

Chapter 6. "You'd better ... otherwise you are dead"

A lifeline?

As we have seen, a simple method had been found by Benveniste's team to "decontaminate" water: heating the samples of before "imprinting" them. Indeed, this simple process eraser any "electromagnetic memory". Demonstrations on the reality of the "electromagnetic transmissions" were thus again possible without being anxious about possible "contaminated" samples.

The opportunity of a demonstration in front of an elite audience – namely, a commission of Inserm – occurred in the spring of 1993. Indeed, at Inserm, every spring saw the return of the four-year evaluations during which the "production" of a quarter of the teams was closely examined by the scientific commissions. In 1993, it was the turn of the laboratory of J. Benveniste to be in the hot seat.

We saw in the first part how hard the task of the examiners had been in 1989 when they assessed the scientific production of the laboratory of J. Benveniste. Indeed, the examiners had been pulled between the "exotic" experiments on high dilutions and the good general level of the laboratory concerning "classic" research, not to mention diverse pressures and extra scientific considerations that had affected this evaluation performed soon after the "*Naturegate*".

However, the evaluation of year 1993 had an important particularity. The Unit 200 of Inserm then reached the limit of twelve years and the rule at Inserm is to close the laboratory at this age.¹ Nothing prevented however the reopening of the laboratory under a new title with the same staff and a new director chosen among the researchers. The purpose of this twelve-year practice, which had been established by P. Lazar, was to challenge the teams periodically. But, the creation of a new research unit required a sufficient number of researchers present on the organigram of the new laboratory. However, given the sulfurous reputation of J. Benveniste during the last years, most of the researchers of the laboratory migrated in less agitated areas. The administration of Inserm moreover made nothing to limit these transfers when it did not facilitate them. The technician staff followed suit. As for young researchers recently recruited by Inserm, a not written law dissuaded them from asking for a permanent position in the laboratory of J. Benveniste.

Not having the possibility to ask for the creation of a plain research unit, J. Benveniste thus made a demand of "junior-laboratory contract". More exactly, it was a researcher of the laboratory, Yolène Thomas, who did the

demand. In the official jargon of Inserm, a junior-laboratory contract is a laboratory in formation, a structure which can precede the creation of a plain Inserm unit when other researchers will join it. If Inserm granted the creation of this junior-laboratory contract, J. Benveniste could maintain what remained from the laboratory and thus could keep facilities, staff, equipment and operating budget. J. Benveniste in fact hardly believed in this last possibility, but he wished to put the administration of Inserm in front of its contradictions.

The title for this future structure would be “Cellular and molecular immunotoxicology of the toxic aggressions”. Three teams would constitute this future structure and J. Benveniste would not be the director anymore but responsible of a team called “Biophysics of the transmission of the molecular signal”.²

J. Benveniste irritates P. Lazar (again)

Joël Bockaert, the president of the Scientific specialized commission that was committed to examine the demand of creation of “junior-laboratory contract” wrote to J. Benveniste to get in touch with him about the visit of the laboratory:

“The documents that you had transmitted to me certainly deserve our attention. If we consider that the observations which you did concerning the effect of the physiological salt solution Biosedra on the heart of immunized guinea pigs are reproducible (and I have no reason to believe the contrary today), there is a good reason to examine the problem.”³

He thus suggested coming to Clamart with experts:

“On this occasion I suggested to Mister Philippe Lazar, who agreed, to ask the eminent physicist colleagues (Serge (*sic*) Charpak or Pierre-Gilles de Gennes) to accompany the members of the CSS [*Specialized scientific commission*] n°5 (Donny Strosberg, Claude Jacquemin and myself). We could add a cardiac physiology specialist. We will be able to examine the scientific aspect of this problem, the only one within our remit.”

J. Benveniste responded to this proposal very favorably and he suggested that another physicist should be added to the team of visitors:

“We are very honored that eminent physicists come to visit (at last) the laboratory. However, I tried to correspond several times with De Gennes [...] and obtained only superficial answers, thus giving me the feeling that he was hardly interested in these

biological problems. *Georges Charpak* (you mean the recent Nobel prize-laureate?) seems to me a priori more open to biology. However, the presence of physicists of this level raises a problem. We will be unable to answer because it is not within our competence to solve problems of physics. [...] In order to be able to provide an appropriate interlocutor to the visitors, we will ask to Professor G. Preparata, chair holder of Nuclear physics at the University of Milan, or to Professor Del Giudice who works in the same department, to attend this visit."⁴

Indeed, he specified:

"[...] the weight of a criticism, possibly left without answer, from a Nobel prize laureate would be such that we cannot approach this examination without possibility of contradictory exchanges that, maybe, we will ask to be included in the report. As for the specialist of cardiac physiology, the name that comes to mind is Pr. Coraboeuf, Orsay, one of the most respected in this domain, but I am obviously ready to examine any proposal that you will be willing to submit to me."

But, the director of Inserm, P. Lazar, had a copy of this letter and he was a bit irritated. He thus reminded J. Benveniste that he was not responsible, but Y. Thomas, for the demand of "junior-laboratory contract" and he added:

"It seems totally abnormal to me that you invited a number of personalities, who are external to the laboratory, to attend this visit that is aimed to supply to the competent authorities of Inserm the direct elements of appreciations about the legitimacy of Mrs. Thomas' demand. I therefore ask you explicitly to give up this invitation.

As regards the name of the other experts, it is obvious that it is out of the question to accept that you would select them yourself or, according your own terms, "examine with the president of the commission any proposal that he would be willing to submit to you"."⁵

J. Benveniste answered that he agreed to separate the demand of "junior-laboratory contract" which was under the responsibility of Y. Thomas. But, for the evaluation of the "magnetic contamination" of physiological salt solution, it seemed normal to him to give an interlocutor of the same level to G. Charpak:

"It is in this context that, with the aim to "provide an appropriate interlocutor" to Mr. Charpak, in order essentially to facilitate

scientific communication to which you cannot be opposed, it seems suitable that Professor Preparata, with whom we collaborated for four years, explains the physical bases of the phenomenon which we observe. He can then withdraw during the statutory evaluation of the demand of junior-laboratory contract.”⁶

And he reminded that an investigation, launched by the ministry of Health, was in progress (cf. previous chapter):

“I remind you that an investigation has just been asked to the National laboratory of health by Mr Kouchner. None of the skills will be too much to avoid errors with particularly heavy consequences in either direction.”

The fact that this visit could be the occasion to raise the question of the “contaminated serum” and especially to prompt an open scientific discussion about this subject appeared to excessively irritate P. Lazar. Indeed, to be clearly understood, he sent again a letter – a very abrupt one – to J. Benveniste where he specified some points:

“The object of the visit of Mister Bockaert and a delegation of INSERM in your laboratory is not "an evaluation of physiological salt solution", but exclusively the evaluation of the demand of “junior-laboratory contract” from Mrs. Yolène Thomas.

I thus most strongly maintain my observations that were formulated in my letter of March 5th, 1993. I recommended very precisely to Mr. Bockaert, President of the Specialized scientific commission n°5 of INSERM, not to accept any dialog with other people than those who appear in the demand of Mrs. Thomas. If you try to go beyond this recommendation of common sense, I would be forced to draw the appropriate conclusions as regards the continuation of the examination by INSERM of this demand of contract.”⁷

With an exasperated tone, he then adds in postscript: “I would be grateful if you did not force me in writing to you a third and why not a fourth letter on this matter. The indications of the present mail are firm and definitive”.

J. Benveniste is not the kind of person to be easily impressed, especially when only administrative and regulatory statements are opposed to scientific arguments. He thus answered to P. Lazar that his letter was in “complete contradiction” with the position of J. Bockaert who suggested “examining the scientific aspect of this problem”:

"I am amazed and worried to see that INSERM remains silent once again in front of a potential problem of public health which is presently investigated by the French drug agency and which, in view of our last results, is about to become an international problem.

[...] I note however that you did not answer my question on the existence of texts which would forbid the presence of some of our collaborators who could inform the scientific debate. This means that these texts do not exist and that your decision has no legal basis. I draw your attention on the fact that your position could be easily interpreted, during later confrontations, as a will from your part to avoid a scientific debate which would answer the question. [...] I have no means to "oblige" you to answer. You are free to do it or not and to proceed through unfounded "firm and definitive indications", that is to say a ukase. I will naturally be forced to obey, at least for the moment, in other words in the expectation of possible further developments."⁸

He specified that he maintained his position for the following purpose:

"taking advantage of the presence of Mr. Charpak to fully examine the biological and physical problems put by the contamination of the physiological salt solution, in a *totally independent* way and obviously off the record for the report of the visit for the "junior-laboratory contract" itself. Thank you for indicating to me *on which legal text* is this ban exactly based."

And, again, he insisted:

"For example, if the visit for the "junior-laboratory contract" was performed in the afternoon, why should scientists who are involved not have the slightest scientific discussion that morning with outside personalities? Given the importance for public health, which you do not appear to completely measure, to achieve a solid scientific dossier on this question as quickly as possible, I am determined to make every effort so that the necessary scientific dialog is established and that, for reasons which escape me, you do not wish to see supervene."

"Where is the trick?"

Finally, it was not a question of "contaminated serum" during the visit even if the visitors could participate in an experiment of "transmission". To answer the

questions of physics, as reported by M. Schiff, “Benveniste could put forward only a scientist who had not done physics for twenty years”⁹ and who was M. Schiff himself. During the presentation of the activities of the laboratory, the latter tried to explain the theory of the coherent domains of G. Preparata and E. Del Giudice, as he said in a letter to G. Charpak shortly after:

«Because the general director of INSERM refused to allow Prepara or Del Giudice to explain their theory of coherent domains, which for the moment seems to me the most promising one to solve the epistemological riddle posed by Benveniste’s experiment on the memory of water, I was led to act as a substitute and to formulate in front of you what I thought I had understood of this theory. I sent you a text beforehand and I gave an introductory talk at the beginning of your visit on April 21st, 1993 to which you seemed to answer through an argument from authority by explaining that you had consulted Mr. De Gennes, who himself had referred to Mr. Nozières, who, according to him, had declared that the theory of coherent domains was valueless.

After my talk on April 21st, you made an allusion to the possibility of mystification by presenting an anecdote about your past work with Joliot-Curie: on the occasion of a magician’s trick, Joliot would have asked to the present scientists: “where is the trick?” You will agree with me that the balance of power and the circumstances did not favor a serene discussion on this point.”¹⁰

But, even the biologists who participated in this visit did not seem to be willing to commit themselves, for example to envisage a collaboration with the laboratory of J. Benveniste. Indeed, as told by M. Schiff:

“At the beginning of the visit, the specialist of cardiac physiology expressed his skepticism about the reality of an observable effect of high dilutions with hearts of guinea pigs or rats, by indicating that he had never observed such effects. The institutional situation did not allow me to ask him the obvious question: did he perform the experiments in conditions of sensitivity which could favor such an observation? For example, did he use hearts of animals previously immunized as Benveniste did? I nevertheless suggested that collaboration with Benveniste was possible. My interlocutor answered me that the researchers of his laboratory would probably not agree and, moreover, that INSERM would have at first to attribute to this research dozens thousand francs.”¹¹

Besides, during the morning, Y. Thomas and G. Charpak exchanged some impressions:

"You think that this famous experiment of "transmission" will work? the Nobel prize laureate asks her.

– Yes, I think. Except for an accident, it works very well usually, Yolène answers.

– You'd better, otherwise you are dead." ¹²

Fortunately, the prediction of the Nobel prize laureate was not put to the test and the results of the demonstration did not lead the researchers in front of the executioner!

"Benveniste killed Charpak"

At the end of morning, having heard a part of the presentations of the researchers concerning the demand of junior-laboratory contract, the delegation participated in an experiment of "electromagnetic transmission". For this purpose, four sealed vials were chosen among twenty. By precaution, these vials were warmed at 70°C for 2 hours in order to "erase" a possible "electromagnetic memory". Four vials were numbered from n°1 to n°4. The vial n°1 was "naive", that is it was left intact. Three transfers were performed with water, endotoxin (LPS) and ovalbumin for vials n°2, n°3 and n°4, respectively. Each vial was "informed" during fifteen minutes by placing it on the output coil of the transmission device. Then the vials were coded (A, B, C and D) using the method of envelopes (cf. Chapter 4).

The rats used for the experiment had been immunized in order to be able to discriminate ovalbumin and LPS. The rats of the first lot (hearts n°1 and n°2) were immunized with bacteria (BCG) and 1 µg of albumin. The rats of the second lot (hearts n°3 and n°4) were immunized in the same way, but 30 days earlier with a booster of 10 mg of ovalbumin two days before the experiment. These various protocols of immunization allowed, according to protocols designed by J. Benveniste and his team, to make hearts n°1 and n°2 more reactive to endotoxin than to ovalbumin and hearts n°3 and n°4 more reactive to ovalbumin than to endotoxin.

Nevertheless, J. Benveniste warned the participants that according to the state of immunization of animals, it was possible that one of the "active" transfers (ovalbumin or LPS) could be ineffective. Actually, only one tube, tube A, successively induced a reaction of 4 isolated rat hearts, more particularly hearts n°3 and n°4 (Figure 6.1). According to the immunization protocols, it was most probably ovalbumin. This was confirmed by the close correlation of

the effects of tube A and those of ovalbumin at classical conditions, thus suggesting that tube A contained albumin-type activity.

As some visitors were in a hurry, they had to leave the laboratory before the end of the experiments with heart n°4. The envelope was then opened and the tube A turned out to be “transmitted ovalbumin” as suggested by the results. J. Benveniste observed G. Charpak who took the blow:

“I feel that Charpak who was haughty and sarcastic up to now, is strongly affected by the results. At the end of the unblinding, his face goes pale and he went out of the building for a few moments. I am even afraid that he might faint and I imagine the headlines of newspapers: “Benveniste killed Charpak”. We will see that it is rather the opposite which is going to occur.”¹³

“A historic responsibility”

As soon as the visitors had left, J. Benveniste began to draft a report concerning the visit of the commission. He broadcast this text by mail to all participants, asking them to indicate their possible points of disagreement. He began by noticing the absence of scientific criticisms from the members of the commission:

“No methodological criticism was presented by any member of the delegation. Our results had been sent to them before the visit, allowing a thorough examination. Nevertheless, no element cast the slightest doubt on statistical validity of the results compared with the controls. *No additional control was requested.* Of course, propositions of new experiments were made, in particular by Mr. Charpak, for example to isolate input vials and/or output vials in the transmission experiment to better understand the involved mechanisms. These requests incidentally presuppose the acceptance of the basic phenomenon with the aim of a deepening of the research, but are absolutely not revealing the lack of a *control*, the definition of which is very precise in experimental research. There is however a contradiction between these requests and the progressive reduction of the means granted to U200 by INSERM, both for funding and staff, including the closure without reopening.”¹⁴

He also insisted on the absence of criticism by the specialist of cardiac physiology: “Mr. Coraboeuf did not criticize the experiment on isolated heart

which appears to him in compliance with the rules of experimental cardiac pharmacology." What allowed him to conclude:

"Overall, no methodological criticism was expressed that allows casting doubt on the validity of the results. None of the members of the delegation even only suggested the possibility of an artefact, often put forward by convenience and/or mind laziness without theoretical or experimental proof. This silence can be considered as tacit approval, in the absence of methodological criticisms."

He then arrived to the experiment performed under the control of the delegation:

"This experiment was particularly demonstrative because not only we indicated the active vial but we announced it was likely ovalbumin (there was indeed another potentially active vial but, on this day, hearts were not sensitive to endotoxin. A check made on April 22 on other rats belonging to the same group show that there is a lack of sensitivity to endotoxin for the complete series)."

And, he maliciously reminded the "reaction" of G. Charpak at the time of the unblinding:

"I am not certain that, except Mr. Charpak who seemed to perceive the importance of this result, the members of the delegation realized to what they assisted: an anaphylactic shock induced by an "electromagnetic" signal *without any molecular support*."

And – as usual – J. Benveniste concluded his letter with lyricism while putting some pressure on the members of the commission:

"At the end of the visit, I drew delegation's attention on its responsibility (which could be a historic day if Mr. Charpak's judgment is confirmed)¹⁵ in the report of this day and the decisions which would ensue from it. I evoked the disastrous precedent of the visits of the Commission n°2 and of the Scientific council, the texts of which will remain in the pantheon of the scientific incomprehension (to be kind). [...] The Commission n°5 for which competence, the integrity and even the open-mindedness are praised is at the center of an epistemological problem with few precedents. Yet, I perceived on several occasions the temptation to give in to the "common sense" – we know what we must think about this in science – which would require exceptional proofs for results seemingly (that is in the light of the knowledge of moment) "impossible". "

A little while after the visit, on May 3rd, J. Benveniste wrote to the theoretical physicist Philippe Nozières, professor in the *Collège de France*, under the authority of whom G. Charpak had sheltered to discredit the theory of the coherent domains of the Italian physicists, asserting the latter “valueless”. In his letter to the theoretical physicist, J. Benveniste asked him the scientific motives which supported his words reported by G. Charpak as a definitive argument. In his answer, P. Nozières appeared to be flabbergasted about exchanges with G. Charpak on a theory he did not seem to know:

“Before answering you, I wish to contact Georges Charpak, who apparently steered you towards me. I do not know what he has in mind and why he considers me as particularly competent. I will certainly read the theoretical articles that you transmitted to me – at least out of scientific curiosity – but not for the moment.”¹⁶

The argument from authority that G. Charpak brandished during the visit of Inserm commission seemed rather weak. But J. Benveniste could not linger over this point because meanwhile the team of Clamart had knowledge of the report of the commission which decided about its fate.

Chapter 6. "You'd better ... otherwise you are dead"

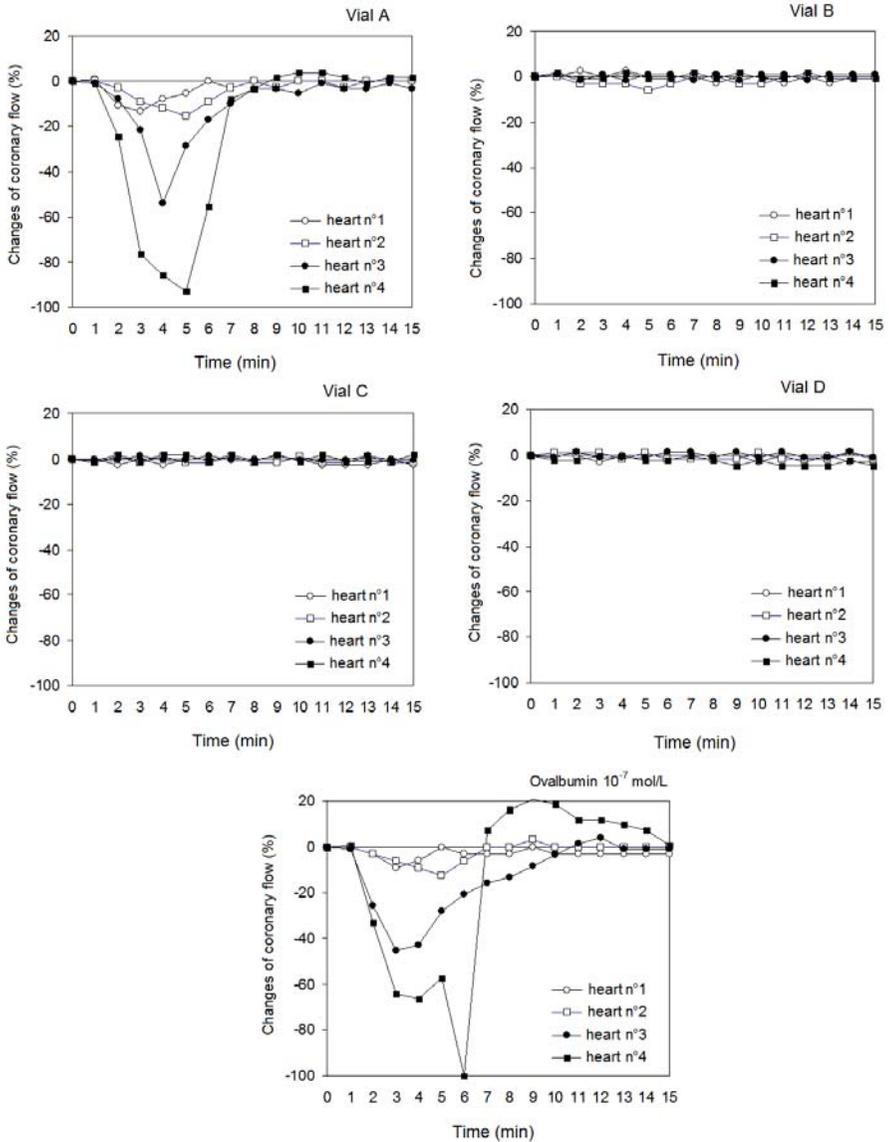


Figure 6.1. Experiment of April 21st, 1993 blinded by G. Charpak and by the members of the Specialized scientific commission n°5. Four sealed vials were numbered from 1 to 4: the vial 1 was kept without any manipulation whereas vials 2, 3 and 4 were “imprinted” for 15 minutes with “information” corresponding to water, LPS and ovalbumin, respectively. Each vial then received a random code (A, B, C or D).

Ghost of molecules – The game of heart and chance

(Followed)

The content of each vial was successively tested on 4 isolated rat hearts. At the end of each series, ovalbumin at usual concentration (0.1 $\mu\text{mol/L}$) was infused.

One notices that among blind vials only the content of A modified the coronary flow. Hearts n°1 and 2 were not very reactive compared with hearts n°3 and 4 which gave very important variations of flow. It is also important to note that the changes obtained with vial A and with ovalbumin 0.1 $\mu\text{mol/L}$ (the molecule the activity of which had been transferred) were correlated: compare the amplitude of the variations according to hearts on the first and last figures.

It was planned to test vials in the order ABCD for hearts n°1 and 3 and in the order DCBA for hearts n°2 and 4. However, for the heart n°2, one visitor was impatient to see the effect of tube A and the order was in fact DCAB. After unblinding, vial A corresponded to ovalbumin. See text for the reasons concerning the ineffectiveness of LPS transfer.

Notes of end of chapter

- ¹ The laboratory having been created in 1980, it was in fact 13 years old.
- ² The three teams were entitled « Toxic aggression and lymphocyte activation » (Y. Thomas, CNRS and C. Carelli, CNRS), « Toxic aggression and phagocytic cells » (C. Damais, CNRS and Y. Manuel, CNRS) and « Biophysics of molecular signal transmission » (J. Benveniste, INSERM and M. Schiff, CNRS).
- ³ Letter of J. Bockaert to J. Benveniste of February 16th, 1993.
- ⁴ Lettre of J. Benveniste to J. Bockaert of February 19th, 1993.
- ⁵ Lettre of P. Lazar to J. Benveniste of March 5th, 1993.
- ⁶ Lettre of J. Benveniste to P. Lazar of March 17th, 1993.
- ⁷ Lettre of P. Lazar to J. Benveniste of March 30th, 1993.
- ⁸ Lettre of J. Benveniste to P. Lazar of April 5th, 1993.
- ⁹ M. Schiff. Un cas de censure dans la science, p. 101.
- ¹⁰ Lettre of M. Schiff to G. Charpak of May 16th, 1993.
- ¹¹ M. Schiff. Séminaire du 19 octobre 1993 au Centre de recherche en histoire des sciences et des techniques, Cité des Sciences et de l'Industrie. A propos d'une recherche participante sur la mémoire de l'eau, p. 34 [*Text for the meeting of October 19th, 1993. About a participant research on memory of water*]
- ¹² J. Benveniste. Ma vérité sur la mémoire de l'eau, p. 154.
- ¹³ J. Benveniste. Ma vérité sur la mémoire de l'eau, p. 155.
- ¹⁴ J. Benveniste, April 23rd, 1993. Commentaires sur la visite de la Commission Scientifique n°5 de l'INSERM le 21 avril 1993 [*Comments on the visit of the Scientific commission n°5 of INSERM on April 21st, 1993*].
- ¹⁵ Cf. Chapter 10; G. Charpak had said: "If it's true, it is the biggest discovery since Newton".
- ¹⁶ Lettre of P. Nozières to J. Benveniste of May 17th, 1993.