Chapter 22. "Their baby is in my bath"

"Benveniste should have recognized this anteriority"

Even if J. Benveniste in advance assumed all the risks related to the highlighting of his person, his assertive attitude as the only one to be able to extract the research in homeopathy from the darkness where, according to him, it stagnated was a motive of irritation for some "homeopaths" who participated in these studies. As a consequence, there were many repeated attempts to remind him that he was not the first one to experiment in this domain and thus – implicitly – that he could not take advantage of having "invented" the high dilutions, what his statements sometimes suggested.

To understand these questions of anteriority, which seem to have fed certain resentment, it is necessary to return to the origins of the story. We have already seen at the beginning of Chapter 2 that two research programs were simultaneously performed for both rival homeopathic firms Boiron and LHF (Boiron absorbed LHF in 1988).

Here is the chronology of the events that according to P. Belon, scientific director of Boiron, led to the experiments with high dilutions on basophils:

> "Jean Sainte-Laudy worked with us [Boiron] since 1981 on high dilutions inhibiting degranulation. We looked for an independent laboratory to duplicate these results. In 1982, we met Benveniste. He hesitated before accepting it the next year. In 1984, during a scientific congress in Florence, we presented our model and published an article in the *Journal de l'homéopathie*. This time, Benveniste lost his mind. He decided to publish on the subject. The affair with *Nature* harmed us a lot." [1]

And he specified:

> "If the first version of the article of *Nature* article, which was based on the model in inhibition, had been published, then Benveniste should have recognized this anteriority."

These words made J. Benveniste blow up:

> "Their baby is in my bath! Saying that the system would work in inhibition but not in activation is anti-scientific. Finally, Sainte-Laudy cannot have the anteriority. I have been working on degranulation since 1975. He practices my test. He even paid me royalties at the beginning. In 1984, at the Congress of Young

> Researchers, at Lille, I signed a paper on inhibition with Bernard Poitevin and professor Aubin, then another one in the Journal of Clinical Pharmacology." [2]

We certainly are not discussing questions of anteriority concerning the discovery of insulin or the elucidation of DNA structure. Furthermore, it is about a phenomenon which has not yet found a satisfactory explanation. It is also advisable to add that, for the reader who is not really familiar to the habits of the scientific community, these considerations can seem rather narrow-minded. Nevertheless, this precision allows us to correct the cliché which was very widespread in this context of a research "bankrolled by the homeopathic industry". The reality was, as we can see, obviously different.

It is indeed indirectly that J. Benveniste became interested in homeopathy. It was by the common point of high dilutions that he was connected with it. In fact – and he expressed it on numerous occasions – he did not look "to prove homeopathy". If there were some points of convergences, this did not disturb him. But for him, the approach of Hahnemann – "father of homeopathy" – was not a scientific and rational approach. According to J. Benveniste, if the effects of high dilutions eventually were to be proven, this could be nevertheless the end of homeopathy. For homeopathic physicians and industrialists of homeopathy, this point of view was naturally unthinkable. For them, high dilutions were certainly an aspect of homeopathy, but it was not the only one. They also mentioned the "law of similars" and they insisted on this specificity of homeopathy, a "global" medicine according to them, very far from "classic" medicine which they call "allopathic".[3]

"He wedged his model"!

As everyone knows, "victory has many fathers, but defeat is an orphan". In 1997, the main protagonists of the "affair" were questioned by E. Fottorino for *Le Monde* which told the "affair" in three long articles from 21 to 23 January 1997. The unspoken feelings could then be freely expressed. The scientific director of Boiron gave a free rein to his resentment:

> "Philippe Belon considers that the faux pas of 1988 in Nature entailed a delay of ten years in the recognition of high dilutions. "We were branded as a shame of the science", he says. On the works of doctor Benveniste, his opinion is clear: "He wedged his model (*sic*). The peaks of activity are not stable. The only possible conclusions must be statistics. Yet the summation of his results is not significant. Elisabeth Davenas had pushed too far. Benveniste leaned on a single experiment which worked. If he had redone it a thousand times, there would have been no problem. But what he

> published in Nature, he does not know how to reproduce it, even in his laboratory. And nobody knows." [4]

About the article of *Nature*, P. Belon declared that "the published text is not the one that he had signed":

> " "Since 1982, we worked with Benveniste on the test of degranulation of basophils which he unquestionably developed. But our researches concerned the inhibition of the phenomenon and not the direct cell activation. I agreed with the first two versions of the text sent to Nature, because they dealt with inhibition. The final text described a direct activation, I did not read it" Why didn't he say it? "I was in an awkward position. I preferred to keep silent about it and continue working on our initial model"."

As for B. Poitevin, he "did not agree with the article of the Academy of Sciences":

> "On the activation of basophils, only one experimenter, Elisabeth Davenas, obtained results. It did not work with the other one. Benveniste offended her. I repeat: it was necessary to say that the phenomenon was difficult to reproduce. As for the model of inhibition, it worked in both cases."

He too seemed to deny an article of which he was nevertheless signatory. One also finds in these words the same emphasis to distinguish the effects "in inhibition" from "direct" effects. He pursued:

> "When Elisabeth worked "open-label", we noted an avalanche of good results. I believe that technical errors could increase the chances of obtaining positive data. But the curves of activities were not imaginary. It was only necessary to finalize the reproducibility of the system and to say that it was difficult to repeat as long as all parameters were not mastered. Benveniste refused."

It is difficult however to understand how the same method was acceptable "in inhibition" and would be to blame for all the troubles "in activation".

For the reader who could be a little misled in front of these arguments, we can summarize the point of view of the "homeopaths" – without betraying their thought I think – by saying that there would be, on one side, a good research in homeopathy – "in inhibition" in the case of basophils – with homeopathic products (or with histamine which, under the name of *histaminum*, is also a homeopathic product sold in pharmacies…) and, on the other side, the research "*à la* Benveniste" which did not care to be in agreement with the principles of Hahnemann and would risk to make lose its soul to homeopathy.

It is also possible that the mistrust towards J. Benveniste, as expressed by the "homeopaths", was related to the fear of being exposed behind Benveniste who only dreamed to confront in a challenge worthy of him on the international scientific scene. For the "homeopaths", there was a great risk that one sees that the "king was naked" or at least very slightly dressed.

"Is it Benveniste-like without Benveniste, as water would produce a molecular effect without molecule?"

These articles in *Le Monde* in 1997 brought nevertheless important information. Indeed, the reader of *Le Monde* could learn with interest that on the initiative of Boiron Laboratories and their scientific director, P. Belon, an international study concerning the effects of high dilutions on basophil degranulation was performed and that the results were positive!:

> "Professor at the University of Louvain, biochemist and toxicologist Marcel Roberfroid recognizes to have coordinated the experiments of four European laboratories on high dilutions (in France by doctor Sainte-Laudy, in Italy, in Holland and in Ulster). But, he specifies: "My purpose is not to know whether Benveniste is right or not. I apply the test of Sainte-Laudy, not that of Benveniste. This last one had no knowledge about our works." [5]

What is the difference between the "test of Sainte-Laudy" and the "test of Benveniste"? J. Sainte-Laudy replaced the blue of toluidine which was the staining agent of basophils by another one, namely alcian blue. But how this technical modification was decisive in the study of high dilutions? Was it only a simple change of a biological test or was this modification crucial in the case of high dilutions? This question was already raised by the journalist M. de Pracontal a few years before. With a lot of goodwill, he candidly asked J. Sainte-Laudy why he had changed the staining agent in the method of basophil counting. He then answered:

> "In 1986, I changed the staining agent because there were major problems with toluidine blue […]. From 1986 to 1988, I confirmed that the results obtained with toluidine blue were also observed with alcian blue. I think that persisting with toluidine blue is a scientific and diplomatic error. The technique with toluidine blue can be performed but in conditions less good than the technique with alcian blue." [6]

The journalist, who was brought down by this convoluted answer, added: "In brief, it works without working, while working nevertheless. Make sense of that if you can.".

Besides the change in the staining agent, the experiments of the European study were performed in "inhibition" (with histamine) and furthermore on the "first peak" of degranulation. Consequently:

> "Professor Roberfroid, the scientific director of Boiron, Philippe Belon, and Jean Sainte-Laudy rely on this difference to deny Benveniste the right to claim some confirmation of his own experiments.
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> Is it Benveniste-like without Benveniste, as water would produce a molecular effect without molecule? No, Roberfroid answers, who considers the expression "memory" of water as a "speculation". "I will not take a position. Science does not still admit the effect of high dilutions. Then, speaking about memory…"
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> Philippe Belon recognizes that the publication of the works of the Belgian professor will help Benveniste, while insisting on the difference of method. "That of Sainte-Laudy preceded that of Benveniste"."

The determination to keep away from J. Benveniste was clearly present in these words. We were thus again in the presence of the strategy "anything but Benveniste", but this time, from what an outside observer would consider as the "natural" camp of J. Benveniste. It is true that J. Benveniste, *volens nolens*, was almost automatically associated at this time with any allusion to high dilutions or to "memory of water". Justified or not, these technical subtleties or these battles of egos were difficult to understand for anyone was not directly involved in this research. For an outside observer, any positive experiment in favour of "memory of water" was always followed by the idea: "and if Benveniste was right?" The article of *Nature* was the reference which everybody remembered. Previous articles published in "minor" or confidential journals were forgotten even if in fact they said nothing very different. As J. Benveniste did not hesitate to claim, these articles were only "farts of rabbits in the stratosphere". He was the only one, according to him, who could bring this research theme on the baptismal fonts of science.

However, when he heard about this international study, the reader of *Le Monde*, who did not grasp technical quibbles and questions of egos, could wonder: "and if Benveniste was right?"

Notes of end of chapter

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1 E. Fottorino. La mémoire de l'eau. Une vérité hautement diluée. *Le Monde*, January 23rd, 1997.

2 E. Fottorino. *Ibid.*

3 We do not envisage in this book the question of homeopathy as therapeutic practice.

4 E. Fottorino. La mémoire de l'eau. Du rêve au soupçon. *Le Monde*, January 21st, 1997.

5 E. Fottorino. La mémoire de l'eau. Une vérité hautement diluée. *Le Monde*, January 23rd, 1997.

6 M. de Pracontal. Les mystères de la mémoire de l'eau, p. 184.