

Foreword

The circumstances that guided the elaboration and the writing of this book deserve some explanations. I entered in 1984 as thesis student in the laboratory of J. Benveniste (Unit 200 of Inserm in Clamart) after studies of medicine and biology; my area of research concerned polymorphonuclear basophils, a type of white blood cells involved in allergic phenomena. It was thus quite naturally that I spent a part of my time with the team “high dilutions” of the laboratory.

During this period the case “*Nature* vs. Benveniste” occurred as told in the first part of this book. Left in 1992 towards other horizons, I had then the opportunity to work from 1996 to 2000 in the immediate vicinity of the team of J. Benveniste. Great changes had taken place in a few years. Indeed, the “high dilutions” had given way to the “electromagnetic transmissions”. Without being a member of the team, but having kept a friendly relationship with all members, I was in a favored position to observe the surprising experiments of my former colleagues, coming occasionally to help for a “blind” experiment. Nevertheless, I was not a team member any more, but spectator of a fascinating phenomenon. I had the feeling – whatever would be the outcome – that with these singular experiments a chapter of the history of science was written under my eyes. From this moment, I tried to understand, I questioned, I noted. Initially actor, I became an observer. Quickly, the object of my interest slid from “memory of water” towards “the phenomena observed by the researchers who study memory of the water”.

Throughout these years – about twenty years – many people who knew my interest for this subject spontaneously transmitted to me information, which added to data from my previous personal work and to my own notes and observations. The classification of the documents and the progressive reanalysis of the experimental results eventually revealed a history which had its coherence and its *raison d'être*. The drafting of this story was becoming imperative. But questioning appeared for referencing some primary documents such as reports of experiments or correspondences. Indeed, for some correspondences – those of J. Benveniste for example – whose knowledge I had had because of my functions or by another channel of information, was I authorized to state them? The content of these letters of which I had copy, did it belong to Inserm? To their addressee? To the legal successors of J. Benveniste? To his co-workers? To the private law company that J. Benveniste funded? Not retranscribing in its entirety these correspondences, I took the view that the usual right of quotation could be granted to me.

The issue of the experiments was more delicate. Did I have the right to describe them? There is a not written principle that considers that an experiment belongs to the researcher who designed and performed it. J. Benveniste not being any more among us, the same questions of “inheritance” could nevertheless arise. Could I describe only experiments with my participation? The narrative risked to be strangely abbreviated. Was it necessary for each experiment to ask for authorization to his/her author or to his “moral heir” in the absence of a “scientific heir”? By doing so – notwithstanding the heaviness of the initiative – was there not a risk to derive towards an “authorized” narrative, but amputated by some quite enlightening episodes?

A beginning of answer appeared by considering the numerous documents that J. Benveniste always widely spread. Indeed, all those who were familiar with him know that J. Benveniste wrote extensively and maintained a dense network of correspondents to whom he sent many letters. In particular, he regularly sent detailed reports of his experiments to French and foreign scientists by means of large mailings to inform them about the progress of his work. Furthermore, a large part of the results of these experiments were already described, at least in their main lines, for example in Schiff's book¹ or on the web site of the laboratory of J. Benveniste. Other results were in the public domain, having been reported at congresses in the form of “posters”; these results are available in the scientific libraries or on line.

A lot of information was also available in texts of patents on “digital biology”. These patents are now public and describe in detail the experimental devices and the results obtained with them. Besides, an American multidisciplinary team appointed by an agency of the American Army evaluated an automatic analyzer designed by J. Benveniste and his collaborators. This device was intended to demonstrate the principles of “digital biology”. The methodology and the experiments that were then performed for this expertise were described in details in a scientific article published in 2006. And, despite new information that I give on the genesis and the developments of “digital biology”, it agrees with the conclusion of this expertise.

These examples thus show that a large part of information concerning the experiments performed by J. Benveniste and his team, either within the framework of “high dilutions” or in that of “electromagnetic transmissions”, is available, but in a scattered way and says little for people who are not familiar with this subject.

¹ M. Schiff. Un cas de censure dans la science (1994). *Albin Michel*

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The scenario would have been quite different if these experiments had been performed in a discreet and confidential small circle not wishing to communicate with the outside world. Indeed – to say the least – J. Benveniste wished that his results benefited from a maximal “visibility”. This attitude that ended in the arm-wrestling with the journal *Nature* had for consequence to raise high expectation in the public. The counterpart of this publicity that was made around “memory of water” is that the same public retains today a feeling of unfinished story and is still wondering “if Benveniste was right”. For the sake of fairness, it would seem thus normal to bring to our contemporaries – scientists, amateurs of sciences, curious persons or simple citizens – all the elements available on this story. Furthermore, these experiments were performed – at least partially and even if it was against its will – thanks to infrastructures and to financing of a public institution on which every citizen holds a legitimate right to inspect.

Finally, not leaving any point in the shadow is also the best way to finish with a number of rumors, approximations, preconceived ideas and untruths of which this story was rich. It is for all these reasons that I adopted an attitude that seemed to me the most reasonable, the most honest and the most relevant at the same time from a scientific point of view, but also towards the history of science to which belongs now this famous episode. To do so, I included in the text any document or information about the only basis of its scientific interest or for the understanding of the story.

As I could not quote everyone, I thank all those – in particular my former friends and colleagues of Inserm – who, sometimes playing “Mark Felt”, brought regularly documents or information to my attention; I thank more particularly Jamal Aïssa and Larbi Kahhak who were always opened to my questions and were also pleasant “bench mates”. I keep a particular gratitude towards Peter Jurgens with whom I had frequent and passionate discussions during which we shared our perplexity in front of the “phenomena of Clamart”. He drew my attention on certain illuminating details. If he had not left us too early, I would have been happy to have his comments on this text.