Chapter 11. “It is a pity to see you unable of appreciating the importance of the stake”

Strategic retreat at Clamart

Just after the series of experiments with the laboratory of G. Charpak were finished, J. Benveniste wanted to redo them, but in experimental conditions that he considered to be more favorable. Indeed, he continued to think that the electromagnetic environment of the physics laboratory of G. Charpak played a role in the anomalies. He also suspected possible interferences during the transport of the tubes between Street Vauquelin and Clamart.

This hypothesis of a “jamming” related to the ambient electromagnetic waves led J. Benveniste to get muffs of mild steel, copper and mumetal. He hoped that these screens would protect the tubes containing “informed water” from disturbing electromagnetic influences. Mumetal is indeed an alloy which possesses excellent performances when one wants to isolate a device from electromagnetic environment. At first, the team of J. Benveniste studied the effect of a muff of mumetal on “electromagnetic transmission”. In principle, if the “impregnation” of water actually works via emission of electromagnetic waves, this muff should block or at least strongly decrease the effect of the “electromagnetic transmission”. The experiments were blinded by people outside the laboratory of Clamart. In a half-dozen of experiments, an effect was recorded, but “wild transfers” did not allow a clear conclusion on the efficacy of the metal screen. Indeed, in some experiments, the content of the “protected” tube had an effect on the heart… However, rather than to question the underlying concepts of the experiment, namely the transmission of a “biological activity” via an electromagnetic wave, J. Benveniste suggested technical reasons to explain these unexpected results.

“Around ten successful experiments”

Nevertheless, still resolute to show that electromagnetic transmission was possible, the team of J. Benveniste performed from February to July 1995 numerous blind experiments according to a protocol similar to that followed in the laboratory of G. Charpak. These experiments are presented in a synthetic way Table 11.1 and Figure 11.1. They were blinded by about twenty people who did not belong to the laboratory.[1] On May 21st, 1995, J. Benveniste could announce to the “participants in the experiments of transmission” that 10 experiments just succeeded:

> "The blind experiments are working normally: about ten successful experiments. Here is one experiment (18/5) with remarkable vascular and mechanical effects […]. After two years of efforts, we are back in the same experimental conditions as during the famous experiment, with Georges Charpak and the CSS5 [*Specialized commission*] of INSERM of April 21st, 1993." [2]

The "10 experiments" are the experiments of April 26th and from May 3rd to 19th, 1995 in bold characters in Table 11.1 (more exactly, there were 5 experiments made on 10 hearts of guinea pig).

In the same letter, J. Benveniste then evoked the question of the "wild transfer":

> "During the previous months, we looked for many explanations for the troubles which we know for a long time as soon as tubes are walked after the transfer. We remember the large experiment two years ago at Cochin, with 4 groups of 10 tubes among which one received the transfer. We succeeded for 2 groups and for the 2 others one tube induced cardiac effects, but it was not the right one. According to null hypothesis, no tube should move significantly. If the method is poor and the results are "random", all tubes or numerous tubes move at random, but we cannot explain that one tube out of 10, always the same during repeated checks, becomes active after transfer. At this time, we had suggested errors of coding or even, in the sometimes hysteric atmosphere during the experiments, malevolence. The same *jumps of activity* or *wild transfers* occurred on numerous occasions. The most spectacular was the first (*sic*) experiment) ([…] 10/5/94) in the laboratory of G. Charpak […]. Dozens of previous experiments, open-label or blind, had always given the same result: 1 or 2 tubes giving a "flat" effect around 5% and 1 tube an effect with a characteristic bell effect. This experiment gave the same result except that tube "A" associated with a typical effect was water. Yet the same water as the one that infuses the heart, diluted 1000 times and then approximately 10 times, cannot have any effect, unless it underwent a transmission."

The experiment about which J. Benveniste spoke is described in Figure 11.3.

Date	Active:inactive samples	"Transmitted" active compound	Number of hearts	Unblinding	N° on figure
February 10, 1995	1 : 2	ACh	2	Correct	1
February 21	1 : 3	ACh	1	Correct	2
February 22	1 : 4	ACh	1	Correct	3
March 23	1 : 4	ACh	2	Correct	4
April 19	1 : 4	ACh	2	False	5
April 20	1 : 4	ACh	2	False	6
April 26	**1 : 9**	**Ova**	**2**	**Correct**	**7**
April 28	1 : 7	Ova	2	False	8
May 3	**1 : 4**	**Ova**	**2**	**Correct**	**9**
May 17	**1 : 4**	**Ova**	**2**	**Correct**	**10**
May 18	**1 : 4**	**Ova**	**2**	**Correct**	**11**
May 19	**1 : 4**	**Ova**	**1**	**Correct**	**12**
May 24	1 : 4	Ova	1	False	13
June 2	1 : 4	Ova	2	False	14
June 6	1 : 4	Ova	2	Correct	15
June 8	1 : 4	Ova	2	False	16
June 14	1 : 4	Ova	2	Correct	17
June 15	1 : 4	Ova	2	False	18
June 16	1 : 4	Ova	2	False	19
June 19	1 : 4	Ova	2	False	20
June 21	2 : 3	Ova/ACh	2	Correct	21
June 27	1 : 3	Ova	1	False	22
June 29	1 : 4	ACh	2	False	23
June 30	1 : 5	Ova	2	False	24
July 4	1 : 3	Ova	2	False	25
July 5	1 : 3	Ova	2	False	26
July 11	1 : 4	Ova	1	Correct	27
July 12	1 : 4	Ova	1	False	28

Table 11.1. Experiments of February-July 1995. Out of 28 blind experiments, 13 were positive (one should expect only 6 on average if only chance was a work; $p < 0.05$). The first 4 experiments took place with rats and the following ones with guinea pigs; there were no open-label active controls. The "10 successful experiments" mentioned by J. Benveniste in his letter of May 21st, 1995 are indicated in bold characters. .

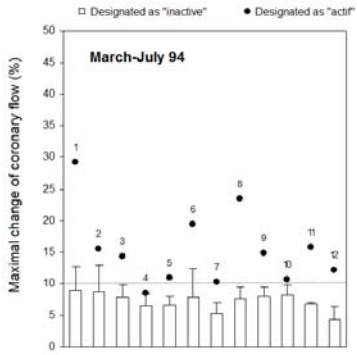

Figure 11.1. These two graphs are intended to summarize and to compare the experiments performed in the laboratory of G. Charpak from March to July 1994 and the experiments performed at Clamart according to the same protocol from February to July 1995. We notice in particular that in the experiments of March-July 1994, the means of the controls were relatively high, close to 10%, making it more difficult the evidence of an effect different from the background noise.
In the experiment 21 of the second graph, two "active" samples were expected.

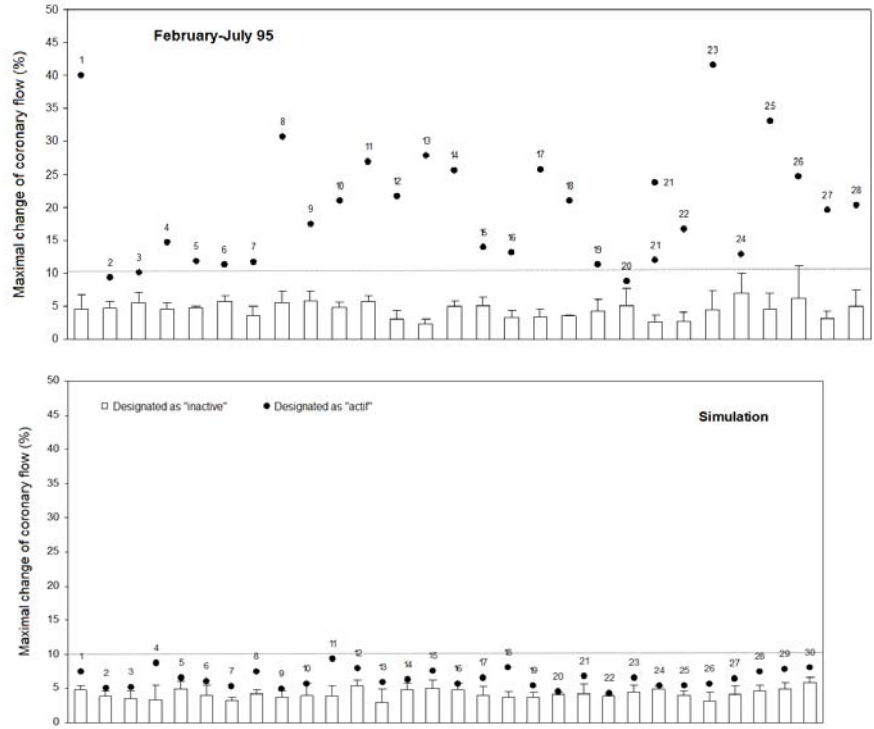

Figure 11.2. In order to explain why the experiments of February-July 1995 are amazing even though the "good tube" was not always correctly designated, a random simulation of this type of experiment has been performed. One could think indeed that, whatever are the results, among the various values obtained in an experiment for the series of tested samples, one of these values is always higher than the others and consequently that the "transmission" results have nothing unexpected and that are simply the consequence of chance.

(To be continued next page.)

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For each of the "experiments" numbered from 1 to 30, 5 random values with a mean equal to 4.6, standard deviation at 1.6 (similar parameters than controls of Figure 11.1 from the results of February-July 1995) and Gaussian distribution were generated. The highest value is named "active" and one calculates the mean of the 4 other values which are then named "inactive". One notices that these points move away of the inactive tubes not so much in comparison with the above figure. One can calculate that they are above 10% only in approximately 0.1% of the cases ($z = (10 - 4.6) / 1.6 = 3.37$). Consequently, the fact that some points "emerged" as they did during the "real" experiments is thus an effect that *must be explained* even if the effect is not where it was expected.

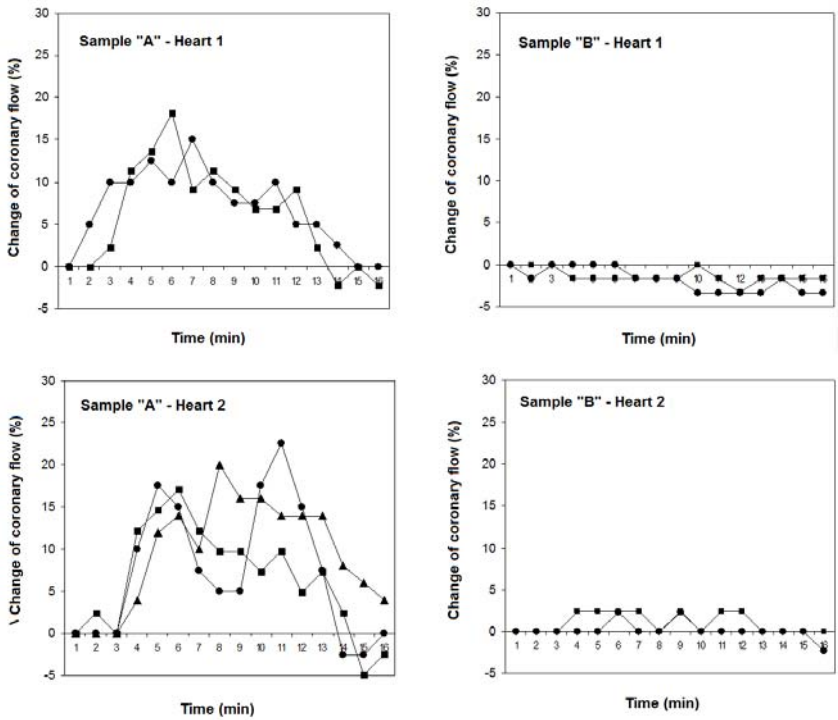

Figure 11.3. Experiment of May 10th, 1994. The transmission of acetylcholine activity was performed in the laboratory of G. Charpak. Samples were tested on two occasions on the device of Langendorff n°1 (Heart 1) and on three occasions on the device of Langendorff n°2 (Heart 2) which worked in parallel. Coherent results were obtained: the sample *A* was "active" whereas the sample *B* was "inactive". Indeed, on both Langendorff devices (heart 1 and heart 2), the sample *A* induced a change of coronary flow during a dozen minutes. Nevertheless, after unblinding, incomprehensibly, *A* was "transmitted" water and *B* was "transmitted" acetylcholine. According to J. Benveniste, it was a typical example of "wild transfer".

This experiment was indeed very demonstrative. It was a typical example of "inversion". The sample *A* supposed to be inactive ("transmitted water") induced large variations of the coronary flow, furthermore on two different hearts. The sample *B*, on the other hand, which should be active ("transmitted acetylcholine") did not induce significant variations. J. Benveniste then gave an example illustrating a possible effect of the environment on the phenomenon of "wild transfer":

> "More recently, after a series of successful transmission experiments performed in the office, relatively dark, of Jacques Testart, the following week we made another series in full light and then transported the tubes in our floor; the tubes had been shaken very close from each other. The result was a magnificent anaphylactic reaction even reproducing the shape of the typical curve obtained with ovalbumin at classical concentration […]. Yet, this active tube was water. It is a typical example of wild transfer. We do not have time to explore all parameters to understand what explains these jumps of information from one tube to another one. The fact remains that this phenomenon, which is perfectly incomprehensible at the moment, is fascinating."

But, he explained, that with the new experimental conditions (the letter was dated May 21st), these "oddities" were not observed anymore. These new experimental conditions consisted in using black cases to protect the tubes from light and to avoid moving tubes too close to one another:

> "Two coders randomly draw 5 tubes of water among 20 tubes (or more) which were previously numbered and then 2 among 5. The 3 tubes that will not undergo transfer (*naive water*) are immediately placed, each in a black case, at distance from each other on a rack in the same room. A tube undergoes *water* transfer, the other one *ovalbumin* transfer and the 2 tubes are placed on the rack, each in a black case. The coders keep the code. The heart operator comes to get, one by one or two by two, the tubes which are never mobilized or shaken together."

He also insisted on the specificity of the transferred biological activity because atropine, an antagonist of acetylcholine blocked the effect of "transmitted" acetylcholine:

> "However, since we did the experiments in the conditions described above, we did not observe such oddities anymore. On May 17th, Jacques Testart being the coder, the blind tube Ova TR

> gave after 4 measurements, 21% of change on average (1 ml on 5 ml) versus 4.9% for water after 16 measurements (0.1 ml on 5 ml). Moreover, on the same day, in an open-label experiment, atropine at classical concentration totally inhibited acétylcholine (ACh) - 7 M, as expected, *but also ACh TR* (ACh TR without atropine: 56.4%; with: 7%). We have approximately ten experiments of inhibition of ACh TR by atropine, which sign the specificity of the transmitted signal."

The experiments of May 17th described by J. Benveniste thus suggested that the new precautions allowed the realization of blind experiments. At the same time, this experiment illustrated the specificity of the transmitted signal because atropine – a "poison" of acetylcholine – inhibited not only the effect of "classic" acétylcholine but also "transmitted" acetylcholine. Here again, these spectacular results appear to confirm that "acetylcholine information" which was "imprinted" into water had the same pharmacological characteristics as "molecular" acetylcholine. Finally, J. Benveniste explained where, according to him, the experimental problems originated:

> "In fact the necessity to transport the tubes introduced, we now know, an experimental bias which explains the irregularity of the results obtained outside. Georges Charpak concludes that our results outside are unpredictable *and* that we cheat at home, what, respectively, does not stand up to an examination of the facts and is a slander. It is simply unusual experimental conditions that have been met with a still embryonic system and all physical bases of which we do not understand."

To avoid any transport of tubes, he logically suggests performing the entire experiment in G. Charpak's laboratory!:

> " […] we have three operational devices and we are ready, if it is absolutely necessary to do the experiments elsewhere, to place an isolated heart device in another laboratory, for example by Georges Charpak. We will install the heart and will let the local staff operates for the transfer and for the injection of the solutions to the heart."

And, being perked up by the last "ten experiments" which correctly identified the active sample, he lyrically ended and not without bombast:

> "[*Your help*] will be furthermore, I believe it, recognized by History because, without neither true nor false modesty, what we do together at this moment could indeed be History."

If this explanation of "wild transfer" from tube to tube which would be facilitated by light waves could be possibly retained for some experiments, it did not take into account other observations. More particularly, this hypothesis does not explain why "wild transfers" are exceptionally observed with open-label samples.

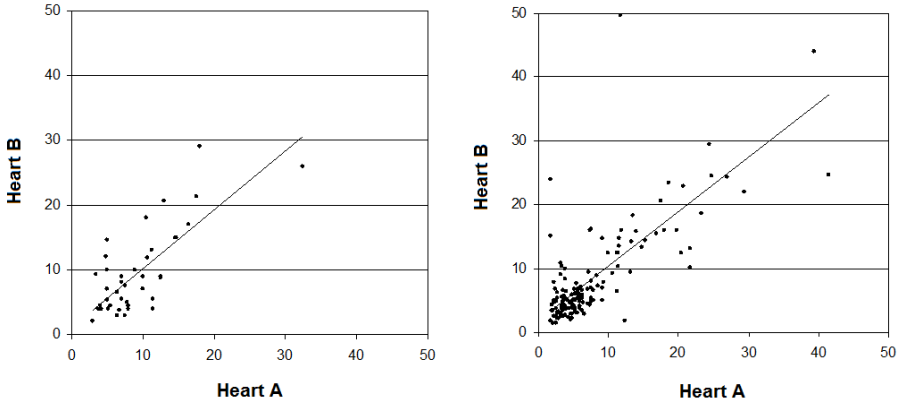

Figure 11.4. Correlations between both devices of Langendorff. These figures illustrate the internal coherence of the experiments performed during the collaboration with the laboratory of G. Charpak (left figure) and the blind experiments of February-July 1995 (right figure). Without taking into account the "success" or not of the experience according to the fitting with the blind code, one notices that the results obtained for the same sample on a device of Langendorff (heart *A*) are correlated with those obtained on the other device of Langendorff (heart *B*) which worked in parallel. This correlation indicates that "something" occurred that could not be reduced to random only.
NB. All the experiments done with Charpak laboratory or those of February-July 1995 were not systematically measured on both devices of Langendorff.

The series of "10 successful experiments" was however only an island of success within the usual "failures". Nevertheless, the background noise which was high during the experiments with G. Charpak decreased. This background noise was indeed closer to 5% than to 10% for "inactive" samples. But in spite of these better experimental conditions, the experiments that followed the "10 successful experiments" contained numerous "wild transfers" once again. Nevertheless, and it is a important point on which it is necessary to insist once again, when samples were assessed on the two Langendorff devices working in parallel, the results were correlated (Figure 11.4). We are thus always in a configuration of "coherent discordance".

"A *crazyness without limit*"

With a slight delay, the letter with the "10 positive experiments" triggered a reaction from G. Charpak and C. Hennion. These latter, summarizing the arguments of their last letter of December, wrote to J. Benveniste:

> "We have your letter in our hands dated May 21st, 1995, in which you announce us the success of about ten blind experiments similar to those that one of us – Georges Charpak – could see in your laboratory in 1993.
>
> It seemed very likely to us that the operation of transport of the properties of an encapsulated chemical towards pure water by virtue of an amplifier which oscillates permanently was either an artefact, or trickery. Because of your titles, of your position in an important scientific community, and in front of your enthusiasm and your good faith, we thought of doing it as a service to you by suggesting that you do the operations of transfer at the *Ecole de Physique et de Chimie* under the supervision of one of us, Claude Hennion.
>
> All in all, 20 experiments[3] clearly showed a totally random effect. The table below summarizes the observations made with you." [4]

The authors of the letter presented a table of synthesis (similar to Table 10.1 of Chapter 10) and then they came back one the question of the fraud which obviously concerned them:

> "During a control made by you, where you knew the result beforehand, you had observed that when the result was known, you or your co-workers found the right answer. You then wondered if you were not betrayed in your laboratory.
>
> You rejected this hypothesis, but you invented reasons that explained why the experiment did not work at the *Ecole de Physique et de Chimie* in a obvious headlong rush, where you took into account only the experiments confirming your hypothesis.
>
> It is interesting to also note that you gave credence to publications which are in your favor and that you use the most outlandish reasons to explain the failures. […]
>
> You also gave us the texts of an Italian theorist, a professor of university. We gave his text to be analyzed to the best French theorists. They said that it was stuffed with clumsy false hypotheses. But as it is written in an opaque language for 99% of the physicists, we understand that he can deceive you by his friendly encouragements."

And, on the same day, G. Charpak wrote to P. Lazar that J. Benveniste was affected by "a craziness without limit". [5] The wish of J. Benveniste of wanting to repeat the entire experiments (transmission, blinding, test of samples, unblinding) in the laboratory of G. Charpak did not thus find an echo in Street Vauquelin:

> "I no longer believe it, explains Claude Hennion. He did the trick fifteen times, I did not want to try a sixteenth. I had invested a lot of time, including at home. As long as he was not controlled, it worked (...). Georges Charpak never believed it possible. He was curious. But one does not have the right to let oneself be fooled." [6]

The collaboration between the two laboratories stops right then. In front of the refusal to collaborate in new experiments, J. Benveniste confirmed the break by answering to G. Charpak:

> "My feelings towards you are in fact rather close to pity. I have on my desk a floppy disk of a computer containing, for the first time in the history of mankind, a biological activity. It is indeed pity to see you unable to appreciate the importance of the stake." [7]

In the next chapter we will see to what J. Benveniste alluded about this floppy disk, supposedly nothing but an important milestone in "the history of mankind". Before that, let us examine what makes the protagonists' viewpoints irreconcilable.

The two faces of Janus

In terms of formal logic, we must admit that G. Charpak was right: a hypothesis was formulated and was apparently refuted by the experiment. One thus had to reject it. Yes, but what was the hypothesis? Although it remained implicit, we could formulate it in the following terms: some device allowed transmitting a "biological activity" to water which was then capable of making a biological system react.

The hypothesis having been "falsified" with this logic, was it indispensable to reject it as a whole and move on to other activities? It is indeed necessary to recognize that one was a little reductive when one designed the protocol of the experiment intended to test this hypothesis. One went from the proposal "transmission of a biological activity" to the proposal "if it is true, then one must be able to discriminate more often than chance the supposed active samples among other supposed inactive samples". In the conditions of the experiments done with G. Charpak, the "divination" was not better than

chance. Nevertheless, if one had knowledge of the experiment in general, one could not be satisfied by this conclusion. Indeed, the biological system reacted differently and in a coherent way – it was particularly striking for the measurements with the two parallel Langendorff devices – while it should not have reacted! (See Figure 11.5). One must also admit that the experimental conditions were not very satisfactory. The reactivity of hearts – whatever the reason might be – remained low and the biological effect emerged with difficulties due to the background noise. Nevertheless, even in the experiments with acceptable quality, "wild transfer" was present.

However the observers of these experiments were as the god Janus who has two faces: one of the faces had eyes fixed on the blinding whereas the other face observed the experimental system. The absence of communication between both faces was a source of mutual incomprehension. Indeed G. Charpak tested the hypothesis without worrying about what the other face saw. He considered the experiments of J. Benveniste as a black box under the responsibility of the latter. And if for one of the faces chance indeed seemed to prevail in these experiments, the other face could notice that a modification of the coronary flow had occured. This change of a parameter of the biological system was not trivial and was not a simple artifact of handling. In fact – and J. Benveniste had the greatest difficulties to get this point understood – what was surprising was not to guess correctly which ones were the active samples. The surprising fact was that "something had moved" and had moved in a coherent way, in particular when two hearts worked in parallel and gave correlated results.

One could nevertheless suggest reformulating the hypothesis but it would be probably expensive in supplementary hypotheses, probably more than the "simple" hypothesis about a structuration of water. For diverse reasons, J. Benveniste preferred to try "to improve" the experimental conditions and the "reproducibility".

Making science takes time

Even though G. Charpak was right to assert that "the experiments clearly showed a totally random effect", he did not try however to be "in sympathy" – it is a euphemism – with J. Benveniste and his experiments. Consequently, he did not try to know what noticed the other "face" or to listen it. Yet every researcher knows that a minimum of benevolence and empathy is needed towards the object under scrutiny. This is all the more true as the discoveries – it is almost a definition – are usually done on the edge of the performances of the technical means of the moment. And J. Benveniste was not wrong when he

asserted that G. Charpak would not have bet on the future of aviation if he had attended the debut of the *Antoinette* of the French aviator Blériot.

If G. Charpak (or one of his collaborators) participated in the life of the laboratory, from the sacrifice of the animal and the removal of its heart, if it attended the injection of the various samples in the system of Langendorff, noticing with excitement that actually some samples gave an answer, then probably his attitude would be different. He would then wonder why some samples had an effect. And his perplexity would be great when he would experience the discordances after unblinding while everything seemed coherent just a moment before. All those who had this approach, even though they were skeptical at first, "got into" (as M. Schiff, for example) or have – at least – suspended their judgment. But making science takes time.

The will to do all experiments Street Vauquelin as proposed then by J. Benveniste was coherent with this approach. The purpose was to take into account the entire experiment and not to focus only on the bet: "If it is true then…" Indeed – and it is the idea which is supported here – what J. Benveniste asserted is "true", but only up to a certain limit. All the difficulty is to highlight the crossing point between "it works" and "it is no longer working". But for that, it is necessary to take the experiment in its entirety (with the eyes of both faces) and not to be satisfied to play the role of a bailiff. Moving forward in the understanding of the experiment requires to realize what is the amazing fact, namely a biological system that reacts differently – repetitively and consistently – to a sample n°1 and to a sample n°2 although these samples are, in the current state of the knowledge, identical because they come from the same bottle.

The working hypothesis of J. Benveniste was thus maybe erroneous or badly formulated. It did not explain however what was daily observed in the laboratory of Clamart. But how to explain these rather subtle arguments when at the same time some people asserted that: "as long as he was not controlled, it worked", as did C. Hennion to the journalist E. Fottorino.[8] This sentence obviously suggested either fraud or incompetence. Since all those who were supposed to control these experiments did it at a distance (without trying to merge the observations of both faces), J. Benveniste could not make them put the finger on the problem that literally undermined him. It was this blind spot in the eyes of those who were supposed to oversee his experiments that logically led G. Charpak to conclude on the "craziness without limit" of J. Benveniste.

Notes of end of chapter

[1] The most important help came from J.C. Salomon, F. Russo-Marie and J. Testart.

[2] Letter of J. Benveniste "to the participants in the transmission experiments of May 21st, 1995".

[3] Overall, there were in fact only 18 experiments; moreover, this is this number that is reported in the summary table included in the letter of G. Charpak and C. Hennion.

[4] Letter of G. Charpak and G. Hennion to J. Benveniste of July 18th, 1995.

[5] E. Fottorino. La mémoire de l'eau. Le temps des passions. *Le Monde*, January 22nd, 1997.

[6] Ibid.

[7] Ibid.

[8] Ibid.