

Chapter 10. "If it's true, it is the biggest discovery since Newton"

"The head on the block"

In its report after its visit of April 21st, 1993, the Commission of Inserm called for a collaboration for the respective laboratories of J. Benveniste and G. Charpak. The latter had however canceled an appointment during the demonstration performed a short time later, on May 13th, and had delegated two of his collaborators. Furthermore, one remembers that a certain tension was born between both laboratories with correspondences where fraud had been evoked. This future collaboration which had been suggested by the Specialized commission in the fervour of the moment seemed to weigh more and more upon G. Charpak and one year passed before the first experiments took place. Besides, contributing to the irritation of G. Charpak, J. Benveniste did not hesitate to repeatedly quote that the latter had said during a phone conversation:

"If all of this is true, it is the biggest discovery since Newton".

He adds even during the same conversation that it would be necessary "to rename Quay Anatole-France [*where the National Center for Scientific Research sits*] as Quay Benveniste" ".¹

The journalist F. Nouchi who stayed in close contact with J. Benveniste echoed these words in *Le Monde* at the end of 1993.² The journalist then wrote that the results of J. Benveniste were:

"A mystery about which a Nobel prize laureate would have said during a private conversation that "if it were true, it would be the most important discovery since Newton." "³

Although his words were anonymous, it was not difficult to recognize G. Charpak behind this "Nobel prize laureate" and he wrote to F. Nouchi and J. Benveniste to replace the conversation in its context so that his words would not be interpreted as an endorsement of the studies on the "electromagnetic transmission". G. Charpak insisted to specify to J. Benveniste about this article in *Le Monde*:

"An article suggests that I am certain that you are not a victim of an experimental artefact. This is not the case and I do not wish for everyone to believe I support the experiments on the memory of water".⁴

In his letter, the Nobel prize laureate also indicated to J. Benveniste the conditions of their future possible collaboration. In particular, he considered

that J. Benveniste had “an erroneous vision of what must be verification of a scientific fact”. He explained that he would collaborate if J. Benveniste granted to make the experiments in conditions of control “satisfactory for a physicist”, that is – always according to G. Charpak – by putting “the head on the block”. We must admit that this was the beginning of a scientific collaboration in poor conditions if one of the protagonists spontaneously proposes himself for the role of the executioner! Therefore, after his “encouraging” sentence for the success of the experiment during his visit at Clamart (“You’d better, otherwise you are dead”)⁵, G. Charpak persisted, thus revealing a rather bloody conception of scientific evaluation!

J. Benveniste answered to G. Charpak, first of all for the article of F. Nouchi and the reference to Newton:

“The article of the latter seemed clear because he reported one of your words without omitting the conditional: “if it *were* true, it would be”. It was not question of making you endorse these results, but rather to be surprised that, even it has a chance of one out of one thousand to be true, the scientific “community” is missing “the biggest discovery since Newton”. I take this opportunity to tell you directly my regret that our collaboration is not closer. In spite of your independence, you probably are as me under the pressure from the scientists propped up on their certainties. I had hoped that after your visit a more confident, close, steady collaboration would be established between the ESPCI and my laboratory.”⁶

He returned then on the question of an experimental error or a possible artefact:

“I remind you that, during your coming with the delegation of INSERM on April 21st, 1993, no methodological criticism or hypothesis of artefact were emitted by this group on the scientific level and the experience of which one cannot dispute. One cannot thus allude to an artefact, as upon a litany, without proposing credible and experimentally verifiable suggestions. Yet, till date, none resisted the most superficial examination. In particular, I received nothing from you and what I received from De Gennes once again illustrated the fragility of the intelligence in front of dogmatism. If all the French Nobel prize-winners and in addition Baruj Benacerraf⁷ did not propose an artifact up to now, can one continue to speak about it, except for separating word and thought? In the absence of this mythical artefact, the immense

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majority of the scientists, including you, refuse in reality to consider these results in the name of: "it is impossible thus it is not" ”

J. Benveniste reminded then to G. Charpak – who consulted colleagues about the theory of G. Preparata and of E. Del Giudice – what is sometimes the value of the opinion of “experts”:

“I do not understand your acceptance without discussion of the judgment of *one* French theoretical physicist. The weakness of French theoretical physics throughout the multiple paradigmatic revolutions of the century is a historic fact. From relativity to quantum physics, everything has always been denied by the “experts”. A theoretical advance, which would allow to shed some light on the structure of condensed matter and which has already demonstrated its power by the assessment of well-established physical constants, cannot be dismissed out of hand. How about organizing a seminar on this subject?”

He reminded also the experiment of April 21st, 1993 in which participated G. Charpak by coding samples:

“Before the opening of the code, we designated the tube A, which induced a reaction similar to authentic ovalbumin, as being "transmitted" ovalbumin, what it was. Where can the error be? Afterwards, you asked to redo the experiment in your laboratory, without my presence and "of every person having shaken hands with Benveniste within three months". It was insulting, but I accepted this because the cowardice which prevails among my peers, in particular biologists, leaves me alone – with the only help of Alfred Spira – in front of this choice. Maybe I have an “erroneous vision of what must be the verification of a scientific fact” and, in this case, I would be very happy if you show me what it is, but I would be surprised that you would agree to see your experiments undergoing this kind of checking.”

He continued on the methods and conditions of collaboration between both laboratories:

“Do you really think that "satisfactory conditions of control for a physicist" consist in putting "[my] head on the block"? I did not know that the world of physics was so barbaric... In fact, a verification according to the usual methods would be, as you had proposed, that one of your collaborators comes one or two days a week during one or two months to work in our laboratory

including, after some developments, in our absence, if that can reassure you.”

And he ended by expressing his disappointment in front of what he judged to be a lack of open-mindedness among scientists:

“In conclusion, I am happy that you still wish to collaborate with us. Certainly I am disappointed that this collaboration is taking place according to unusual scientific rules, on the mode of the "Russian roulette". This atmosphere clearly reflects the one who prevails within French scientific "community" – and not only for my affair – and contrasts with the open-mindedness which you were the only one up to now to express among the great French scientists. However, I have decided to do this experiment with you, as soon as the intensity and the regularity of the responses of the hearts will be as they were during last spring”.

J. Benveniste and G. Charpak nevertheless succeeded in agreeing on the technical and experimental conditions. It was decided that the transmission experiments would take place at ESPCI (*Ecole Supérieure de Physique et Chimie Industrielle de la Ville de Paris*) located Street Vauquelin in the 5th arrondissement of Paris and that samples would be then transported to Clamart where they would be tested.

An oppressive atmosphere

As indicated by J. Benveniste, the period itself was rather unfavorable for these demonstrations because hearts reacted weakly to stimuli for poorly understood reasons. The preparatory experiments consequently took time. Thus, a first experiment was performed on March 7th, but was canceled due to technical problems at the time of the measurements. The second experiment took place only on March 30th. Furthermore, as indicated by J. Benveniste:

“The atmosphere which reigned during this phase of preparation and then during the experiments is extremely painful. The collaborators of Charpak show honesty and benevolence towards us, but the Nobel prize laureat still behaves with a contemptuous attitude. To such a point that at no time I had the opportunity to sit down in his company to discuss the protocol or obtain enlightenments on some questions of physics.”⁸

Moreover, the absence of M. Schiff was felt in the methodological organization of the experiments. Thus, results with open-label samples performed in the same conditions as blind samples were only rarely reported.

Yet, these controls would have allowed validating (or not) the experiments before unblinding. In other words, all experiments were taken into account for the analysis even though a simple quality control would have rejected a large number of them. But having promised a lot, J. Benveniste is condemned to a faultiness round.

“Wild transfers” occurred (or at least errors of allocation in the codes were interpreted as such). At the beginning, J. Benveniste incriminated the commercial physiological salt solution and the hearts that poorly reacted. Finally, he suggested that the intensity of the electromagnetic background was higher in the laboratory of physics of G. Charpak than in the laboratory of Clamart:

“To explain the errors that appear during the unblinding, Doctor Benveniste suggests two phenomena: the hearts of guinea pig would not be very sensitive (the reactions of these animals vary according to the seasons); the radiations blur the data during the transport in car between Street Vauquelin and Clamart. To prevent it, the researcher locks tubes inside big tinsplate boxes. He wraps them in aluminum foil and then tries again other armoring methods (mild steel, copper and finally mu-metal, an alloy intended to block magnetic fields). "I let him establish his protocol and validate it. But it still did not work", Claude Hennion regrets this.”⁹

A paranoid ambiance then developed within the laboratory of Clamart. J. Benveniste who did not succeed in understanding the origin of the “wild transfers” wondered if somebody did not play with him in the laboratory. A scenario similar to the one which had ended with the dismissal of L. Hadji in 1991 was being set up. J. Benveniste even announced his suspicions concerning his own co-workers to C. Hennion. Samples were tested to Clamart in an atmosphere often heavy and suspicious. I. Béhar – a retired engineer and entrepreneur who spent several months in the laboratory of J. Benveniste at this time to participate in this research – testified about this “atmosphere of generalized suspicion which reigned there”. He also confirmed the felling of headlong rush:

“During all the period of the Charpak experiments, Benveniste was effectively obsessed by the problem of water [...] and he made trials everyday with new water by changing the details of the experimental protocol also very often.”¹⁰

Nevertheless, Street Vauquelin, C. Hennion was patient and did everything he could so that the experiments were performed at their best. The relations of

J. Benveniste with G. Charpak became evermore tense. Contrary to his commitments of April 1993 during the visit of the laboratory in Clamart, the physicist appeared to take some distance towards these experiments, letting his co-workers manage them, J. Benveniste told:

“Charpak rarely attended the operations of transmission. During one of the rare occasions where the Nobel prize laureate is present, a statistician, Director of research at Inserm, is also at the premises. I do not know this researcher and had with him only a brief phone contact. It is he who has to perform the operation of coding of tubes. Probably suspecting that the statistician could be in cahoots with me, Charpak intercepts a secretary who passes in the corridor and he made her redo the coding. Another source of confusion.”¹¹

The experiments continued nevertheless. The laboratory of Clamart appeared entering into a suicidal enterprise. When, in spite of the poor experimental conditions, a forecast was tempted on flimsy results, it was naturally mostly a failure:

“Benveniste took the blow silently, observes Claude Hennion. But when he was right, he was like a visionary. His behavior was not scientific any more.”¹²

The last experiment was performed on late July 1994.

Disappointing experimental conditions and results

The analysis of the results of the experiments performed with the laboratory of G. Charpak is a clear proof of the poor reactivity of the rodent hearts. The samples which were designated as “active” hardly induced changes of coronary flow: 15% on average. We have seen that a change of 10% of the basal flow was the limit which had been empirically defined to discriminate between “active” and “inactive” samples. The experimental conditions were thus mediocre because the intensity of the signal was near the background noise.

After reading Table 10.2 which summarizes these experiments, it is striking to notice that many experiments did not succeed for technical reasons; moreover, experiments without the usual open-label controls were numerous. We are far from the rigour and from the quality control which prevailed for example during the experiments organized by M. Schiff.

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Date	Active:inactive samples	"Transmitted" active compound	Number of hearts	Open-label actives samples	Unblinding	N° on figure*
March 7, 1994	1 : 3	Ova	2	Not done	No results	-
March 30 and 30 bis	2 : 5	Ova + tet. vac.	3	Not done	False (<i>in fact uninterpretable</i>)	-
April 21	1 : 4	Ova	2	18-15%	Correct	1
April 21	1 : 2	ACh	Not tested	-	No results	-
May 10	1 : 2	ACh	3	Not done	False	2
May 11	1 : 2	ACh	3	Not done	Correct	3
May 13	1 : 2	ACh	3	Not done	No conclusion	4
May 17	1 : 4	Ova	3	Not done	False	5
May 18	1 : 4	Ova	2	Not done	False	6
June 1 ^{er}	1 : 4	Ova	4	18-14-8-11%	False	7
June 3	1 : 4	Ova	Not tested	-	No result	-
June 6	1 : 4	Ova	2	25-40%	False	8
June 8	1 : 4	Ova	2	13-21%	False	9
July 7	1 : 4	Ova	Not tested	-	No result	-
July 13	2 : 4	Ova	1	12%	1 correct sample	10
July 13 bis	1 : 2	ACh	1	15%	False	11
July 22	1 : 4	Ova	2	Not done	False	12

Tableau 10.1. Summary of the transmission experiments performed in the laboratory of G. Charpak. Among 18 experiments, 13 were considered as exploitable (but with 10 of them the correct code was not found, 2 fitted the code and 1 was intermediate). If we make a selection by defining quality criteria before taking into account the results, only the experiment of June 6th is selected. Unfortunately, the sample which "emerged" in an obvious manner in this experiment was not the correct one (this experience is detailed in Table 10.2).

Tet. vac.: tetanus vaccine; * Figure 11.2 of Chapter 11.

With better experimental conditions, could better results have been obtained? Nothing is less certain. First of all, if we proceed to a selection of the experiments according to quality criteria, a unique experiment of the series combines enough criteria: open-label samples with correct results and change of the coronary flow of 20% or more (experiment of June 6th; Table 10.1). But, even though a biological signal was recorded, thus suggesting that a "transmission" indeed occurred, the biological activity was not where it was supposed to be. It was a typical case of "coherent discordance" with results correlated on both Langendorff's devices which worked in parallel (Table 10.2).

Tested samples	Number of measurements	Maximal changes of coronary flow (%)	Biological activities in increasing order	Unblinding
<i>Blind tests</i>				
G	2	6.0 ± 1.4	1	Water
F	2	7.5 ± 3.5	2	Ova tr.
M	2	9.5 ± 0.7	3	Water tr.
B	2	23.5 ± 7.8	4	Water
<i>Open-label tests</i>				
Water tr.	2	4.5 ± 0.7	-	-
Ova tr.	4	32.0 ± 26.5	-	-
Ova 0.1 µmol/L	2	45.0 ± 21.2	-	-

Tableau 10.2. Transmission experiment of June 6th, 1994 performed in the laboratory of G. Charpak. This experiment was one of the rare of the series for which open-label controls were realized and allowed validating the experiment. Both Langendorff devices which worked in parallel gave correlated results. Unfortunately, after unblinding, the most active sample was “naive” water. There was no effect for transmitted ovalbumin which should have modified the coronary flow.

“You practice a headlong rush which will cut you definitively from scientific circles”

Not long after the end of the experiments, G. Charpak wrote to J. Benveniste:

“I consider it necessary to make an assessment of the experiments which you made to the *Ecole Supérieure de Physique et de Chimie*.

It is clear that the results which you obtained are compatible with those for which one could expect with an effect simply due to chance.

In front of negative results, you searched for explanations in interference effects. Apparently, you never wondered if your previous observations were not vitiated by error.

I understood that you wondered, a few months ago, if in your entourage, one of your collaborators did not bias the results systematically because, when these were predictable they were generally confirmed by the experiments.

You seem to have pushed aside this hypothesis, which appeared as the most plausible to me.

Why did it seem to me plausible, you might ask me? Because your experiments challenge the elementary laws of the physics and those of simple common sense.

It is not reasonable to imagine that your amplifier, which is in a state of permanent oscillation, transmits to water electromagnetic

signals that structure this water. The environment in which you are immersed in the laboratory is full of radiations of all wavelengths, having an amplitude whose the value is higher than those of the waves you claim to transmit, by vertiginous factors.

It was obvious from the beginning, but I wanted to give you a possibility of correcting a mistake."¹³

G. Charpak pursued:

"Many famous scientists met artifacts which sometimes excited them because they thought of having fired of the big game. They knew how to, generally, move back in time thanks to a poorly exciting virtue which is a critical mind towards oneself. I really believed that you were manipulated by an unscrupulous circle of acquaintances which found there an interest and that if you could see that your observations were not reproducible magic, you could save your reputation.

You have to your credit, according to your peers, good works in biology and the simple recognition of an error would have been put to your credit.

But you practice a headlong rush which will lead you only to cut you definitively from scientific circles. [...]

When cold fusion was announced, dozens of experiences, each more false than the last, confirmed the first observations. I know that some artists as far as extortion of subsidies is concerned continue to become agitated in this domain because one finds an incredible quantity of gullible people even in high positions and I am not surprised that one of these artists gave you the illusion that very learned theories were compatible with your experiments.

I got their articles examined by theoretical physicists of the most eminent. They found them absurd." [...]

And G. Charpak ended his letter by distancing himself:

"There is no interest to give the illusion that you undertake rigorous experiments at the *Ecole Supérieure de Physique et de Chimie*. I thus ask you to never mention any collaboration with my team in which I think that Mister Claude Hennion gave a perfect example of patience and rigor".

After one month, J. Benveniste answered to G. Charpak by a long letter:

"I waited a few weeks before answering your letter so that it is not influenced by the sadness which I felt after its reading. I could detail the reasons of this sadness, the two main reasons being your

contemptuous tone and the distance between your arguments and the scientific stake. But I do not want to be involved in a controversy with you and I prefer to answer you on the content. [...]

You have once again mentioned "fraud" and indicate that I considered such a hypothesis myself and that I ruled it out. [...]

It would be a fraud because our "experiments challenge the elementary laws of physics and those of simple common sense." Dare I remind you that the "simple common sense", before the development of the theories and the relevant scientific observations, had led to admit that the sun turns around the earth, that the X-rays, the heaviest than the air, the recorded voice, the laser, etc., were hoaxes [...] The same "common sense" authorized the most eminent "theorists" to deny the existence of bacteria, that the light can be described at the same time in term of corpuscles confined in a volume and of waves propagating infinitely, that matter is energy, that moving closer two pieces of metal could kill thousands of people in a few seconds? [...]

Is development of the sciences not more often made by bringing answers to the contradictions with the prevailing scientific laws than by subscribing to "common sense" and other "elementary laws"? [...]"

And once again he reminded G. Charpak about the experiment this latter attended at Clamart:

"I also remind you the conditions of the experiment in which you participated on April 21st, 1993; you performed, locked into a room with the delegation of INSERM, the transfers of ovalbumin and endotoxin on two tubes of water chosen among twenty identical tubes. We had told you in advance that the one and/or other one of these transferred tubes could work, according to the state of immunization of animals. You blinded four tubes among which two were control tubes (it is necessary to remind that all samples were water which had never left its tube). We then measured the effect of these blind tubes and on four hearts we constantly found an activity for the tube A, an activity that was strictly proportional to the one obtained with ovalbumin at -7 M [$0.1 \mu\text{mol/L}$]. After unblinding, tube A was Ova-TR [*transmitted ovalbumin*]. The results were in the order 13, 15, 32, 93% of change of coronary flow for Ova-TR (that is once again water) and respectively 9, 12.5, 45 and 100% for Ova -7 M. Where can be the

cheating, the fraud which you then evoked and that you repeat in your letter? [...]

I would like to believe that your letter was written in a fit of anger and maybe under the blow of exterior events. I hope with all the involved researchers and technicians that, after examination of the facts summarized above, you will be willing to maintain the collaboration which we think is essential, because only a multidisciplinary research will allow to make progress in the understanding of the phenomenon.”¹⁴

The experiments with G. Charpak were a unique opportunity for J. Benveniste. Although he is not a biologist, G. Charpak with his aura of Nobel prize laureate would have been a considerable support if he had been the slightest bit convinced. Without going so far, the neutrality of the physicist in the “debate” would have been preferable in the situation which now prevailed. Indeed, J. Benveniste has now made a new “enemy” in the person of the physicist. Before being committed in the “affair”, G. Charpak did not certainly “believe” in it, but it was for theoretical reasons and due to a matter of principle. After the failure of the experiments performed in G. Charpak’s laboratory, J. Benveniste thus burned out invaluable ammunition. He could certainly put forward poor experimental conditions, but what would be retained was that “the experiments with Charpak did not work”. Moreover, G. Charpak had now concrete reasons for “not believing” in these experiments. He did not hesitate to let it be known by all the authority that conferred him his status and he was furthermore helped by his popularity in the media.

Notes of end of chapter

¹ E. Fottorino. La mémoire de l'eau. Le temps des passions. *Le Monde*, January 22nd, 1997.

² The article in *Le Monde* had been written on the occasion of the publication of the article of Hirst *et al* published in December 1993 which, one remembers, tried to reproduce the results of the article in *Nature* of 1988 (see first part).

³ F. Nouchi. Une équipe de chercheurs anglais n'a pu reproduire les travaux du docteur Benveniste sur la « mémoire de l'eau ». *Le Monde*, December 11th, 1993.

⁴ Letter of G. Charpak to J. Benveniste of January 18, 1994.

⁵ Cf. Chapter 6.

⁶ Letter of J. Benveniste to G. Charpak of January 24th, 1994.

⁷ Immunologist, Nobel prize laureate in 1980.

⁸ J. Benveniste. Ma vérité sur la mémoire de l'eau, p. 158.

⁹ E. Fottorino. La mémoire de l'eau. Le temps des passions. *Le Monde*, January 22nd, 1997.

¹⁰ I. Béhar. Distinguer l'homme du résultat scientifique. *Le Monde*, February 8th, 1997.

¹¹ J. Benveniste. Ma vérité sur la mémoire de l'eau, p. 159.

¹² E. Fottorino. La mémoire de l'eau. Le temps des passions. *Le Monde*, January 22nd, 1997.

¹³ Letter of G. Charpak to J. Benveniste of December 5th, 1994.

¹⁴ Letter of J. Benveniste to G. Charpak of January 9th, 1995.