniste began to understand that he had fallen in a case of scientific misconduct. This suspicion on the intention of the investigators – namely, to investigate on a presumed misconduct – took shape when the qualifications of the investigators were known. Indeed, the experts were not professional biologists, but one of them was a self-proclaimed investigator in scientific frauds and the other one a conjurer specialized in the denunciation of “false sciences”. This escapade was managed by J. Maddox undoubtedly Director of *Nature* but whose speciality was formerly physics. It was however too late to move back.

But let us return at early June. On June 13th, J. Maddox unexpectedly announced to J. Benveniste that he agreed to publish the results. Highly ironically, it was now J. Maddox who put pressure on J. Benveniste! The latter indeed would have preferred the article be published a bit later. He told:

“Mid-June 1988, John Maddox, most probably titillated by press articles after my conference at the congress of homeopathy of Strasbourg, contacts me urgently while I am traveling in the United States. He suggests publishing the article at the end of the month, but imposes an additional condition: I have to accept the principle of a mission of expertise in order to verify the quality of the experiments. A delegation would be at work as early as July in Clamart. Once again I am surprised by this incredible requirement, but, caught by surprise, and not wanting to give up while I am

reaching the target, I accept. Given the urgency, it is by fax that I send the answers to the ultimate objections of the referees of the referees of *Nature* after having drafted them in the plane which takes me in Canada.”²⁵

And somewhere else:

“During June 1988, I call Maddox several times. On June 13th, he tells me that he is ready to publish the article. I remember a rather lively exchange on the date of publication. He proposed June 30th, but at this date it was impossible to organize the broadcasting of information, so that the press does not tell anything. I preferred September but Maddox refused.”²⁶

The possibility to see very soon the work published in black and white in *Nature* seems to have blunted the suspicion of J. Benveniste and of his team. Moreover, at this stage, it seemed difficult to be opposed to this investigation. To refuse would mean that there was something to hide. The scientific authority of *Nature*, which aims to be at the forefront of scientific excellence was very high and appeared as a sufficient guarantee. Furthermore, why to worry? The biological system correctly worked in the laboratory of Clamart. There were also these impressive blind experiments made under bailiff's control and described in the article of *Nature*. Two other articles on high dilutions had been accepted, of course in less prestigious scientific journals, but scientists do not spend their time to try to publish their results only in *Nature*. The procedure of checking that had been proposed appeared at this moment rather as a formality. Maybe the investigators simply wanted to verify that the laboratory notebooks were in accordance with the data reported in the article. At that time it seemed difficult to put in balance in one hand the work of several years and on the other hand an expertise of no more than a few days. If this expertise was the price to pay to get an article in *Nature*, why not. It would be a last effort before summer holidays. The fact that the checking procedure, which was previously scheduled before the publication was now planned after it did not seem to disturb many people. This was the state of mind which prevailed in the laboratory of Clamart at this time. Naivety? Certainly.

But before beginning the narrative of the legendary and disputed investigation of *Nature*, how did we arrive at this situation?

Notes of end of chapter

¹ *Le Monde*, June 30th, 1988.

² Press release of Inserm on June 29th, 1988.

³ *Le Monde*, May 29-30, 1988.

⁴ *Libération*, May 30th, 1988.

⁵ Michel de Pracontal. *Les mystères de la mémoire de l'eau*, p. 121.

⁶ *Le Médecin Homéopathe* 1988, n°3, p. 40.

⁷ Letter of J. Benveniste to J.Y. Nau and F. Nouchi on May 30th, 1988.

⁸ J Maddox takes some shortcuts with the facts. The expression “memory of water” was of course never used in the article; the term was coined later. Furthermore, J. Maddox seems to suggest that J. Benveniste elaborated a complex theory such as the theory of relativity. In fact, there was never any theory at all. We will see in a next chapter that some sentences were simply added at the end of the article to propose possible directions for future studies.

⁹ J Maddox gave reasons of his decision a number of times. In 1997, he explained it in these terms to a journalist of *La Recherche*: “Here is how things took place. I took home the complete file during a weekend, and I wrote what seemed to me a courteous letter of refusal to Benveniste, and I sent it. Some days later, Benveniste called me and asked me if I realized that I was stifling the biggest discovery of this century. He was furious. I answered him that his article did not consider how the data could find an explanation within the framework of classical physics and chemistry [...]. He answered me: “No problem”, and he sent me a fax on the “memory of water” as early as the next day. Then he again phoned me to ask if I was going to publish him this time. I answered him that it was ridiculous, that his new explanation was even more fanciful than the article itself. After that, I lost patience, and he then began comparing himself with Galilee. Then, I said to him: “OK, we will publish the article, but with a warning and if you let us visit your laboratory” ”. When the journalist of *La Recherche* asked him: “But once again, why to have published the article?”, J. Maddox answered: “I said it to you, the extravagant claims made me lose my cool” (*in* J. Maurice. *L'hebdomadaire « Nature »*. Un sanctuaire de la science en marche. *La Recherche*, July-August, 1997).

¹⁰ M. Pracontal. *Les mystères de la mémoire de l'eau*, p.13.

¹¹ It is also possible that J. Maddox had in mind the initiative of Robert W. Wood, an English physicist who reported in *Nature* at the beginning of the twentieth century his visit in France in the laboratory of R. Blondlot who claimed having discovered a new type of rays. The experiments to show the hypothetical N-rays were performed in the darkness. Indeed, the method used by R. Blondlot was based on the variations of the brightness of an electric spark, which was a very subjective method. Having put away a prism that played an important role in an experiment, the same positive results continued nevertheless to be imperturbably announced by R. Blondlot and his assistant. The report made by R.W. Wood in *Nature* marked the end of the N-rays. This episode of the history of science is now an archetype, which allows illustrating the bias that the

subjectivity of the experimenter can introduce into experimental results. Blind experiments eliminate this bias. The story of N-rays was told and analyzed by P. Thuillier (*La triste histoire des rayons N in Le petit savant illustré. Seuil. 1980*).

J Maddox alluded to this episode one year after the visit to the laboratory of Clamart: “In September 1904, we (*sic*) asked the distinguished specialist in physics R.W. Wood to visit one of the laboratories that claimed having detected N-rays, a more powerful type of X-rays. The latter wrote: “I went there not without skepticism, but with the hope that I could be convinced of the phenomenon.” He was not convinced. Pure coincidence, the laboratory was also in Paris” (*in* J. Maddox. Plus vrai que « Nature ». *Le Monde*, July 26th, 1989). Swept along by its own momentum to draw a parallel between both cases, J. Maddox made an error of geography. The laboratory of R. Blondlot was situated not in Paris but in Nancy. It was precisely a tribute to his city that Blondlot called N-rays this “new” radiation.

¹² J. Benveniste. *Ma vérité sur la mémoire de l'eau*, p. 63.

¹³ J Maddox had then declared: “I thought that it was important, in the current context, that there was no delay for publication. Among other considerations, the more the comment on the report of Benveniste would be delayed, the more we would be in danger – because it is, in my opinion, a danger – to see the partisans of the homeopathic medicine spreading in declarations, asserting that their curious way to cure was legitimized.” (P. Alfonsi. *Au nom de la Science*, p. 72).

¹⁴ Philippe Alfonsi. *Au nom de la science*, p. 34.

¹⁵ The title of the book of J. Maddox “What it remains to be discovered” is curious. A real discovery is precisely not scheduled.

¹⁶ Cyrille Vanlerberghé. *Qui sera le prochain Einstein ? Le Figaro*, May 2nd, 2000 (emphasized by me).

¹⁷ Joël Le Moigne. Interview of John Maddox. *Le Journal International de Médecine*, 1988, September 15–30, n°117, p. 15.

¹⁸ M.W. Browne. Journal publishes theory in disbelief. *New York Times*, 30 juin 1988 (emphasized by me).

¹⁹ Fax of Peter Newmark to J. Benveniste on June 3rd, 1988.

²⁰ William Broad and Nicolas Wade. *La souris truquée. Seuil (1987)* [Translation of “Betrayers of Truth”. Simon & Schuster: New York. 1982].

²¹ P. Alfonsi. *Au nom de la science*, p. 27.

²² It seems that the initial proposal to integrate J. Randi to the group of investigators came from W. Stewart.

²³ Michel de Pracontal. *L’imposture scientifique en 10 leçons. La Découverte (2001)*, p. 91.

²⁴ J. Maddox. Can a greek tragedy be avoided? *Nature* 1988 ; 333 : 795.

²⁵ J. Benveniste. *Ma vérité sur la mémoire de l'eau*, p. 57.

²⁶ P. Alfonsi. *Au nom de la science*, p. 24.

Crossed portrait #1

by Franck Nouchi

“The *enfant terrible* of the French medical and scientific community”

“At fifty three years old, Doctor Jacques Benveniste is still, under teenager's look, the *enfant terrible* of the French medical and scientific community. Poorly known by the general public, he cultivates not without elegance or naivety an outstanding character, halfway between a member of the generation of May 68 and a member of the Establishment that he hopes never to become.

"Immigrated of the first generation" – his father, native of Salonika, arrived to Paris in 1925 – this Parisian, son of local doctor is, in his young age, seduced by racing cars and wishes only one thing: becoming an automobile engineer. Having passed his "baccalaureate" at fifteen years, "too bad in mathematics", he takes refuge within medicine.

It was what was called the “royal road”. Medicine student, resident of the hospitals of Paris and staff physician, the future mandarin put an end to this academic career to enter the world of the research. After the thunderstorm of 68, he leaves France for California. At La Jolla he discovers the PAF [...] Then he decides to return in France, in 1973, in Professor Jean Hamburger's team and, finally, he obtains autonomy in Clamart, with the creation of the Unit 200 of Inserm that he manages since 1980 and where fifty people work on the fundamental mechanisms of allergy and inflammation today.”

(*Le Monde*, June 29th, 1988)