

## Chapter 16. “We should open a chip shop”

### *The last session?*

Encouraged with the last results obtained between Clamart and Chicago, J. Benveniste was determined to pursue the transmission experiments at the Cochin institute by benefiting from the lessons learned during the collaboration with the researcher of Chicago. Among these lessons there was the “cautious” manipulation of the computer files. J. Benveniste then wrote to the participants – whose number steadily decreased – in these experiments:

“As you perhaps remember, before the holidays, we had left off on the following observation: when the activities are "played" from the hard disk the results are erratic while if one "replays" the original floppy disks they fit to what is expected. These results were consolidated by the third series of experiments with Chicago [...]. I remind you the principle: the laboratory of Chicago records activities (ovalbumin or water) and sends us by phone, two by two, the blind or open-label recordings. These recordings are made on individual floppy disks and received on individual floppy disks”.<sup>1</sup>

It is useless to insist again on the irrationality to consider that the “inversions” could find their source in the fact that the various recordings on the same IT medium could “interfere” at this level. But J. Benveniste hanged on to this hypothesis and he decided to do experiments again at the Cochin Institute according to the following principle inspired by the experiments with Chicago:

“We will record 10 series of 2 pairs of ovalbumin/water, pair by pair. We will return the results as we go along for every pair, avoiding a work of several weeks if there was another technical problem. These recordings will be made on floppy disks because we did not solve the mystery of the jamming of the recordings on hard disk.”

And, undoubtedly very optimistic, J. Benveniste concluded his letter with: “with the hope to see you in what could be the last session.” But on September 23<sup>rd</sup>, which was the date scheduled for the experiment, the recordings could not be correctly performed due to “a poor electronic connection” and the experimental session was postponed.

*“Where is the bug?”*

The demonstration was finally performed on September 30<sup>th</sup>, but the initial protocol which planned to record the samples by pairs was finally not followed. The records were tested on the isolated heart device from September 30<sup>th</sup> to October 4<sup>th</sup>.

Tested recordings	Number of measurements	Maximal changes of coronary flow (%)	Biological activities in increasing order	Unblinding
<i>Blind tests</i>				
n°1	2	2.6 ± 0.0	1	<b>Digital Ova</b>
n°4	4	4.2 ± 1.3	2	Digital water
n°8	2	5.0 ± 0.1	3	<b>Digital Ova</b>
n°9	2	5.2 ± 0.1	4	Digital water
n°7	2	13.6 ± 16.2	5	<b>Digital Ova</b>
n°5	1	15.8	6	Digital water
n°3	3	23.1 ± 10.7	7	Digital water
n°10	1	23.7	8	Digital water
n°2	2	34.0 ± 7.2	9	Digital water
<i>Open-label tests</i>				
Digital water 1	1	16.3	-	-
Digital water 2	1	45.0	-	-
Digital Ova 1	2	4.0 ± 0.1	-	-
Digital Ova 2	1	9.8	-	-
Ova 0.1 µmol/L	4	30.7 ± 14.4	-	-

Table 16.1. Public experiment of September 30<sup>th</sup>, 1996 (unblinded on October 8<sup>th</sup>). An incorrect number of active samples was found (5 active while only 3 had been included). Note that the open-label recordings were also unsatisfactory (they had been given blind to the experimenter). In this picture and the following ones, the results are given as mean ± standard deviation. NB. There was no recording n°6.

The blinding was performed by a researcher of the CNRS. When the experiments were finished, the latter communicated the code to J. Benveniste by fax. The results and the code were incoherent. “It is a mess. But it is not a mess” commented then J. Benveniste in a letter to the “coder” of the experiments.<sup>2</sup>

J. Benveniste illustrated his words through examples that were derived from this last experiment which demonstrated that once again something surprising occurred. Thus, samples supposed to be only water modified very clearly the coronary flow in animals immunized with ovalbumin, but were without effect in non-immunized animals. Furthermore, the profile of variation of the coronary

flow obtained with such a sample which was supposed to be "inactive" was identical to that of ovalbumin at "classic" concentration. This "water" thus possessed all the characteristics of an ovalbumin activity. It seems that there was "transmission" of a biological activity but not at the right place! J. Benveniste concluded: "where is the bug?" Always clinging on to IT problems, he suggested redoing in-house experiments before launching again public demonstrations:

"If the unused floppy disks give nothing better, it means that the bug remains unidentified and it will be necessary to do experiments by telephone, floppy disk by floppy disk, as with Chicago where it worked 29 times out of 29".

*"The results were excellent"*

On October 24<sup>th</sup>, J. Benveniste sent a letter to his usual correspondents to review the recent experiments previously described:

"The last three experiments in Cochin did not work. According to the logic at present dominant in the "research", we should open a chip shop on N-306.<sup>3</sup> "

He nevertheless drew up the inventory of what was "absolutely sure":

1. Water that has been recorded and "replayed" by a computer to water cannot influence the parameters of an isolated organ infused by the same water. [...]
2. What we record is indeed ovalbumin. All the criteria, which I will not detail again, are present. Except that it is found on a floppy disk "water" and vice versa. [...]
3. It means that the system stumbles (do not ask me how) as if there was a "persistence" and that, although we believe ovalbumin is recorded, we sometimes record water and vice versa."

An issue that is not addressed by J. Benveniste is the fact that the "inversions" are quite infrequent as we previously noticed with the open-label recordings performed at the same time as the blind recordings.<sup>4</sup> Besides, he did not evoke either the fact that the experiments performed "in house" – and not during public demonstrations, such as the "High Masses" at the Cochin Institute – are most often successful. This was involuntarily illustrated by J. Benveniste during his letter where he reported the results of experiments of limited size and completely performed in house:

"We have just made two experiments at Cochin, perfect successes in the following conditions: laptop computer, naked sensor, no

box of Faraday, no cylinder of mumetal. Recording of "water" files one after the other on floppy disk coming from a brand new box. One switches off the computer, one talks 5 min and then recording of "ova" files, saved without blinding. All is thus done open-label without a screen, etc. Then two operators using a software on the laptop erase the hours of recording and rename the files with random numbers”.

I report these technical details to show again the obnubilation of J. Benveniste for the IT media which could be the source of a possible “persistence”, what led him to use floppy disks “coming from a brand new box”. We also note that all the processes which had previously been considered as progress, for example protection from ambient electromagnetic waves (Faraday cage, box of mumetal) are now forgotten and neglected. He pursued :

“The results were excellent. For the first one, I only knew the code. The second one was recorded in Cochin by Pete Jurgens alone and then coded at Clamart by Francine Joly and Francis Beauvais. Neither me, nor Jamal knew the codes. Here are the results of the second one [...]. A third experiment is on going. [...]

The results of these “second” and “third” experiments mentioned by J. Benveniste are presented in Table 16.2. The first experiment was performed on October 16<sup>th</sup>, 1996 and the two next ones on October 22<sup>nd</sup> and 25<sup>th</sup>. In these last two experiments, which took place in-house, but blind for the experimenter J. Aïssa, one notices that it was a success because the lowest effects fitted indeed “water activity” and the highest effects fitted “ovalbumin activity”. It was then very difficult to understand why such successful results were not obtained during the public experiments.

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*In-house experiment of October 22<sup>nd</sup>, 1996 ("second experiment")*

Tested recordings	Number of measurements	Maximal changes of coronary flow (%)	Biological activities in increasing order	Unblinding
<i>Blind (in-house) tests</i>				
A	2	2.4 ± 0.1	1	Digital water
F	2	3.1 ± 1.4	2	Digital water
B	2	5.7 ± 1.7	3	Digital water
E	2	21.0 ± 5.7	4	<b>Digital ova</b>
C	1	27.3	5	<b>Digital ova</b>
D	1	31.1	6	<b>Digital ova</b>
<i>Open-label tests</i>				
Ova 0.1 µmol/L	2	28.6 ± 1.1	-	-

*In-house experiment of October 25<sup>nd</sup>, 1996 ("third experiment")*

Tested recordings	Number of measurements	Maximal changes of coronary flow (%)	Biological activities in increasing order	Unblinding
<i>Blind (in-house) tests</i>				
D	3	2.9 ± 1.2	1	Digital water
C	2	3.9 ± 1.8	2	Digital water
F	4	7.4 ± 5.5	3	Digital water
B	3	14.3 ± 10.9	4	<b>Digital ova</b>
A	2	22.7 ± 1.6	5	<b>Digital ova</b>
E	1	42.2	6	<b>Digital ova</b>
<i>Open-label tests</i>				
Ova 0.1 µmol/L	2	39.4 ± 9.8	-	-

Table 16.2. In-house experiments of October 22<sup>nd</sup> and 25<sup>th</sup>, 1996.

The "second" experiment mentioned by J. Benveniste in his letter of October 24<sup>th</sup>, 1996 (see text) included 6 recordings (3 ovalbumin and 3 water); the recordings were performed in Cochin institute on October 22<sup>nd</sup> and were tested blind for the experimenter on October 22<sup>nd</sup> and 23<sup>rd</sup>. The "third" experiment was recorded on October 25<sup>th</sup> in Cochin institute. The recordings were tested from October 25<sup>th</sup> to 30<sup>th</sup>; for technical reasons, only the results of October 28<sup>th</sup> and 30<sup>th</sup> were included in the analysis. Despite the variability of the results of these two experiments, after unblinding it turned out that the 3 most active recordings (on average) were indeed the 3 "active" recordings (Digital ova). These experiments were performed in blind conditions for the experimenter; there was a new interim blinding during the experiment of October 25<sup>th</sup>, but not for the experiment of October 22<sup>nd</sup>.

*“An irritating problem, which has nothing to do with the content of the experiment”?*

After these successful, but in-house experiments, a new “public” attempt took place on November 4<sup>th</sup>. Public is perhaps not a very appropriate term because only two people not belonging to the team were present in the Cochin institute to help J. Benveniste and one of his collaborators.<sup>5</sup>

Tested recordings	Number of measurements	Maximal changes of coronary flow (%)	Biological activities in increasing order	Unblinding
<i>Blind (in-house) tests</i>				
n°9	2	2.1 ± 0.0	1	Digital water
n°3	4	3.0 ± 1.2	2	Digital water
n°6	2	3.2 ± 0.1	3	Digital water
n°7	2	4.0 ± 0.0	4	<b>Digital ova</b>
n°5	2	5.1 ± 2.3	5	<b>Digital ova</b>
n°2	3	8.6 ± 4.2	6	<b>Digital ova</b>
n°4	1	14.3	7	<b>Digital ova</b>
n°8	1	16.7	8	Digital water
n°1	3	17.4 ± 4.1	9	<b>Digital ova</b>
n°10	2	22.6 ± 13.2	10	Digital water
<i>Open-label tests</i>				
Digital ova	2	14.7 ± 1.9	-	-
Ova 0.1 µmol/L	4	35.2 ± 14.3	-	-

Table 16.3. Public experiment of November 4<sup>th</sup>, 1996. The experiment was tested from November 5<sup>th</sup> to 8<sup>th</sup>, 1996 and the unblinding was done on November 8<sup>th</sup>. There was no in-house blinding.

As we can notice on Table 16.3, the experiment was once again a failure. These results were received with fatalism. Nevertheless, as Sisyphus and his boulder, J. Benveniste did a new “private” experiment. A “Cochin-type experiment” was thus performed on November 13<sup>th</sup>, 1996. And again, in spite of important variations of the measurements for some samples, if we consider the means of the 4 more active and the 4 less active, the samples were in the expected order (Table 16.4).

Tested recordings	Number of measurements	Maximal changes of coronary flow (%)	Biological activities in increasing order	Unblinding
<i>Blind (in-house) tests</i>				
n°1	2	3.1 ± 1.5	1	Digital water
n°7	6	4.2 ± 3.3	2	Digital water
n°4	5	4.6 ± 6.2	3	Digital water
n°2	3	5.7 ± 5.7	4	Digital water
n°6	8	9.7 ± 9.0	5	<b>Digital ova</b>
n°5	4	16.2 ± 6.7	6	<b>Digital ova</b>
n°8	1	18.0	7	<b>Digital ova</b>
n°3	1	20.0	8	<b>Digital ova</b>
<i>Open-label tests</i>				
Ova 0.1 µmol/L	4	25.9 ± 6.2	-	-

Table 16.4. In-house blind experiment of November 13<sup>th</sup>, 1996.

The recordings were tested from November 13<sup>th</sup> to 18<sup>th</sup>. There was an additional blinding for the last measurements. The recordings that were on average the most active correspond well to the recordings which were supposed to be active (digital ova).

At the end of November, J. Benveniste summarized the situation in these terms:

“Here is where we stand:

We have just made 5 in-house blind experiments, among which 4 were in Cochin. In spite of some irregularities of response of the sensor which we detected (thus showing the difficulty making blind experiments with this biological system where we inject the samples one after the other on the same organ during 6-8 hours), we did not make errors. [...] On the other hand, during the last public experiment, on 4/11/96, many activities were inverted. Recordings of ovalbumin had no activity, something which is still possible if the recording is missed. Indeed, the recordings of 4/11, made without external amplifier were weak. Much more surprising, some recordings of water had the typical activity of ovalbumin [...] what is obviously impossible. This is indeed a substitution because the recordings are “true” or “false” but always work in the same way. How these substitutions occur? No hypothesis is likely. What can we do? We are going to redo a public experiment (4 Ova and 4 Water) by adding an additional precaution which we tested in the last two in-house experiments: every step will be recorded on an external hard disk. In this way we can compare the profiles of each

recording [...]. The cartridge of the hard disk will then be entrusted to the manager's assistant of the ICGM of Cochin where the recordings are done. We will get back it only after the unblinding. We should understand this irritating problem, which has nothing to do with the content of the experiment, but which blocks us for one year.”<sup>6</sup>

One has the feeling that J. Benveniste did not know which solution to opt for. In spite of these improper substitutions, he clung up – with good reasons – to the fact that some recordings of “water” had an incomprehensible ovalbumin-type activity. It is his own “*E pur si muove*”. As to whether “this irritating problem does not have nothing to do with the content of the experiment”, nothing is less sure and one may be entitled to disagree with J. Benveniste.

J. Benveniste concluded by scheduling a public experiment for December 4<sup>th</sup>. This experiment was in fact the swan song of the “Cochin experiments”. Indeed, because of a lack of sensitivity of the hearts of guinea pigs, the experiment was not pursued until term and only the first 7 recordings were tested (Table 16.5). After unblinding, the most active samples were supposed to be inactive!

Tested recordings	Number of measurements	Maximal changes of coronary flow (%)	Biological activities in increasing order	Unblinding
<i>Blind tests</i>				
n°6	2	4.0 ± 2.0	1	<b>Digital ova</b>
n°3	8	4.4 ± 0.9	2	<b>Digital ova</b>
n°2	10	4.5 ± 0.9	3	<b>Digital ova</b>
n°4	7	9.1 ± 3.5	4	Digital water
n°5	7	12.8 ± 8.8	5	Digital water
n°7	1	13.9	6	Digital water
n°1	5	19.3 ± 12.1	7	Digital water
<i>Open-label tests</i>				
Eau num	7	5.2 ± 5.7	-	-
Ova num	12	12.9 ± 4.2	-	-
Ova 0.1 µmol/L	9	19.0 ± 4.0	-	-

Table 16.5. Public experiment of December 4<sup>th</sup>, 1996.

The measurements were performed from December 4<sup>th</sup> to 24<sup>th</sup>. The experiment was unblinded only on April 28<sup>th</sup>, 1997. There was in-house blinding during the tests. Ironically, there were the recordings which had the highest mean biological effect which were supposed to be inactive.

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The year 1996 finished in such an experimental mess that it was difficult to imagine how to escape this obsessing and incomprehensible circle. Nevertheless, quite unexpectedly the year 1997 offered to J. Benveniste the possibility of believing in brighter future.

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<sup>1</sup> Letter of J. Benveniste “to the participants in transmission experiments” of September 13<sup>th</sup>, 1996.

<sup>2</sup> Letter of J. Benveniste to P. Lacombe of October 8<sup>th</sup>, 1996.

<sup>3</sup> National Road not far from the laboratory...

<sup>4</sup> Except precisely for this experiment of September 30<sup>th</sup>, 1996 where samples from open-label transfers were nevertheless blind for the experimenter.

<sup>5</sup> The two participants who did not belong to the laboratory were Michel Troublé (Framatome) and Dominique Esclar (L'Oréal).

<sup>6</sup> Circulat letter of J. Benveniste of November 25<sup>th</sup>, 1996.