Chapter 3. From “high dilutions” to “electronic transmission”

Back to “high dilutions”

Before speaking about the developments with the “telephone for molecules” in Chapter 4, let us go back a few years and see which thought process J. Benveniste followed in order to set up these outstanding experiments.

The first experiments with the device of Langendorff and high dilutions took place in March 1990. Indeed, a researcher of the laboratory, Lahlou Hadji, then used this experimental model to study the effects of the mediators of inflammation and allergy on heart functioning. Quite naturally, given the context and the “high-dilution” atmosphere which reigned in the laboratory, L. Hadji studied if substances which modified the functioning of heart at “classic” concentrations had also an effect at dilutions where molecules had virtually disappeared. High dilutions of paf-acether – the mediator discovered by J. Benveniste – were thus prepared according to the usual method of dilution and shaking. Positive results were obtained – as well as with high dilutions of histamine – and it appeared that the most reproducible and most marked effects were observed on the coronary flow.

It was thus a major result. It meant that the results obtained with basophils could be generalized to another experimental model. Moreover, this new model possessed a notable advantage. Indeed, it allowed visualizing “in live” the effects of high dilutions without any intermediate. It was thus much more convincing than the previous experiments with basophils.

We remember that high dilutions had an effect on basophils that depended on the place of the dilution in the series and gave the famous “sinusoidal” curves. To avoid testing long series of dilutions, an ingenious method was used. A series of successive dilutions – generally the dilutions from $1/10^{31}$ to $1/10^{41}$ – were mixed. This high dilution was named “pool 31–41” and was often used during these experiments. Figure 3.1 shows the effects obtained with histamine at a high dilution in two experiments as an example of experiments performed in January 1991.

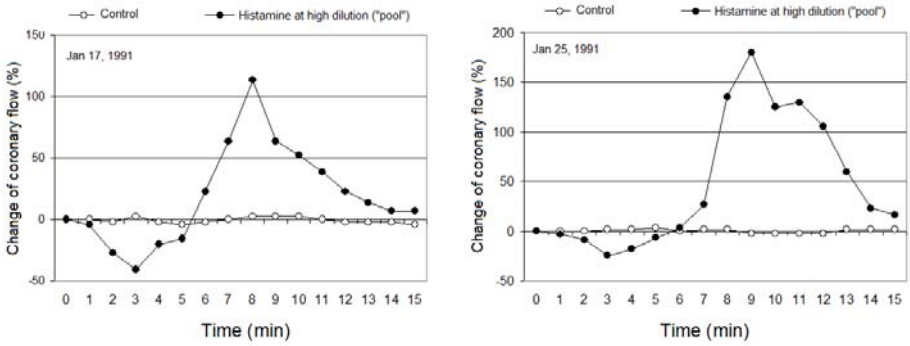

Figure 3.2. This figure shows the effect of histamine at high dilution ("pool 31-41") in 2 out 10 experiments which were performed from January 17th to 25th, 1991. One notes a large change of coronary flow which exceeded 100%. Such large variations were only rarely observed afterward (for the 10 experiments the mean change of coronary flow was 51%).
The high dilution of histamine was obtained by dilution-shaking of histamine at 1 mmol/L up to dilution $1/10^{41}$. The dilutions from $1/10^{31}$ to $1/10^{41}$ were then mixed. Before being injected into the infusion circuitry of the heart, this "pool" of high dilutions of histamine was diluted with shaking at 1/1000 in physiological saline that was used for infusion. During this series of experiments, each injection of histamine at high dilution was preceded and followed by injection of a control prepared in the same conditions, but with solvent alone.

"The high dilutions, we do not know how it works, but it works"

The device of Langendorff offered then a unique opportunity to move forward in the understanding of the physics of high dilutions. This period reminds us of the one which followed the "discovery" of the second peak of basophil degranulation (cf. first part). A wide boulevard seemed to open under the feet of the Clamart team. With the system of Langendorff, the effect of physical means (heat, electromagnetic radiations) could be studied in a relatively easy way. Thus, the effect of heat which had been highlighted with the basophil model was found again: heating at 70°C for 30 minutes "erased the memory". The specificity was also highlighted; an inactive analog of histamine (methyl-histamine) had no effect at high dilutions.

J. Benveniste kept in mind the theory of the Italian physicists, Giuliano Preparata and Emilio Del Giudice, which was published in the same year as the article of *Nature*. This theory could be the support of a possible "memory of water", as J. Benveniste explained:

> "The Italian physicists had developed a theory known as theory of the "coherent domains", which postulates that the molecules of solids and liquids are not connected between them only because they exercise electrostatic forces on their neighbors, as it is usually admitted. According to their theoretical model, these molecules would also exercise long-range *electromagnetic* forces and fields between them." [1]

J. Benveniste thus moved towards an explanation of the "memory of water" where these hypothetical long-range electromagnetic fields would play an important role. It should consequently be possible to modify the effects of the high dilutions by submitting them to electromagnetic fields:

> "To verify this, I got in touch with physicists of the Central Laboratory of magnetism of the CNRS at Meudon. […] We designed together a protocol of experiments: I sent a series of test tubes to this laboratory, containing histamine at usual doses and histamine diluted up to 10^{-41}. On site, the various tubes were submitted to electromagnetic fields with a low frequency. […] About hundred experiments were performed in 1990 and 1991 (in particular with histamine, but also with other active compounds)."

These experiments, performed blind with the cooperation of two CNRS researchers, Marcel Guyot and Vladimir Cagan, allowed J. Benveniste to conclude that the physical support of "memory of water" had an electromagnetic nature:

> "With the the hearts of guinea pig infused with various liquids, I notice that magnetic fields inhibit the effect of histamine at high dilution whereas they have no effect on histamine at usual active dose. […] The laboratory of the CNRS in question can testify the reality of the results of these blind experiments. These researchers often repeated to me: "The high dilutions, we do not know how it works, but it works" ". [2]

J. Benveniste did a scientific communication on these results as a "poster" during a congress of the FASEB (*Federation of American Societies of Experimental Biology*) in 1991. [3]

Difficult days

However, dark clouds accumulated in the sky of Clamart. Indeed, after the enthusiasm of the first experiments, the spectacular biological effects which were then observed became scarcer. The exploration of the physical properties of the high dilutions passed in the background. The priority was to find a stable biological system reacting to high dilutions. To explain these difficulties, reasons concerning the sensitivity of animals according to the season and according to the state of immunization were hypothesized by J. Benveniste:

> "These experiments gave impressive results and then relatively irregular results until December 90. However we obtained enough information to be able to present an abstract to the congress of the FASEB [4] in April 91 concerning the first results obtained in autumn 1990 on histamine at high dilution on isolated heart of guinea pig. At this date, we had also collected enough elements with M. Guyot and V. Cagan to show an inhibition by a magnetic field of 50 periods 150 oersteds during 30 min […].
>
> However it appeared that the sensitivity of guinea pigs to histamine, even at usual concentration, was variable, most probably according to the season and, furthermore, according to poorly known experimental variations." [5]

In this uncertain context, the first public demonstration was nevertheless programmed on February 13th, 1991. The results were not convincing as J. Benveniste told in his report:

> "This first session for the demonstration of an effect at high dilution on the heart of guinea pig, in front of people who did not belong to the laboratory, has been instructive. Let us specify that, on this day, the heart did not work as we expected. There is approximately one heart among ten which does not react at all to histamine, but the type of reaction that we saw today is seemingly unique. Indeed, while the heart was generally stable, it began reacting to any injection by a weak but clear and *immediate* increase of the coronary flow, with either histamine or diluted buffer." [6]

These difficulties have nothing unusual in physiology but, for a first demonstration, these trivial problems were particularly inopportune. Other public experiments, on April 3rd and 15th, took place with results which were not more encouraging. [7]

Faced with these technical difficulties, a new protocol was set up. Histamine was discarded and replaced by ovalbumin (white egg albumin), a protein often

used to induce allergy in laboratory animals. Animals were thus prepared a few weeks before with the injection of ovalbumin at "classic" concentration and a reaction of the heart was induced by the same protein at high dilution:

> "As a consequence, during year 1991, we began to increase the sensitivity of guinea pigs by immunizing them against a very sensitizing antigen, ovalbumin, associated with an adjuvant capable of increasing the production of antibodies, the complete Freund's adjuvant. At the end of December 91, we had enough information to be able to send an abstract to the FASEB again […] reporting a reaction of hearts to highly diluted albumin." [8]

But again the experimental results became disappointing:

> "However, the results continued to be erratic, excellent for a few weeks, and then null. Altogether, these variations could not be imputed to the system at high dilution because they also occurred on hearts stimulated with normal concentration. In fact, the practice of the technique was rather unreliable in the laboratory and at that time we had many difficulties obtaining an experimental regularity of the experimenter and of the researcher in responsibility."

J. Benveniste, not succeeding in understanding the source of these variations, was eventually persuaded that the source of these problems was a lack of care and precautions during the experiments led by L. Hadji. A conflict emerged between J. Benveniste and L. Hadji which ended at the departure of the latter from the laboratory. A malaise persisted after the conflict because the reasons of the grievances of J. Benveniste towards his researcher seemed irrational and questionable both scientifically and humanely. In a tense atmosphere, J. Benveniste had nevertheless to resume the experiments with staffers having no experience of this biological system which required some dexterity and long experimental practice. Other difficulties arose and they were then interpreted as water "contamination":

> "From January 1992, we have thus changed the staff and resumed both the process of immunization and the various experimental steps from the beginning, because from this time we were very worried about contaminations by endotoxin, coming for example from water used for infusions." [9]

We will dedicate a complete chapter to the question of the "contaminated serum". But these temporary difficulties with the biological model were forgotten for a while because a major event arose in spring 1992.

The "invention" of the electromagnetic transmission

As we will repeatedly notice, when the experimental system became difficult to master, a cunning improvement of the experiment or a new attractive technique each time allowed "to relaunch the machine" and to find faith in future. In this case, a decisive event opened a new chapter, the advent of "electromagnetic transmission". The idea that the support of the effects of high dilutions was electromagnetic made its way because, as J. Benveniste said:

> "In spring 1992, I speak about these experiments done in association with the CNRS to a friend electronics engineer.
> "If it is an electromagnetic field which is emitted by molecules, he explains to me, you must be able to do it get through an amplifier and to make it circulate"." [10]

The friend of J. Benveniste then built a low-frequency amplifier using a cheap kit which one finds in electronic shops. Two electric coils (solenoids) were connected, one at the input and the other one at the output of the device. Having placed a tube of histamine on the coil at the input and a tube of "naive" water on the coil at the output, the first experiment could be performed:

> "I let the amplifier work during fifteen minutes with maximal volume. For the first testing, the content of the tube at the output, infused in the Langendorff system, induced a response of the heart of isolated guinea pig." [11]

The fact that the experiment was a success as soon as the first attempt remains intriguing for anybody who has some experience about experimental work. It is a permanent feature during this story to see the first attempts almost systematically successful. Thus, the first experiments performed in association with the Laboratory of magnetism of the CNRS to "erase the memory" were performed for practical reasons with fields of low frequency at 50 Hz (the same frequency as mains electricity). In case of failure, higher frequencies would have certainly been tried. But, here again, the first attempt was the good one. Concerning the amplifier, it was far from evident that a cheap amplifier limited to the audible frequencies (20 to 15 000 Hz) would work. Indeed, one would rather expect electromagnetic waves at high frequency if they were the support of the effect of high dilutions as explained by J. Benveniste:

> " […] the physicists consider that molecules taken individually emit vibrations of very high frequency (in the terahertz). Making the hypothesis that they would emit signals in the range of sound waves […], what must be indeed the case since a phone amplifier transmits them, would be thus incompatible with the dominant theory. But this contradiction could be overtaken if we do not consider the vibration (*one* wave), emitted by a given molecule, but wave trains, that are billions of vibrations emitted by a molecule or a set of molecules *every second*. We collect in this case the "beat frequencies" of this train of waves, which is the average of the differences between the frequencies. The beat frequencies summarize the billions of vibrations in a single wave whose the frequency could presumably be in the range of low frequencies." [12]

The explanation of the phenomenon with low-frequency beatings is thus an *ad hoc* explanation which allowed reframing the theory with the experimental facts and therefore to "save the phenomena". Indeed, nothing proved at this stage that this explanation was the good one. Moreover, low-frequency beats between two waves require that they have very close frequencies (less than 1 % of difference).

"The perfect trap"

During the summer of 1992, blind experiments with a public were again performed but now with the system of "electromagnetic transmission". Thus, on June 16th, 1992, a public demonstration was performed in the presence of visitors, in particular M. Schiff who will be soon talked about. But, as J. Benveniste indicated in his report: "the results were not satisfactory".[13] And he added:

> "The "transmitted control" was negative but not the naive vial which induced a slight reaction after a simple dilution. We had not seen a wrongly positive control for several months! On 17th, this vial once again induced a mechanical and vascular reaction. Other vials of distilled water [with brand name] Biosedra also induced a reaction of the coronary arteries. Conclusion: the water in bottle is excellent; this one of the same brand in vial is contaminated! The perfect trap".

We will talk again about this experiment of June 16th because it was the starting point of the "contaminated serum" affair that we will describe in Chapter 5. In the same report, J. Benveniste underscored again the difficulties of these demonstrations with spectators:

"Furthermore, we observed that it is materially difficult to perform complex experiments, implying numerous steps, each of them being crucial, in the middle of five to six people who cannot remain silent and motionless. Demonstrations can be made, but with a simpler protocol: an active vial versus one control. […] Since 16th, five or six transfers were performed with a total success, including a blind experiment and including a heart which definitively stopped having received distilled water imprinted with information from "histamine" log 31–41, that is distilled water."

Unfortunately these last successful experiments mentioned by J. Benveniste were not performed with the participation of outside visitors who could testify. A new demonstration was performed on June 30th, 1992 in front of visitors but, again, it was a failure:

> "The results of the analysis are clear: the two experiments with histamine did not work, with numerous controls giving positive results and, on the contrary, tubes supposed to be active giving no result. On the other hand, we detected 7 ovalbumin tubes (OVA) among 7. […] The results of the samples OVA are particularly clear, in particular when we compare the very positive effect of the sample 15 on the OVA-immunized heart with the heart of a guinea pig having received only the adjuvant (alum) where the same sample gives no result. This indicates that a transfer indeed occurred, that it is completely specific, but that we are still disturbed by very numerous background noises." [14]

He added a postscript on July 2nd before sending this letter to the participants: "the controls in distilled water, saline solution and clean sterile vials are negative. Transfers work […]. All these experiments are open-label. If this is confirmed in blind experiments during several days, […] we could resume our games."

On July 9th, the experiment described in Chapter 1 was performed and satisfactory results were finally obtained. J. Benveniste hoped that this celebrated demonstration was the first one of a series of successes which would allow him to convince the scientific community that his approach was valid.

A participating researcher

During some of these experiments, we saw Michel Schiff's silhouette, researcher at the CNRS, making its apperance. A physicist by training, M. Schiff then turned to human sciences and sociology of sciences. Having ended a thesis of

physics in the United States at Yale University (New Haven, Connecticut), he returned to Paris and entered the CNRS to study nuclear physics in the Leprince-Ringuet laboratory at the Ecole polytechnique. In 1970, he radically changed his area of study and approached experimental psychology. He then studied the role of social background and heredity on the intellectual performances of children who had comparable genetic capital but were adopted by families having different social and occupational levels. This work was published in 1978 by the journal *Science*. He also wrote several books concerning the school system and the place of the experts in the society.

Early 1992, M. Schiff attented J. Benveniste's experiments out of curiousity, to which he did not grant much credit at first. The attitude of numerous scientists and the passionate reactions incited him to focus on this affair. He explained his approach in these terms:

> "I have been working since March 1992 to conduct a participating research on the memory of water. From a study of laboratory notebooks on high dilutions, I began to participate in a more recent research in Unit 200 of Inserm, essentially as adviser on some methodological points. In this function, my previous practice of research in physics is useful for me. My current research is however centered on the researchers as knowing subjects, and more exactly on the obstacles to communication and scientific knowledge. [15]

On another occasion, he specified in which state of mind he began this inquiry:

> "It appeared to me that, if I came to see Benveniste and his co-workers with a suspicious state of mind to lead an inquisitory-like inspection (as the investigators of *Nature* who had prompted the affair), I would accomplish nothing and I would miss most of the processes. Even if I avoided being quickly expelled, I would not succeed in acquiring information necessary to real understanding. That is why I decided to try a participating research: in exchange for helping in the current research, I would obtain information about this research." [16]

And he added:

> "During the year 1992–1993 (which covers the main part of my inquiry), I came to Clamart only twice a week on average, generally on Monday and Thursdays. The rest of the time, I examined documents. I also reserved time to think and take some distance

> with regard to the research in which I had decided to get involved." [17]

In spite of his initial skepticism and critical distance, M. Schiff eventually adopted a position similar to that of J. Benveniste – even if it differs on the idea of "crucial experiment" – namely that the explanation of the observed phenomena is in water itself:

> "Instead of raising a problem, which one had to eliminate as quickly as possible, the memory of water would be on the contrary one of the elements of the solution of a scientific puzzle. The memory of water would be thus a detail among others which would include some problems of physics of condensed matter (in particular water), the effects of alternate magnetic fields on some cell processes and also chemical communication inside cells". [18]

As for A. Spira and later Didier Guillonnet, M. Schiff's rigor allowed "channeling" J. Benveniste who had a natural tendency to leapfrog the steps, not hesitating for example to change two parameters at the same time during an experiment. Mr Schiff brought some methodological rigour, more particularly during public demonstrations. After 1993, he participated in some experiments only occasionally.

The year 1992 was therefore fertile in experimental results. J. Benveniste knew that he took a true step forward with "electromagnetic transmission". The arguments with "contamination" as the only explanation of his results did not hold any more, even if at this stage some difficulties persisted during public experiments. These recurring problems were experienced by J. Benveniste and his team as technical obstacles that would be eventually overcome. Moreover, electromagnetic transmission put J. Benveniste's research outside the field of homeopathy. His early works could be considered as a support for homeopathy, but his original scientific contribution should be now recognized.

In the next chapters we will describe experiments performed under the supervision of M. Schiff.

Notes of end of chapter

[1] J. Benveniste. Ma vérité sur la mémoire de l'eau. p. 126.

[2] *Ibid.* p.128.

[3] The results of a series of experiments intended to assess the effect of a magnetic field on high dilutions of histamine were reported in the summary of this communication to a congress: 32.6 ± 4.5% of maximal change of the coronary flow (n=24 experiments; mean ± S.E.M.) before any treatment of high dilution of histamine ("pool 31-41") and 3.7 ± 0.5% (n=20 experiments) after exposition to a magnetic field (50 Hz, 150 oersteds, 15 min) (L. Hadji, B. Arnoux, J. Benveniste. Effect of dilute histamine on coronary flow of guinea-pig isolated heart. Inhibition by a magnetic field. *FASEB Journal* 1991; 5: A1583).

[4] The congresses of the Federation of American Societies for Experimental Biology take place each year in USA.

[5] J. Benveniste. Aspects physique, chimique et biologique des échanges biologiques dans l'eau. Document préparatoire à l'occasion de la réunion du 5-6 mars 93. *[Physical, chemical and biological aspects of the biological exchanges in water. Preparatory document for the meeting of March 5-6, 1993]*

[6] J. Benveniste. Rapport sur la session « cœur-invités » du 13 février 1991 *[Report on the demonstration of February 13th, 1991]*.

[7] Circular letter of J. Benveniste of May 13th, 1991.

[8] J. Benveniste. Aspects physique, chimique et biologique des échanges biologiques dans l'eau. Document préparatoire à l'occasion de la réunion du 5-6 mars 93. *[Physical, chemical and biological aspects of the biological exchanges in water. Preparatory document for the meeting of March 5-6, 1993]*

[9] Ibid.

[10] J. Benveniste. Ma vérité sur la mémoire de l'eau. p. 129.

[11] *Ibid.* p. 130.

[12] *Ibid.* p. 130.

[13] J. Benveniste. Compte-rendu de l'expérience du 16 juin 1992 ; daté du 19 juin 1992. *[Report on the experiment of June 16th, 1992; dated June 19th, 1992]*.

[14] J. Benveniste. Commentaire sur le dépouillement de l'expérience à l'aveugle du 30 juin ; daté du 2 juillet 1992. *[Comment on the analysis of the blind experiment of June 30th; dated July 2nd, 1992]*.

[15] A propos d'une recherche participante sur la mémoire de l'eau, Michel Schiff, octobre 1993. p. 2. *[About a participating reasearch on memory of water, Michel Schiff, October 1993, p. 2]*.

[16] M. Schiff. Un cas de censure dans la science. L'affaire de la mémoire de l'eau, p. 15.

[17] *Ibid.* p.16.

[18] A propos d'une recherche participante sur la mémoire de l'eau, Michel Schiff, octobre 1993. p. 1 *[About a participating reasearch on memory of water, Michel Schiff, October 1993, p. 1]*.