

Chapter 13. "If one wanted, one could look at fingerprints"

The sprinkler sprinkled

Because of the authority of *Nature*, the report that was published after the investigation struck a harmful blow to the credibility of the results reported by J. Benveniste. However, the journal of London faced numerous criticisms. Indeed, besides the scientific aspect of the affair, the attitude of *Nature* for its management was not comprehensible for numerous observers.

After the investigation, *Le Monde* published an article where J. Benveniste was backed quite evidently. The commitment of the journal appeared through titles and paragraph headings: "a strange anti-fraud squad", "sleight of hand in the laboratory", "a group strangely constituted"¹. F. Nouchi, the *Le Monde's* journalist, moved in the laboratory on Wednesday during the famous week and, with the agreement of J. Benveniste, he dressed in a lab coat to go unnoticed in the laboratory. He reported in an article the atmosphere which prevailed there, then adding: "we wondered how a researcher with the temperament of Benveniste could accept such machinations."²

At the time of the publication of the investigation report in *Nature*, the direction of Inserm issued a press release again:

"The additional publication appearing in the issue of "Nature" dated July 28th, 1988 and the diverse comments which accompany it confirm the Institute in its principle of reserve, inspired by the respect for the freedom of research. In particular, the Inserm Administration judges that its role is not to take part in the debate that today brings into conflict Dr Benveniste and the editors of the journal concerning the processes used by "Nature". This debate enters, obviously, the field of the controversies announced by the previous communiqué of the Institute."³

In this press release, the direction of Inserm thus confirmed its desire not to interfere in what it considered as the normal process of the research, even if we guess a light criticism towards the "processes" of *Nature*". Then, in the same text, the Institute reminded that all the laboratories were submitted to an evaluation every four years by their "peers" and that it will be the case for the Unit 200 in spring 1989. The communiqué concluded that at this moment the passions "will have calmed down to leave room to the indispensable serenity of the long-term scientific judgments".

This press release allowed the direction of Inserm to remind the policy of the Institute based on the “freedom of the research”, undeniably a noble task. When the work of the researchers at the Institute was threatened by outside elements, the direction of INSERM nevertheless considered that it should not intervene. It was thus a new version of the “fox in the free henhouse” applied to scientific research and its institutions.

We can summarize the criticisms towards *Nature* and its investigators through several main questions that we will be considering successively.

Critic n°1: “Why to publish these results if Nature considered that they were false?”

It was the most frequent criticism. As we saw, the – surprising – answer of J. Maddox was that J. Benveniste would have removed his article if an investigation had taken place before publishing the article. In a letter to *The New York Times* of September 26th, 1988, J. Maddox clarified his thought:

“For the well-being of the scientific community as whole, there is an urgent need that practitioners know that second rate science exist, can be exposed, and should be more openly categorized as such.”⁴

A posteriori, it is obvious that the investigators thought – even before entering the laboratory – that the results were false. However, after the investigation, their comments on the reasons of the “falseness” of the results were a bit discordant.

With their *a priori* ideas, it was absolutely impossible for the investigators to quit the laboratory with “positive” results (in the sense of J. Benveniste) or even with ambiguous results. We remember that J. Maddox had firmly expressed his obligation of result in these terms: “I had committed to publish the investigation report. The risk here was to end up in the situation where I would have a report whose conclusion would be: the magic is true.”⁵

The whole investigation report thus took care to highlight an absence of results. This was the reason of the sentence deleted at the last moment, which risked contradicting the rest of the article because, according to the words of J. Maddox himself, simple statistical fluctuations could not explain the results of the 4th experiment. However, at the same time, W. Stewart and J. Randi made statements in the press that were full of allusions. It should be reminded that this report was nevertheless signed by the three investigators. Obviously, it was J. Maddox who held the pen and who took care of erasing everything which could be understood as a charge of deceit. J. Maddox was a director of a newspaper and he knew that he was not protected from the laws on the press concerning the defamation. As he had no proof of a fraud, he based the text

mainly on technical questions of a statistical nature. Without making an unfounded accusation, it is possible that J. Randi and W. Stewart felt left out during the elaboration of the report and that they wanted to bring their “personal touch” outside the “official” report of *Nature*. This last hypothesis is not extravagant since according to an editorial manager of *Nature* “it was a bit of caper to get this particular gang together and take them to Paris.”⁶

In a letter which he sent to J. Benveniste during the summer 1988, J. Randi did not hesitate to say:

“[...] in the set of experiments that were supervised, double-blind,⁷ by the Nature team, we have positive proof that there was an (unsuccessful) attempt to cheat, and we know who did it.”⁸

Not long after, in *Liberation* of October 3rd, 1988 (quoting the Portuguese weekly journal *The Espresso* of the same day), J. Randi stated:

“We do not hesitate to assert that contrary to what was said or told, we possess the proof of fraud or more exactly deceit.”^{9,10}

Interviewed by the journalist M. de Pracontal, he even stated about the famous envelope stuck on the ceiling:

“If we wanted, we could look at fingerprints. I do not believe that it is necessary to make it, I do not want to destroy somebody.”¹¹

In another occasion, J. Randi again indicated:

“If fingerprints other than mine appear there, it will prove something. A friend, who works in a laboratory of police in Washington, suggested analyzing these fingerprints. I did not consider this necessary.”¹²

It was talking too much or not enough. It is really a pity that J. Randi did not accept the proposal of his police friend. We note that in August J. Randi knew the culprit and later he had only the means to know him. Afterward, J. Randi used more gentle words and spoke of “self-delusion”.

W. Stewart played the same game of allusions. We remember that he had asserted with a finger snap that the results were “made-up”. In *The New York Times* of July 27th, just after the publication of the report, he also adopted an off-the-wall position about the report:

“Their report avoided any charge of fraud. But Dr. Stewart said in a telephone interview that bias was “not an adequate explanation” for some of the reported dilution results. He declined to say

whether he thought there had been trickery, but he said that the uniformity of some test results was disturbing.”¹³

Likewise, answering to M. de Pracontal about the “bias of the experimenter”, W. Stewart affirmed:

“Unfortunately, it does not take into account all the results. As you know, the experiments were reproduced in an Israeli laboratory. But it raises a problem because there were not true reproductions. Elisabeth Davenas indeed went to Israel. On this occasion, she performed experiments whose results are published in *Nature*. Yet, these results are "too perfect" and they cannot be attributed to an observation bias because the countings were done blind. In this case, I have no other explanation than the deceit.”¹⁴

Contrary to what W. Stewart suggested about the experiments performed in Israel, we have seen that the repeated countings (in triplicate) were not blind; it was each dilution that received a code number. A bias of the experimenter cannot objectively be eliminated concerning the triplicate counts. However, the purpose of the experiment was to detect a difference between “active” tubes and controls. This point has been already discussed in Chapters 10 and 11 and we have seen that several explanations, not mutually exclusive, could explain these counts that W. Stewart considered as “too perfect”.

During the same interview, M. de Pracontal pointed out to W. Stewart that he did not notice a deceit at Clamart. He answered:

“No, except the fact that somebody touched the envelope containing coded data that we had stuck on the ceiling of the lab. But this attempt of fraud did not succeed. However, the precautions taken in the case of the Israeli experiment did not prevent a deceit. And there is another aspect than I do not want to discuss here.”

We will never know this “other aspect”, because when M. de Pracontal asked W. Stewart what he was referring to, the latter refused to say anything more because he explained: “I did not speak about that publicly before”.

These sudden innocent maiden reserves of W. Stewart were rather surprising for anybody who knew the character and his doggedness in previous affairs where he investigated. No doubt that if he had discovered a substantial proof of deceit, he would not have hesitated to make it public.

These inconsistencies between, on one hand, J. Maddox who tried to show that the results did not exist¹⁵ and, on the other hand, J. Randi and W. Stewart who insinuated that there was deceit enabled J. Benveniste to say:

“Let us underline incidentally a delicious contradiction: on one hand, Maddox who goes everywhere claiming that “there is no result” and on the other hand, Randi who accuses us of having cheated!

It would really be, for the first time ever, an absolutely extraordinary deceit: cheating to have no results!!! ”¹⁶

He also summarized these inconsistencies with this sentence: “A fraud with five laboratories and no results!”¹⁷

Critic n°2: “Nature went out of its role of scientific journal”

The mainstream press as well as some medical and scientific journals – regardless of their appreciation for the works of J. Benveniste – criticized during summer 1988 the attitude of *Nature*, which went out of its role of scientific editor and had played the role of a “scientific thought police”.

Thus, in *The Los Angeles Times* of August 7th, 1988, one could read:

“Science editors should not dismiss results out of hand simply because they conflict with orthodox views. Throughout history, much progress in science has come from just such challenges. Every new idea starts out being unorthodox. At the same time, it is also true that most unorthodox ideas are wrong. The problem is to distinguish the right ones from the wrong ones beforehand.

The editors of *Nature* probably acted correctly in publishing the paper despite their misgivings. It is better to err on the side of publishing too much than of suppressing a potentially worthwhile idea. Publication allows the results to be scrutinized and tested by others. Still, the magazine might have conducted its investigation before it published the paper rather than afterward.”¹⁸

In *The Scientist*, E. Garfield summarized all arguments very clearly. More specifically, he suggested the use of a procedure which – contrary to the investigation of *Nature* – would at the same time evaluate the research while respecting scientific approach and ethics:

“In sending its own team (including Maddox) to France to investigate the experiments, *Nature* showed poor judgement. Why the team did not include an immunologist is baffling. In broader terms, it is even more regrettable that the journal took upon itself

this role of jury *after publishing the article*. Why not before? A better course, as many have noted, would have been to send an independent, fully expert group before a decision to publish had been reached – in effect, a more intensive process of peer review. If it had done so, and had still decided in favor of publication, it could have printed it and the independent investigators' report in the same issue."¹⁹

And after:

"Furthermore, the investigators' report (July 28, pages 287-90), in tone and length amounting to a bludgeoning of Benveniste and company, only reinforces the question, "Why didn't they check this out before publishing it?" Moreover, Benveniste's seemingly sincere and wounded response (page 291) prompts real sympathy for the French investigator, despite what may be thought of his experiments and claims.

Nature made a regrettable series of editorial decisions – sloppy at best, irresponsible at worst. Even Walter Stewart, one of the investigators and a reviewer of Benveniste's original paper, now says that its publication was "an imposition on the scientific community" (Wall Street Journal, July 27, page 30)."²⁰

For other detractors of *Nature*, the journal was not sufficiently open to new ideas:

"The fury of Nature's attack on Benveniste has prompted some scientists to suggest that the journal is not open enough to unorthodox ideas. "If journals try to suppress or discredit heterodoxy, they will suppress both good and bad," says Harry Collins. "Marie Curie and her work would have fared very badly if she'd been treated like Benveniste." "²¹

The most direct and the most explicit criticisms (but not necessarily the most disinterested) came from other directors of prestigious journals, in particular from Arnold Relman, editor of the *New England Journal of Medicine* – the equivalent of *Nature* for medicine – and from Daniel Koshland, editor of *Science* (and additionally direct competitor of *Nature*...).

Thus, for A. Relman:

"What the journal should not have done [...] was publish the paper and then undertake an investigation itself. A journal should not be an investigative body, [...]. An editor's job is to see that material is rigorously and fairly reviewed [...] and when a journal acts as

Nature did, the editor becomes the judge, the jury, the plaintiff and – in some sense – the accused. Such a fraud investigation by the editor is a conflict of interest. [...]”²²

And A. Relman specified at another opportunity:

“Truth squads and special investigative teams are not only unnecessary, but would also be destructive of the scientific spirit.”²³

One could quote also the point of view of D. Koshland:

“D. E. Koshland Jr., editor of *Science* [...] said he found the original report "more flimsy" than the editor of a journal would like. Dr. Koshland said the improbability of the test results had been established by many earlier experiments and the data published in this case did not seem to make sense. They were "internally peculiar", he said.

The role of a general scientific journal, Dr. Koshland asserted, should be to "encourage heresy but discourage fantasy." While there is nothing wrong to publishing something that turns out to be wrong, he suggested, the situation is different when a proposition, such as perpetual motion or "memory" in water, is totally implausible.”²⁴

In France, the journal *La Recherche* also wondered about the strange chronology with publication initially and investigation over a second time:

“The investigators came to the laboratory of J. Benveniste fortnight after the publication of the article, why did not they come before? The composition of the group obviously implies that J. Benveniste is a fraudster, so why publish him? Among all experiments that have been performed, it seems that only one was not convincing and it was enough for W. Stewart to denounce the fraud; what is the meaning of these checks which were made in the most total confusion?”²⁵

But, curiously, the author of this article considers that “*Nature* probably underwent many pressures to comply with such a mock investigation”. As we have seen, this hypothesis does make sense because the investigation accompanied the decision to publish and the least we can say is that the initiative of the investigation and the conditions of the latter were the personal decision of J. Maddox.

A large number of scientists who did not approve the work of J. Benveniste thought nevertheless that *Nature* assumed exaggerated rights by conducting an investigation which moreover was similar to a “circus”. *Nature*, according to them, should not have published these results. So H. Metzger – who was one of the first experts of the article – and S. Dreskin in a *Correspondence to Nature* explained:

“It is reasonable to ask whether the observations of Davenas *et al.*, should have been published in *Nature*. We think not. One of us (H.M.) reviewed this paper in Avril 1987, and urged that the findings be checked by one or more laboratories chosen by the editor. Instead, Dr Benveniste made his own choice, and *Nature* decided to publish the report and then to despatch an investigative team consisting of the editor, a magician and a scientist, none of whom has experience in the relevant field. Their report provided no support for the published claims and will dismay serious scientists: it adds to the circus atmosphere engendered by the publication of the original paper. [...] We believe that the approach chosen by *Nature* is regrettable. We feel that all ideas no matter how revolutionary deserve to be heard. However, when new data are proffered that grossly conflict with vast amounts of earlier, well-documented and easily replicated data, a different editorial standard is required. Before the imprimatur inherent in publishing them in a leading scientific journal is granted, the new results must be reproducible by disinterested individuals familiar with the field.”²⁶

The use of the word *imprimatur* is rather unexpected about scientific publications because it seems to endorse the idea of an “official science”. We could add that the implementation of special editorial requirements for results that question the scientific knowledge would certainly have slowed down the diffusion of discoveries in the past. The opinion of H. Metzger reflects however a very frequent view of science. This conception of the scientific approach is justified when a new domain has been opened after a significant progress. It is however a conservative attitude which certainly obstructs the progress of new ideas when the preceding paradigms are questioned.

J. Maddox answered then directly to H. Metzger and to the other critics in an editorial in the same issue of *Nature*. First, he addressed to his “colleague” D. Koshland – without naming him – who had thoroughly criticized the coverage of the affair:

“Metzger goes on to echo a not disinterested toffee-nosed opinion recruited last week by the *New York Times* that journals such as this should not lend their reputation to spurious science by publishing it.”²⁷

Having settled a score with his competitor, J. Maddox then argued that journals as *Nature* received “a torrent of heterodox would-be literature offered for publication”, while underscoring that “it is rare that some such claim should come from a government-supported laboratory, that its principal author should urge publication in the face of common sense – and should complain that failure to publish will be tantamount to the suppression of the truth.”

But above all, according to J. Maddox, the non-specialized journals such as *Nature*, are also empowered with a role of information and education. Thus, he explained, “there is occasions when publication of spurious science may be a public service”. Then he quoted the example of an article published 16 years before about scotophobin where W. Stewart has already played an important role:

“Some readers may recall the case of scotophobin, a protein suppose to reside in the brains of trained rats which, when injected to naive rats, would transfer the first rat’s learned capacity to run a maze, for example. *Nature* published a version of such a manuscript after several preliminary accounts had appeared elsewhere, but accompanied it with a devastating critique from Mr Walter Stewart [...]. Nothing much has been heard of scotophobin since. Is not a little of the “circus atmosphere” inescapable on these occasions?”

Not that belief in the magical properties of attenuated solutions will be as quickly exorcised. Since the emergence of homeopathic medicine in the early nineteenth century [...] the theory of biological activity at extreme dilution has been a theory in search of verification. It would be naive to expect that the hunt for verification will now be abandoned simply because *Nature*’s opinion of Benveniste’s experiments is unsatisfactory.”

Here again one notices that it is undoubtedly homeopathy that J. Maddox had in line of sight. But the words “magical” and “exorcised” are strange under the pen of *Nature*’s director! We could also add that the silence following the prohibition of a research area did not prove *a posteriori* that there was nothing of interesting to explore.

Critic n°3: ‘The investigators were self-proclaimed experts’

Among the three investigators, the one who best exemplified the self-proclaimed expert of “scientific misconduct” was obviously W. Stewart. Taking his role of “Mister Clean” of science with utmost seriousness, he did not smile, he never laughed. As “hung-up” as J. Randi was extrovert, he was the exact opposite of the latter. J. Randi indeed was always ready to show one of his surprising magic tricks. In front of this professional clown, the absence of sense of humor of W. Stewart was even more obvious.

Thus, on the last day of the week of the investigation at Clamart, J. Benveniste joked and – tongue in cheek – proposed a position to J. Maddox when – the reality of high dilutions having finally been recognized – he would be at the head of a prestigious institute. W. Stewart – who attended the scene – took the proposal seriously. This is demonstrated in the report he spontaneously did to the journalist who interviewed him early 1989:

“He even told Maddox that when this was over he’d be happy to offer him a job. He was apparently serious, but I was flabbergasted. Even the world’s top scientists don’t go around offering job to John Maddox, who, as editor of *Nature*, already has a distinguished job.”²⁸

J Maddox recognized himself that the special behavior of W. Stewart was a problem:

“Stewart has no manners” [...], “He’s a zealot”. As the temperature rose, so did the pitch of Stewart’s voice”. Maddox explains, “He does have a high-pitched voice and when he’s tense, his voice sounds like that of a Dalek²⁹. We had to tell him to talk naturally.”³⁰

At the same time, pursuing his obsessional crusade for more “purity” in science, W. Stewart drifted during a colloquium over the ethics in scientific research. *Nature* distanced itself – once more – from W. Stewart and reported this revealing episode on the state of mind of this character:

“Stewart has incurred researchers’ wrath for his investigations of alleged scientific fraud, investigations that have been marked at times by an almost religious fervour. Indeed, at the Bansbury meeting, Stewart astounded participants by equating the moral taint of scientific fraud with that of Holocaust. Although his point was the responsibility for identifying and tackling problems falls on everyone’s shoulders, the idea that an incorrect scientific paper, even one written with knowing deception, can be in any way

compared with the slaughter of 6 million people suggests that his enthusiasm for his work has exceeded reasonable bounds; he may no longer be a credible force in these investigations.”³¹

It is unfortunate that, six months before, W. Stewart was considered by *Nature* as a “credible force”.

Critic n°4: “The experts have no scientific qualifications in the assessed area”

Concerning their lack of scientific qualifications, every investigator justified himself with his own arguments. In a letter that he sent to J. Benveniste during summer 1988, J. Randi explained that he nevertheless had some scientific past:

“As a youth, I took summer employment with the Banting-Best Laboratory in Toronto, Canada, as a mere glassware washer. I hardly required a doctorate in Detergent Science to fill that position but my employer recognized that my dedication in performing that simple task indicated that I might step up to more important involvement in the zinc-protamine insulin assays that were the product of the laboratory. I learned proper pipetting procedures and a rather sensitive sugar titration process upon which the entire bio-assay depended. [...] True, I have no academic background to support my claim; but I feel that I need not present my credentials and my passport before reporting a fire...”³²

Indeed, a fireman in the Opera does not need to possess the qualifications of a tenor. One does not ask him however to appear on stage or to judge skills of the singers.

As for W. Stewart, he willingly recognized that he had no doctorate and only a few publications to his credit. To a journalist who asked him how it was possible that in twenty working years, he published less than a dozen articles, he answered: “I publish only when I have something I think is worth communicating to other scientists. This hasn’t happened frequently.”³³

It is well intentioned not wanting to submerge his colleagues by useless readings, but apparently this rather short explanation was not enough for the NIH who employed W. Stewart. Indeed at that time, in an article of *New Scientist*, the Director of the NIH declared about W. Stewart and of his colleague N. Feder:

“They are supposed to be working scientist, and their scientific productivity has been extraordinarily low. They hasn’t been much originally for a while.”³⁴

And, according to *Science*:

“Stewart and Feder may be self-appointed guardians of scientific accuracy, but they have managed to get NIH’s approval to spend 20% of their time on investigations of published papers. In fact, they have been spending closer 100%, according to their supervisors.”³⁵

And after:

“By Stewart and Feder’s own admission, their research is somewhat on hold and misconduct studies occupy most of their time. They said it because NIH has so cut back their research resources that they can no longer do science. [...] They accuse NIH officials of retribution. In off-the-record interviews with *Science*, NIH sources argue that when space is tight, as it is all over the campus, you do not assign large amounts of space to unproductive workers.”

Then why did the NIH continued to employ researchers as W. Stewart? According to the same source of the journal *Science*:

“[...] it would be political suicide to go after Stewart and Feder, whose public status as whistle-blowers has gained them the protection of powerful members of Congress [...].

"It costs NIH perhaps a couple of hundred dollars to keep Stewart and Feder", [...]. "The political costs of dumping them would too high".

As regards J. Maddox, accused with his team-mates of amateurism by J. Benveniste, he answered with these arguments:

“The short answer to the question is that if a group of mere amateurs can so quickly discover procedural errors of such importance, that is sufficient justification.”³⁶

The argument is somewhat circular. Indeed, according to J. Maddox, what a self-proclaimed expert names “error” proves *a posteriori* his skill in the domain. We have seen the limits of this rather strange conception of the expertise. And, if we fully extend this reasoning, we can wonder why scientific journals – such as *Nature* – keep up-to-date lists of (true) experts in all scientific domains for reviewing of manuscripts.

Critic n°5: “Why was a magician present in the team?”

The presence of a “magician” in the team was a recurring reproach. It participated in the atmosphere of “circus” which for some – as H. Metzger, one of the experts appointed by *Nature* to analyze the manuscript – was harmful to the image of science. Still, nobody knew at this moment that not only was a “magician” was present in the laboratory of Clamart, but also a real “false medium”, namely J. Alvarez, the person who accompanied J. Randi! To further complicate the situation, we know now that J. Randi was aware that J. Alvarez was not the true name of his assistant and close friend. Indeed, this latter was travelling in France under a false identity that he had stolen some years ago... (See note 3 Chapter 9).

We have seen that the reason for the presence of J. Randi is now obvious. Indeed, J. Randi was not just any “magician” or conjurer. He was a founder member of the CSICOP (*Committee for the Scientific Investigation of Claims of Paranormal*). This association created in 1976 aims at pursuing and at denouncing what it considers as “false science”. In 1996, J. Randi created his own foundation. Certainly, one can only agree with any effort tending to develop critical and scientific mind. The reading of the papers of the CSICOP reveals however a rather primary scientism accompanied with some arrogance. In the reports of the CSICOP meetings, science is in fact barely mentioned and seems secondary. It is not the scientific knowledge which seems to be the mainspring of the association, but rather the pleasure to pursue, to chase away and to denounce. After the exploit has been achieved, mocking appears to be the main mode of expression of the members of the CSICOP.

J. Randi was thus not a neutral observer. He was not only a conjurer specialist in “manipulations” as J. Maddox had initially presented him. As W. Stewart, he led his own fight as self-proclaimed expert. Every new “trophy” added to his fame of debunker. He could then hold interviews, conferences, articles and broadcast shows. It was his “small business” which appeared then to work well. It is true that the credulity is widespread and that the absence of scruples of the quacks ensure him an almost unlimited business. But were methods used by J. Randi to denounce false prophets or astrologers adapted to scientific expertise? Is any singular phenomenon observed within a laboratory inevitably a matter of embezzlement or deceit? Is not there some risk of derive towards a thought police organized by uncontrolled brigades in the name of “scientifically correctness” that they would have defined themselves?

In fact, the question of the participation of a magician in the investigation of *Nature* means wondering: “*Why a plan of experiments or a detailed program was not performed?*” The answer to these two questions is the same. Indeed, the absence

of a plan of experiments and the presence of J. Randi share the same logic, namely that the investigators were not inspired by a scientific approach, but hoped to quickly find the proof that the experiments were forged.

In the context of a scientific approach, a plan of experiments would have allowed defining what was acceptable for all parties concerned. Particularly the results could have been published in a peer-reviewed article describing the experiments in detail. In case of a disagreement on the interpretation of the results, two contradictory articles could have been drafted. It was then building a scientific controversy with sound arguments, always more useful than the hullabaloo which resulted. We have the feeling that the certainty of the investigators to hold the truth authorized them to assume rights on the team of J. Benveniste by not considering them as full partners, but as subjects who allowed them illustrating their thesis on "scientists who delude themselves".

In the logic of the investigators, a plan of experiments could only disturb them in their research of the "smoking gun". Considering the laboratory of Clamart as a field of experiments, it is obvious that they preferred to keep control. A plan of experiments would have restrained them. It was preferable for them to decide only according to the events. That is why the first three days, the investigators wished only to observe how experiments were done and to consult the experimental data recorded in the laboratory notebooks. They appeared to glean from right to left in a kind of Sunday walk, as told by J. Benveniste:

"That lasted five days. When they arrived on Sundays, they did not even know how long they would stay! And every evening, we told them: "Well, that worked. Are you convinced? Is that enough?" – and they answered: "No, no! We want to do again tomorrow!"³⁷

The only one of the investigators who really appeared "to work" was W. Stewart. J. Maddox let him do, just calming him down when he was warming up. For example, when someone approached a little too much for his taste in the room where E. Davenas counted basophils under a microscope:

"Joking around, John Maddox pretended to be there only for lip service "Jacques, he told Benveniste, these experiments are really extraordinary. And you are so kind..."³⁸

Today, being aware of the frame of mind of the investigators at the onset of the survey, one understands better their behavior. They had to demonstrate during these few days that somebody cheated. Indeed, as clearly expressed by J. Maddox:

"We thought it quite probable that there was someone in Benveniste's lab who was playing a trick on him."³⁹

In other circumstances, J. Maddox declared:

“We envisaged the possibility of a joke, a hoax performed by somebody else than Benveniste, or a member of his team. Obviously we thought of a swindle, but I must specify that we found no proof in this direction. But, in front of such strange experimental data as those that Benveniste sent us, was it not normal to suspect the worst?”⁴⁰

And yet:

“We thought we would find a "poltergeist" or more seriously, some obvious errors.”⁴¹

During the same interview, J. Maddox specified:

“But, before coming to Paris, one year ago, we suspected that somebody could play a trick on him. That is why we included a professional illusionist in our team, James Randi. The latter, well known to have discovered and reproduced the “tricks” of Uri Geller, declared from the second day that his presence was no longer justified.”

This is the reason of the very open behavior of the trio, investigating in a very “naturalistic” way, observing the life of the laboratory, without particular constraints for anybody. In the casting, J. Randi had to unmask “the spirit-rapper”. It was his specialty. However, he quickly declared – on Tuesdays, as said J. Maddox – that his mission was ended.

The investigators then had to face the facts, the explanation which motivated their action, namely the presence of a cheater in the laboratory, did not hold water any more. It was nevertheless difficult for them to stop and to go home empty-handed. It was thus necessary to use plan B.

The problem was that they had most probably no alternative plan. They must then improvise. We enter then the second part of the investigation where the observers became actors and got involved in the experiments. As seen at the end of Chapter 11, it was also on Tuesday evening that W. Stewart concluded – after an erroneous statistical calculation – that the results reported in the notebooks were “too beautiful”. The tactics for the next days was then set up. The purpose was no longer of chasing away the presumed cheater, but of discrediting the experiments with statistical arguments and by resorting to what should be called an attempt of destabilization.

It was for this reason that J. Randi was quickly back in action and his sense of the staging was then utilized. One should not forget that J. Randi was first of

all a man of spectacle. He knew what was a show. He took part to television programs in the 50s, participated in a tour with rock star Alice Cooper in the 70s peppering the spectacle of surprising special effects, playing in particular on scene the role of a crazy dentist and that of an executioner (yes, it is indeed about the same artist of variety show who came to exercise his talents in a laboratory of Inserm). Therefore J. Randi also known as "The Amazing" had showmanship. Naturally, he was the one who had the idea of sticking the envelope on the ceiling. But what was the sense of this dramatization since it was a simple code made by W. Stewart who could have kept it in his pocket? J. Randi did not hide that it was a trap. Speaking about the envelope, he explained:

"Normally, it would have been necessary to give it to a bailiff. Or better to post it at the address of the lab, so that it would have been returned to us the next day without anybody being able to touch it. But I had been called in this place for a precise purpose: to assess all personalities.

I thus decided that the envelope would be stuck on the ceiling of the laboratory. So nobody could read the code without obvious trace. If somebody wanted to cheat, I would know it. To reach the envelope, it was necessary to use a ladder which was against a wall. Without anybody knowing about it, I made marks on the ground with a pencil to locate the exact position of the ladder."⁴²

The following morning, he noticed that "the ladder was moved by several meters". But, according to J. Benveniste: "the explanation is simple: my collaborator Yolène Thomas, penetrating the next day into her laboratory and seeing this ladder in the middle of the room, had considered logical to replace it where it usually rested."⁴³

Others were able to move it, including the housekeeper! Indeed, contrary to the words of J. Randi who asserted: "there was no cleaning team. The lab was closed from our departure to our return"⁴⁴, on one hand, a housekeeper came early in the morning and, on the other hand, there was not only J. Benveniste who possessed the keys of the laboratory and the code of the alarm. The alarm concerned all the Inserm building which housed several laboratories. Furthermore, anybody in the building could easily penetrate into the Unit 200. The number of people who would have been able to approach the ladder (and the envelope) was thus potentially high. Finally, if we take the assertions of J. Randi literally, it meant accusing J. Benveniste himself! When the investigators returned on Thursday morning with J. Benveniste, they were certainly not the first ones to penetrate into the Inserm building.

We see which types of arguments – closer to a novel of Agatha Christie or Conan Doyle than to a scientific expertise – the investigators used. But here again – since we are on the same lines of logic like in detective novel – it is necessary to wonder who benefited from the crime. Indeed, when the envelope was stuck on the ceiling, the experiments had been prepared and the plates of cell cultures were in the cold room, waiting to be counted. One could not change anything. One does not understand for what purpose the code would have been useful since W. Stewart distributed himself the samples to be counted under a new code (for an unclear reason since he held both successive codes...). As the journalist M. de Pracontal correctly pointed out:

“Randi seems to be a victim of the self-deception that he denounces in others: he is so sure of the reality of the fraud that he does not seem to take into account the inconsistencies of his demonstration.”⁴⁵

In spite of the numerous criticisms directed toward *Nature* and the inconsistencies of its investigators, there was now a doubt in the media. The media wave which followed upon the publication of the report vanished with the summer. J. Benveniste must now tackle year 1989, because as announced in the press release of Inserm of July 27th, 1988, the Unity 200 of Inserm had to be evaluated.

It was soon in front of his peers of Inserm that J. Benveniste had to explain himself, the stake being the survival of his laboratory and his position as director.

Chapter 13. 'If one wanted, one could look at fingerprints

Notes of end of chapter

¹ J.Y. Nau. Nouvelles polémiques sur la « mémoire de la matière ». Une commission d'enquête conteste les résultats du docteur Benveniste. *Le Monde*, July 27th, 1988.

² F. Nouchi. Passe-passe au laboratoire. *Le Monde*, July 27th, 1988.

³ Press release of Inserm of July 27th, 1988.

⁴ J. Maddox. A too polite silence about shoddy science: why scold those who expose error. *New York Times*, September 26th, 1988 (page A23).

⁵ M. de Pracontal. Les mystères de la mémoire de l'eau, p. 42.

⁶ B. Dixon. Criticism builds over *Nature* investigation. *The Scientist*, September 5th, 1988.

⁷ It was not a "double-blind" experiment but two successive codes (which were both known to W. Stewart...)

⁸ Letter of J. Randi to J. Benveniste of August 6th, 1988.

⁹ In the same article, J. Benveniste answered to these charges in these terms: "This new revelation underlines the internal contradictions of the group supposed to check our research work and demonstrates that they did not still arrive a tangible conclusion".

¹⁰ G. Pial. Nouveau trouble pour la mémoire de l'eau. *Libération*, October 3rd, 1988.

¹¹ M. de Pracontal. Les mystères de la mémoire de l'eau, p. 47.

¹² P. Alfonsi. Au nom de la science, p. 81.

¹³ W. Sullivan. Water that has a memory ? Skeptics win second round. *New York Times*, July 27th, 1988, p. A14.

¹⁴ M. de Pracontal. Les mystères de la mémoire de l'eau, p. 66.

¹⁵ In fact, an attentive reading of the report of *Nature* of July 28th, 1988 shows skillful equilibrium with veiled charge followed by an ambiguous denial. Here is how is described the opening of the envelope during the meeting of Friday: "Opening sealed envelopes is Randi's expertise. He found that the sealed flap of the envelope had detached itself at a surprisingly straight angle when the scotch tape attaching the code to the ceiling was pulled away, but inspection of the aluminium foil allowed him to pronounce himself satisfied that the code had not been read." In summary, if there was attempt, it did not succeed...

The scene of the opening of the envelope was filmed by J. Benveniste and can be seen into in the episode dedicated to J. Benveniste in the documentary series "Heretics" of BBC2 and first broadcasted on July 15th, 1994.

¹⁶ M. Alfonsi. Au nom de la science, p. 31.

¹⁷ J. Benveniste. Benveniste on the Benveniste affair. *Nature*, October 27th, 1988, p. 759.

¹⁸ The Nature of Science (Editorial). *The Los Angeles Times*, August 7th, 1988.

¹⁹ E. Garfield. Contrary to *Nature* ? *The Scientist*, September 5th, 1988.

²⁰ The complete reference quoted by E. Garfield is: "R. Hudson. *Nature* debunks piece it just published that supported homeopaths' claims. *Wall Street Journal*, July 27th, 1988."

- ²¹ B. Dixon. Criticism builds over Nature investigation. *The Scientist*, September 5th, 1988.
- ²² R. Pool. More squabbling over unbelievable result. *Science*, August 5th, 1988, p. 658.
- ²³ A.S. Relman. *New York Times*, October 17th, 1988, p. A20.
- ²⁴ W. Sullivan. Water that has a memory? Skeptics win second round. *New York Times*, July 27th, 1988, p. A14.
- ²⁵ Quand l'eau fait frémir les scientifiques. *La Recherche*, September 1988, p. 1005.
- ²⁶ H. Metzger and S. Dreskin. Only the smile is left. *Nature*, August 4th, 1988, p. 375.
- ²⁷ J. Maddox. When to publish pseudo-science. *Nature*, August 4th, 1988, p. 367.
- ²⁸ Interview: Walter Stewart. *Omni*, February 1989, p. 65.
- ²⁹ Dalek are extraterrestrial robots in a famous British TV series (Dr Who).
- ³⁰ S. Young. Breaking the laws of Science. Is Dr Benveniste a genius or a cheat? *Telegraph Week End Magazine*, (no date), p. 25.
- ³¹ J. Palca. Research, misconduct and Congress. *Nature*, February 9th, 1989, p. 503.
- ³² Letter of J. Randi to J. Benveniste of August 6th, 1988.
- ³³ Interview: Walter Stewart. *Omni*, February 1989, p. 65.
- ³⁴ Have the fraudbusters gone too far. *New Scientist*, July 11th, 1988.
- ³⁵ B.J. Culliton. A bitter battle over error. *Science*, 1988;241:18.
- ³⁶ J. Maddox. Waves caused by extreme dilutions. *Nature*, October 27th, 1988, p. 762.
- ³⁷ M. Alfonsi. Au nom de la science, p. 29.
- ³⁸ F. Nouchi. Passe-passe au laboratoire. *Le Monde*, July 27th, 1988.
- ³⁹ R. Pool. More squabbling over unbelievable result. *Science*, August 5th, 1988, p. 658.
- ⁴⁰ M. Alfonsi. Au nom de la science, p. 72.
- ⁴¹ J. Maddox. Plus vrai que « Nature ». *Le Monde*, July 26th, 1989.
- ⁴² P. Alfonsi. Au nom de la science, p. 41.
- ⁴³ J. Benveniste. Ma vérité sur la mémoire de l'eau, p. 68.
- ⁴⁴ M. de Pracontal. Les mystères de la mémoire de l'eau, p. 46.
- ⁴⁵ *Ibid.*, p. 49.