

Chapter 8. "The stakes are beyond us, you and me"

The reluctances of G. Charpak

After the visit of the laboratory on April 21st, 1993 by the delegation of the specialized commission of Inserm, the principle of a collaboration with the laboratory of G. Charpak became obvious. One remembers that this proposal appeared in the report of the commission of Inserm. Moreover, faced with the results of the experience performed on that day, the skepticism of the physicist had seemed shaken – at least for a few minutes. Therefore, J. Benveniste decided to take advantage of the apparent good intentions of the Nobel prize laureate without wasting time. But, before setting up a true scientific collaboration which would take time, J. Benveniste organized a public experiment on May 13th to which G. Charpak was invited.

It was planned that this demonstration – “electromagnetic transmissions” followed by blinding of samples – would take place in a room at the *Institute Cochin of Molecular Biology* (ICGM)¹ put at the disposal of J. Benveniste by its director Jean-Paul Lévy. Indeed, since the experiment of December 10th, 1992 (which had not been completed²), the demonstrations of transmission experiments were performed at this place:

“Even if he is cautious about Jacques Benveniste's studies, Professor Jean-Paul Lévy, specialist of AIDS, gladly lends him a room at Cochin allowing him to lead his experiments: "it is necessary to let him search. He is not the devil. I do not need to exorcise the room when he leaves.”³

However, G. Charpak, who announced at first that he would personally attend the demonstration, finally delegated two of his collaborators, Claude Hennion and Jacques Lewiner. It was a disappointment, but it was nevertheless a positive sign with the aim of a future collaboration and the hope for J. Benveniste to escape from his scientific isolation.

M. Schiff presided over the organization of this experiment, which we are going to describe step by step. The experiment was indeed performed extremely carefully and, to increase the chances of success, it was simplified as much as possible. This demonstration was constituted in fact by four independent experiments. The purpose of each of these elementary experiments was “to guess” the position of a unique “active” sample among ten. Nine inactive samples contained “naive” water that was water not having undergone transmission. Indeed, in order to be not bothered with a possible background

noise, the transmission was performed only for the “active” samples. Furthermore, ten samples of every series were tested on a single heart. So, if biological activity was detected, it cannot be due to a previous sample which would have contaminated the system or modified the behavior of the heart for the following tests.

Transmission and blinding of samples were successively performed by eight people not belonging to J. Benveniste’s staff and working in pairs.⁴ Every pair performed a transmission and then blinded the samples by replacing the initial label with a code. The method of envelopes was used, like in the past. M. Schiff oversaw all the operations but did not participate himself in the process of transmission and blinding.

The manipulations to be carried out were exactly defined in a protocol. Each of the stages was registered on a check list and every stage must be carefully recorded. Within every pair of outside observers, the tasks were distributed in the following way: one of the observers performed the various operations whereas the other one watched him/her and participated in the blinding.

At first, a vial containing ovalbumin at 10^{-8} mol/L was placed on an input coil of the transmission device. Each of the teams successively chose ten tubes of distilled water among a stock and placed them in 10 envelopes. One of the envelopes was chosen and the corresponding tube was placed on the output coil of the machine. Each of the four transmissions lasted 15 minutes. The tube which underwent the transmission was then placed in its envelope and a label with participants' signatures was stuck *inside*. Envelopes were then placed in a box and were mixed. Labels in double were placed at the same time outside of the ten envelopes and on each of ten corresponding tubes, of course without looking inside the envelope. Labels were numbered from 1 to 10 for the first team, from 11 to 20 for the second, from 21 to 30 for the third and from 31 to 40 for the fourth. For every tube, both participants verified the concordance of the numbers of both labels. Envelopes were then placed in a large envelope which was sealed and entrusted to a bailiff until unblinding. Before and after the transmissions on tubes intended for blind tests, a transmission was performed with open-label samples in order to verify that the experimental conditions were correct, both at the beginning and at the end of the session.

Technical sheet of the experiment of May 13th, mai 1993

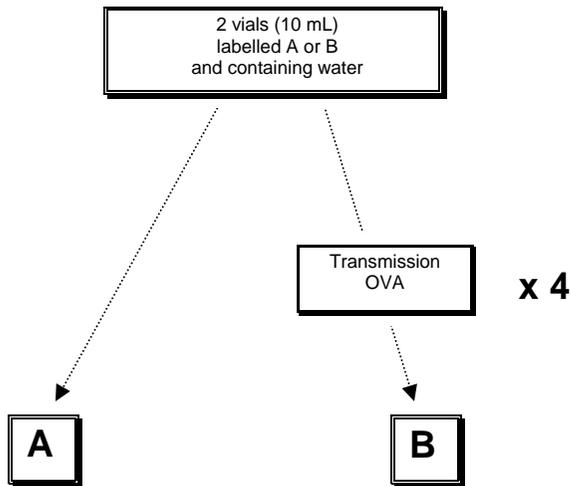
Type of experiment: transmission on May 13th

Place of the experiment: ICGM (Cochin institute) for transmission on May 13th and Clamart for assessment of samples from May 13th to 17th

Blinding: on May 13th by 8 participants not belonging to U200; unblinding on May 19th

Number of blind tubes to be tested: 4 experiments with 10 tubes each; each experiment was tested on 2 hearts

Additional in-house blinding: yes (between the two hearts)



Blinding of 10 tubes numbered from 1 to 10* (blind tests):

9 tubes « A »; 1 tube « B »

+

2 tubes not blinded (open-label tests):

1 tube « A »; 1 tube « B »

*Experiment 1: labels from 1 to 10; Experiment 2: labels from 11 to 20; Experiment 3: labels from 21 to 30; Experiment 4: labels from 31 to 40.

The tubes were then transported to Clamart where they were tested. Thanks to the method of envelopes, nobody, even those who attributed the codes, could know the codes of the active tubes. The tests were performed from 13 till 17 May on hearts of immunized rats.

Coherent results

The samples of four experiments were successively tested on four hearts of immunized rats on May 13th and 14th. The results (maximal changes of the coronary flow) are presented in the Table 8.1. The results were very encouraging because in each of the series, only one sample induced a change of coronary flow (8, 17, 21, 34).

Exp 1		Exp 2		Exp 3		Exp 4	
N°	Result	N°	Result	N°	Result	N°	Result
<i>Blind tests</i>							
1	3%	11	7%	21	21%	31	4%
2	3%	12	6%	22	5%	32	2%
3	3%	13	6%	23	5%	33	6%
4	3%	14	3%	24	5%	34	20%
5	6%	15	3%	25	3%	35	2%
6	3%	16	3%	26	3%	36	2%
7	6%	17	24%	27	3%	37	6%
8	46%	18	3%	28	5%	38	2%
9	10%	19	3%	29	3%	39	2%
10	7%	20	3%	30	3%	40	2%
<i>Open label tests</i>							
Water	5%	Water	3%	Water	4%	Water	4%
OVA tr	21%	OVA tr	18%	OVA tr	20%	OVA tr	15%
OVA 0.1 μmol/L	74%	OVA 0.1 μmol/L	59%	OVA 0.1 μmol/L	56%	OVA 0.1 μmol/L	44%

Tableau 8.1. Results of the first series of measurements (maximal changes of coronary flow) on 4 series of samples (experiments 1–4) of the transmission experiment dated May 13th, 1993. The contents of tubes 8, 17, 21 and 34 (in bold characters) induced changes of coronary flow. tr.: transmitted.

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To confirm these results, a new series of measurements was performed on May 15th and 17th after a new blinding of samples made by M. Schiff in the evening of May 14th. The second series of measurements was thus performed blind for the experimenters who could not link the second series of results with the first one. Thus, the experimenters had to test again four lots of ten samples whose the order had been modified within each series. Besides, the four series were switched around. The results of these second measurements are presented in Table 8.2.

N°	Result	N°	Result	N°	Result	N°	Result
<i>Blind tests (after additional in-house blinding of the first measurements)</i>							
A	-	B	2%	D	2%	C	5%
E	20%	F	2%	H	2%	G	5%
O	7%	N	2%	J	2%	I	6%
Q	3%	P	15%	M	2%	K	3%
U	3%	W	2%	S	2%	L	3%
V	3%	AB	2%	T	2%	R	6%
AA	7%	AG	2%	Z	2%	X	3%
AD	7%	AH	5%	AE	2%	Y	3%
AF	7%	AI	5%	AK	11%	AC	9%
AM	5%	AJ	5%	AN	2%	AL	3%
<i>Open-label tests</i>							
Water	3%	Water	4%	Water	4%	Water	3%
OVA tr	13%	OVA tr	10%	OVA tr	15%	OVA tr	13%
OVA 0,1 μmol/L	-	OVA 0,1 μmol/L	25%	OVA 0,1 μmol/L	17%	OVA 0,1 μmol/L	23%

Tableau 8.2. Results of the second series of measurements (maximal changes of coronary flow) on 4 series of samples (experiments 14) of the transmission experiment of May 13th, 1993 after additional in-house blinding by M. Schiff. Note that besides the additional blinding of the 10 samples within every series, the 4 series were switched around. The contents of tubes E, P, AK and AC were those that had the most importing effect on the coronary flow. The hearts of this second series of measurements were less reactive than those of first one.

tr.: transmitted.

For the second measurement of samples, the reactivity of hearts was clearly decreased, including for open-label samples and for ovalbumin at pharmacological concentrations (0.1 $\mu\text{mol/L}$). Nevertheless, in each series, a sample emerged: E, P, AK, AC. The internal unblinding indicated:

8 = E	17 = AC	21 = P	34 = AK
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The results of this second series of measurements were thus coherent with those of the first series. It was thus an extremely important result and apparently the final unblinding should confirm the success of the overall experiment. Were samples 8, 17, 21 and 34 the “first-four winners”?

The experiment is finally unblinded

On May 19th, the large envelope was opened in the presence of the participants and the small envelopes of the four series were opened. The numbers of the envelopes which contained a label indicating the active tubes were the following ones:

Experiment n°1: envelope n°8 = right
Experiment n°2: envelope n°18 = false
Experiment n°3: envelope n°26 = false
Experiment n°4: envelope n°34 = right.

And, again, the results were half disappointing because only two tubes out of the four, in the experiments n°1 and n°4, were in accordance with the expectations. Once again one did not understand why an activity could be present but not in the exact place where one expected it to be.

M. Schiff tried to understand the origin of these anomalies. But he was faced with two difficulties (that we will systematically find later on): on one hand open-label samples behaved as one could expect and on the other hand there was in-house blinding of these tubes so that the experimenters, J. Aïssa and H. Litime, could not influence the results. Yet, this second series of samples gave results which were coherent with those of the first series. Moreover, M. Schiff himself performed the in-house blinding of these tubes.

In a probabilistic model, M. Schiff tested two hypotheses. In the first one, there was a dysfunction of the measurement system working in an all-or-nothing manner. The second hypothesis supposed a contamination of some tubes with also all-or-nothing reactions that would be coherent for a given tube,

but at random concerning the origin of the tube. In both hypotheses, he obtained extremely low probability and he must reject them. He then concluded:

"The discordance observed in the blind experiments n°2 and 3 seems to be the consequence from anomalies of numbering which would have occurred between the transport of blind tubes to Clamart and the first series of measurements. It already seems to have occurred in another blind experiment.

In summary, the observed results do not seem to result from a random artefact, an artefact which would be due either to the measurement system or to a contamination of the study tubes. If there was an artifact, it would be much more subtle than the one which would result from such random errors in our *modus operandi*." ⁵

Thus, the conclusion of M. Schiff was close to that of J. Benveniste for the experiment of September 28th, 1992, namely an "anomaly of numbering". And he finished:

"The situation in which we are after this last series of experiments looks as dangerous as the one that resulted from the visit of three delegates of the journal *Nature* after the publication of the article on the achromasia of basophils. Too confident in the functioning of his experimental device and too confident in the ability of the other scientists to estimate in a rational way experiments that were at the same time difficult and surprising, the person in charge of these experiments was booby-trapped in a problem about fraud, which led to push the experimental device and his operators beyond their possibilities. Besides, the publication of the results of this "expertise" was followed by publications which were only a mockery of reproducibility as we have demonstrated. This past experience must serve as a warning, as well for us as for scientists whom we tempt to interest in the transmission experiments. Clearly, we look for collaborations and help to move forward in a complex research, from both theoretical and experimental standpoints, but we refuse to take the risk of repeating the scenario played in 1988 with *Nature*."

"Act as a scientist, not as a cop"

Even before the unblinding, a dispute began between the two collaborators of G. Charpak and M. Schiff. Indeed, during the session of May 13th, M. Schiff

considered that both representatives of G. Charpak had a nonchalant and unconcerned behavior that irritated him. Besides, when they told the Nobel prize laureate about their visit to Clamart, J. Lewiner and C. Hennion evoked – as a hypothesis it seems – a method which according to them would allow to mark the active tubes, in brief a possibility of fraud. As tinder which needs only a spark to ignite, a brief dispute began. M. Schiff being the organizer of this demonstration, he felt directly targeted by this suspicion. It was painfully ironic for him who tried to understand what was at work in “Benveniste’s experiments” by setting up experimental protocols which were flawless. Here he was in his turn in the eye of the cyclone. He could certainly say to himself that it would make another chapter in the book that he intended to write, but the suspicion, even light, was very hurtful. Some correspondences followed.

Thus, M. Schiff wrote to G. Charpak shortly after the experiment:

“Last Friday, I learned from Mr Benveniste that the report made to you by Mr Lewiner (or Mr Hennion, I do not know which) about the series of 4 demonstrations that I managed on Thursday the 13th of May within the laboratory of Unit 332 of INSERM at Cochin led you to be convinced that this series of demonstrations must have been vitiated by fraud, a fraud of which I was probably the agent. In an affair as complex and as delicate as this one, the fact that of going through intermediaries increases the communication problems. This is why I prefer to communicate with you directly. [...]

It seems that you interpreted my temporary irritation and the fact that I objected to your delegates interfering with an ongoing experiment as indicating that a fraud must have occurred. In case your informers did not report it, I mention the fact that I insisted that they should watch at least one of the four experiments; I also insisted that they should accept to play the role of participant-observer and of witness described in the protocol that you should have received. They refused and I insisted that they should at least be present to watch one of the experiments. Actually, they spent only half of the duration of one experiment in the demonstration room. What provoked my irritation was the fact that, instead of watching the ongoing operations, they turned their back to the apparatus and proceeded to argue with Benveniste on fraud and about the "open-mindedness" of the scientific community, which, according to them, is not as narrow-minded as Benveniste claimed. You must admit that I had excuses for loosing my temper!”⁶

J. Benveniste also wrote to the physicist:

"I am rather worried about the way things have evolved. I think that you are aware of how serious the simple use of the word "fraud" can be. [...] I regret your absence during the coded experiments of 13 May. You would have seen that the way it took place showed that every precaution had been taken against the possibility of some system of recognition. The point of the coding was not to combat fraud, but simply to avoid any possible bias of the technicians. Note that they receive numbered syringes that have been prepared by another technician, which means that they never see the original tubes.

When we heard of your coming, we said: "Finally a scientist!" It is therefore quite disappointing to hear that you are taking up again gossip which we thought we had been rid of since 1988. The idea that "someone is cheating behind Benveniste's back" was the way out used by *Nature's* group with its magician. At the present time, at least 10 people are involved in this research; each of them is thus under trial. Usually, scientists choose their best results once they are convinced that their hypotheses have been demonstrated. We do not act that way, but show everything to everybody, thus taking the risk that misinformation of the worst kind might come out of it. Sir, act as a scientist, not as a cop. What we have found, almost by chance, is indeed enormous. The stakes are beyond us, you and me. Given the issue involved, mediocre attitudes cannot be justified and are intolerable. You do not understand? Neither do I. But it exists. Contribute to the outcome of truth. [...]

Concerning the difficulty you have in understanding what is going on with this machine, you are not the only one. As you well know, the argument: "I do not understand, therefore it does not exist" has been used so often in the past that is completely discredited. [...]

However, the best way to cut short any suspicion of fraud would be for you to perform the experiment yourself in your laboratory. I remind you of the fact that this is what I had initially suggested (instead of Cochin). The experiment would be performed by two outside observers designated by both of us who would guarantee that the transfers occur according to a protocol that has been defined in advance. [...] You are a man of honour: you cannot make remarks that are degrading to a colleague and refuse to perform a verification that would stop the rumour."⁷

In a letter sent from the CERN at Geneva, G. Charpak tried to calm the situation:

“Please excuse my delay in answering your messages. I was not available because of journeys and conferences.

However, I made certain that two of my co-workers of the School of Physics and Chemistry go to Cochin, because their collaboration is essential for tests in their laboratory. They confirmed to me that the amplifier oscillated in a permanent way. But after thinking about it, I do not intend to draw any conclusion from it for the moment.

The effect which you observe, and you say is easily reproduced, needs only a simple test. The use of about twenty vials of water, some of which have been sensitized according to your method, using a protocol determined by you and without you being able to know the distribution of the vials, should permit an objective test.

During the visit of my co-workers at Cochin, there was a small discussion with Mr Schiff because they thought that they had noticed a possibility of marking the vials that had been sensitized during the phase of vibration. This certainly does not mean that this possibility was used. But it is clear that no doubt should remain. It will be easy to Mr Spira to define a protocol forbidding any suspicion.”⁸

J. Lewiner himself sent a letter to Mr Schiff in order to minimize what he and his colleagues were supposed to have said:

I received a copy of the letter you sent to Mr Charpak on May 16th, 1993 and it seems important to dissipate the wrong interpretation that seemed to have developed after our visit of 13 May.

Actually, on our return, we communicated to Mr Charpak our feeling about the experimental procedure chosen and we proposed one that differs very slightly and seemed susceptible to us either to convince the scientific community of the interest of your experiments or to show the necessity of additional experiments.

We certainly never claimed that that the series of demonstrations that you conducted was vitiated by fraud, and *a fortiori* fraud perpetrated by you.

Therefore we will propose to Mr Charpak an experimental procedure which, if it seems to him to be of interest, will probably be submitted to you.”⁹

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For the moment, however, the incident seemed closed. Besides, Mr Schiff discontinued at the end of 1993 his direct implication in the experiments of the laboratory. He explained afterward:

“As for me, the period when I conscientiously drafted experimental protocols of demonstration for people to whom the transmission experiments of transmission raise existential problems has passed. Tired of speaking with deaf people, I address others.”¹⁰

Then M. Schiff dedicated himself to draft a book on the censorship and the self-censorship in the scientific world based on the affair of the “memory of water” and his experience at Clamart. His methodological rigor was missing when the laboratories of J. Benveniste and G. Charpak collaborated. But before telling this episode, we have to review at first a curious phenomenon.

Notes of end of chapter

¹ Located Street Méchain in the 14th arrondissement on the campus of Cochin hospital.

² Cf. Chapter 5.

³ E. Fottorino. La mémoire de l'eau. Du rêve au soupçon. *Le Monde*, January 21st, 1997.

⁴ The four pairs of participants were the followings: experiment n°1, Françoise Russo-Marie and Jean-Claude Salomon; experiment n°2, Isaac Béhar and Jacques Testart; experiment n°3, M. Reynier and P. Pacaud; experiment n°4: J.Y. Follezou and P. Richard.

⁵ J. Benveniste and M. Schiff. Compte-rendu des expériences réalisées le 13 mai 1993 pour mettre en évidence la possibilité de dissocier une information moléculaire de son support d'origine. p. 13 [*Report on the experiments performed on May 13th, 1993 to evidence the possibility to dissociate a molecular information from its original support*].

⁶ Letter of M. Schiff to G. Charpak of May 16th, 1993.

⁷ Letter of J. Benveniste to G. Charpak of May 14th, 1993.

⁸ Letter of G. Charpak to J. Benveniste of May 19th, 1993.

⁹ Letter of J. Lewiner to M. Schiff of May 18th, 1993.

¹⁰ M. Schiff. Un cas de censure dans la science, p. 64.