Chapter 15. Transatlantic dreams

The shadow of Lindbergh

In February 1997, J. Benveniste presented a communication to a congress in San Francisco in the form of a “poster” describing his last results. The title of the communication was “*Transatlantic transfer of digitized antigen signal by telephone link*”.[1]

Except the reference to “digital biology”, this title was unusual. Why was such a geographical parameter specified in an experiment about biology? If needed, one could have spoken about the “long-distance” transfers to further emphasize on future possible applications. Even in this case, billions of computer files permanently go around the world and nobody is upset. Indeed, as soon as information is digitized, the material medium (compact disk, floppy disk, magnetic tape, hard disk or file transferred by Internet) does not matter. To finish, the files are transmitted through Internet by “packets” that are not necessarirly in keeping with the geographical logic of the shortest path.

Maybe the answer is cultural, not to say generational. J. Benveniste was indeed an admirer of the pioneers of aviation and particulary the airmail service pionners such as Henri Guillaumet (“What I have done, no animal would have done.”) It is thus possible that this insistence to talk about “transatlantic transfer” in the title of a communication at a scientific congress was related to the dream which always accompanied any victory on this ocean in those days. The crossings of the first “transatlantic” liners, the installation of the first submarine telegraphy or telephony cables, the first flights above the Atlantic Ocean, all first scientific or technical successes concerning this ocean were always human adventures. The passion of J. Benveniste for car racing, engines, sailing and exploits accomplished with panache, is the likely explanation for this curious precision on which he insisted on many occasions. In support of this idea, one can evoke his letter to the French President when he tried to draw the attention on his discoveries: “A phenomenon of which he warns the president of the Republic, on June 13th, 1996, by presenting it as an issue which was more important than the flight of Lindbergh over the Atlantic Ocean…” [2]

A “Masked Researcher” comes on stage

These “transatlantic transfers” were performed in collaboration with a scientist of Chicago. But, up to February 1997, which is the date of the congress of San Francisco, J. Benveniste refused to reveal the identity of the American

researcher not to damage the latter. Who was this "Masked Researcher" on whom J. Benveniste cast a shade of mystery for a while? Without revealing any name, E. Fottorino portrayed this scientist early 1997:

> "Contrary to what Georges Charpak suggests, "the masked professor of Chicago" is not at all an eccentric. The numerous publications of this professor (more than eighty) in high-level journals (European Journal of Pharmacology, Journal of Immunology and even... Nature) demonstrate his professional qualities. As a renowned pathologist, he manages at once, as it is common in this domain in the United States, practitioner's activity (diagnosis before surgery) in a hospital and in a research program regularly renewed by NIH (National Institute of Health). His studies on PAF-Acether brought him, for twenty years, into contact with Jacques Benveniste. But, as he admits himself, he does not understand anything "neither about water nor physics"."

The journalist explained the role of the scientist of Chicago in the experiments) of "digital biology":

> "His role is at the same time modest but essential for the French researcher. Modest, because he simply records the frequencies of ovalbumin and water on a floppy disk on his computer and then transfers them by Internet to the computer of Benveniste, after having coded them. Why go to Chicago while a transfer from Paris would be enough? This is where the role of the masked researcher becomes essential: the latter asserts that no fraud is possible; Benveniste has one chance out two to guess (or to make a mistake). Among twenty nine sendings, he recognized "naive water" or ovalbumin each time by "playing" messages recorded in Chicago on isolated hearts of guinea pigs in Clamart. "I strictly respect his protocol, the American professor explains. He sends me his results. He cannot falsify them. His data are right. But I cannot interpret them nor evaluate their impact. In fact, I am not the right person to help him, because it is not my area of expertise. His problem is to meet a physicist of water." "

Let us see in which circumstances these "29 experiments", which are presented as a success, have been performed. If this success was so certain it was of course extremely important because, at the same moment, the "public experiments" took place at the Cochin institute and J. Benveniste as we saw in the previous chapter the same irritating problem is always an obstacle.

The Chicago-Clamart connection

The "masked researcher" was Dr Wei Hsueh, Professor of Pathology in Children Memorial Hospital at the Norwestern University Medical School of Chicago. She is the opposite of G. Charpak because – except the fact that she is a woman – she considered the experiments of J. Benveniste with benevolence and friendship. As she was acquainted with J. Benveniste for a long time, she knew his qualities, both good and bad. *Le Monde* – which spoke about her using the masculine gender to respect her temporary anonymity – reported her words about J. Benveniste and his studies:

> "According to this researcher, it is premature to judge the work of Benveniste. "He is himself too much in a hurry. He should have better controlled his system before showing it to Charpak. If it is an artefact, it is consistent. If it is the truth, it is consistent." He adds: "The main problem of these experiments is that they come from Benveniste. I sometimes meet honorable researchers treating him of scientific swindler. I ask them if they know him. They answer no. Benveniste is sometimes a little bit megalomaniac, as many are in this milieu, persuaded that they are themselves the truth. Maybe it is the key to success. Before this affair, Benveniste was on the way towards the success. His contribution on PAF-Acether is indisputable." While admitting that his provocative attitude (and his impatience) is detrimental to him, the professor of Chicago wonders about the "excessive" reaction of the milieu. "It is not worthy of a scientific community to condemn what is unexpected. Benveniste does not deserve that type of treatment. He needs means and one should leave him with a real opportunity to prove what he claims. If he is lucky, he will find the practical verification and the therapeutic application of the phenomenon before the theory. In science, it is often the opposite. Such a stake could justify investments." [3]

For the first experiments, the recordings performed by W. Hsueh were sent through Internet to the laboratory of Clamart. It is important to note that W. Hsueh performed the recordings by pairs that systematically contained an "active" recording and an "inactive" one. The "aim of the game" was thus to "guess" their respective places. Thus, each time one had one chance out of two to find the expected result. But again one has to repeat that this is not a simple exercise of divination because one observes a modification of a biological parameter, namely coronary flow. In other words, something "moves" although

the biological system should remain stable because what is administered to the heart is not different from the fluid that permanently infuses it.

To perfom these "transatlantic" experiments, J. Benveniste supplied W. Hsueh with all the necessary equipment, in particular the sensor which was connected to the soundcard of the computer, allowing the recording of "activities". He even went to Chicago to explain its functioning. The first experiment took place on April 10th, 1996. It was a success, but the experiment was open-label in order to verify that everything correctly worked (Table 15.1). Five other pairs were tested until April 19th. Each recording was "played" to naive water which was then tested on both devices of Langendorff which worked in parallel for the consistency of the results. Among these 6 experiments containing each a pair of recordings, the correct answer was obtained for 2 pairs (for the pair n°3, one could not conclude). This was thus the same configuration as the experiments of Cochin with frequent "inversions" of activity.

J. Benveniste and W. Hsueh then decided to change the method. The recordings would be copied to floppy disks which would be sent to Clamart by surface mail. The rationality of this decision is difficult to understand because a digital recording is the same whatever the medium. One remembers however that during the experiments performed at the Cochin institute, J. Benveniste suspected that the results could differ if files were recorded on floppy disk or on hard disk. On May 24th, that is one month later, two new pairs of recordings arrived by surface mail were tested and gave correct results (pairs n°7 and 8 of table).

A new series of 3 pairs of recordings (from n°9 to 11 of Table 15.1) was then realized by W. Hsueh each containing a recording of "acetylcholine" and a recording of "water". The recordings were sent by Internet, but J. Benveniste took care, as soon as he received them on his computer, to save them on floppy disk and not on the hard disk. Once again, this procedure could seem totally irrational. Three pairs of recordings were tested from June 3rd to 19th. But the effects observed on isolated hearts until June 10th had low amplitude and were unconvincing. Only the answers obtained from June 10th to 19th were taken into account.

N° of experiment *Test date*	Names of recordings	Number of measurements	Maximal changes of coronary flow (%)	Blinding and sending (Mail or Internet)	Success
n°1	D	3	4.7 ± 2.0	Digital water (I)	Yes
April 10	F	6	17.8 ± 9.0	Digital ova (I)	*(open-label)*
n°2	A	2	18.8 ± 15.1	Digital water (I)	No
April 16	C	3	5.6 ± 2.9	Digital ova (I)	
n°3	G	2	19.0 ± 11.2	Digital water (I)	?
April 16	I	2	14.4 ± 8.6	Digital ova (I)	
n°4	J	2	14.2 ± 3.5	Digital water (I)	No
April 17	L	2	4.5 ± 0.7	Digital ova (I)	
n°5	M	4	31.5 ± 18.4	Digital water (I)	No
April 17	N	4	5.3 ± 1.8	Digital ova (I)	
N°6	O	2	4.4 ± 2.5	Digital water (I)	Yes
April 19	P	2	25.1 ± 10.5	Digital ova (I)	
n°7	**Q**	2	7.0 ± 1.9	Digital water (M)	**Yes**
May 24	**S**	1	17.1	Digital ova (M)	
n°8	**W**	2	16.8 ± 15.6	Digital water (M)	**Yes**
May 24	**X**	2	4.7 ± 2.2	Digital ova (M)	
n°9	**21**	10	4.9 ± 0.5	Digital water (I)	**Yes**
June 10-19	**22**	6	20.9 ± 2.8	Digital ACh (I)	
n°10	**23**	6	22.4 ± 1.8	Digital ACh (I)	**Yes**
June 10-19	**24**	8	9.8 ± 3.9	Digital water (I)	
n°11	**25**	9	10.2 ± 2.2	Digital water (I)	**Yes**
June 10-19	**26**	5	26.7 ± 7.1	Digital ACh (I)	
n°12	AA	4	20.4 ± 3.7	Digital water (I)	No
June 17-26	AB	8	4.7 ± 0.8	Digital ACh (I)	
n°13	AC	6	3.7 ± 2.1	Digital water (I)	Yes
June 17-26	AD	9	13.8 ± 9.0	Digital ACh (I)	
n°14	AE	6	10.2 ± 2.9	Digital ACh (I)	No
June 17-26	AF	4	30.9 ± 6.1	Digital water (I)	
n°15	AL	7	9.8 ± 6.6	Digital water (I)	Yes
June 17-26	AM	7	16.3 ± 10.2	Digital ACh (I)	

Tableau 15.1. "Chicago experiments" of April-June 1996.
During this series of experiments performed with recordings which were sent either by Internet (I) or by surface mail (M), thirteen blind experiments were interpretable. A success was obtained for 8 of them (chance only would allow 6.5 successes on average).
The results are given as mean ± standard deviation.
The experiments which were included in the communication at the congress of San Francisco (see text) are in bold characters.

The last recordings having given the activities which fitted the code, J. Benveniste asked W. Hsueh to send an official letter describing the results of these experiments (that is experiments from n°7 to 11) and guaranteeing that files were sent blind to Clamart. W. Hsueh thus sent a letter on headed paper of her hospital department where she specified that she "guarantees that she herself recorded the files and that she was the only one to know the code before Dr Benveniste sent her the results." [4] J. Benveniste transmitted this letter to his usual correspondents, but he specified that "for the sake of discretion" he masked the author of the letter. [5]

The third series of recordings which included 4 pairs (from n°12 to 15 in Table 15.1) was then launched. The purpose of J. Benveniste was to achieve a sufficient number of experiments to present them to the congress of immunology of San Francisco which would take place in February 1997. The recordings were tested from June 17th to 26th. But, for this series, the results did not fit the codes. Other experiments were performed in order to understand the source of these discrepancies, but the different and contradictory results according to the transportation of the computer files were obtained and the highest confusion settled down between Chicago and Clamart.

"In thirty years, I have never been treated in such a manner"

J. Benveniste incriminated the computer of W. Hsueh and he remained persuaded that the same recording gave correct results when it stayed on the original floppy disk but that the problems arose when it passed through the hard disk of the computer. He evoked even the possibility of persistence at the level of the computer memory. He also persuaded himself that a single recording with one floppy disk was "safer" than several recordings on the same medium. For an IT specialist it is complete nonsense. Indeed, any computer record is a series of 1 and 0. It is the only "reality" of IT. Nevertheless, J. Benveniste submitted the computer which he used to a strict "cleaning"; he asked to W. Hsueh to do the same cleaning for her computer and to eliminate all the former recordings in order to start fresh again.

At the end of August, W. Hsueh finally performed new recordings by pairs (one "active" and one "inactive") or by triplets (one "active" and two "inactive"). J. Benveniste asked the latter to buy new preformatted floppy disks of a brand which was different from the previous one and to do each recording on a single floppy disk. Once again, from an IT point of view, it makes no sense, especially because W. Hsueh sent the recordings on floppy disks *via* Internet; she gave the code after having received the results of each pair or triplet. It is also necessary to note that the files were repeatedly tested by the experimenter, J. Aïssa, under different new codes so that he could not link,

consciously or unconsciously, the successive results. The results are summarized in Table 15.2. But, towards the end of the series, J. Benveniste realized that W. Hsueh coded the recordings always in the same order, with the active recording in first position. She was thus invited for the following recordings to pay attention on this point which could be criticized (in fact there was only one additional recording).

Besides, "technical problems" with experiments n°2 and n°4 made that J. Benveniste knew the code. In reality, an "inversion" had been straightaway obtained for these experiments. The supposed "technical problems" having been fixed, J. Benveniste performed new transfers and gave blind samples to the experimenter. The "expected" results were then obtained.

I do not specify these points by obsessional passion for detail or in order to suggest that J. Benveniste sometimes "adjusted" the results. Moreover, the fact that for two pairs of recordings the code was known has not been hidden and has been clearly indicated in the communication to the congress of San Francisco. My purpose is to show what is bench research with trials and errors, hesitations, periods of enthusiasm or disappointment. Especially, we can see in these experiments that J. Benveniste himself had the attitude he blamed his "opponents" for: he is prisoner of his own prejudice on what results should be. In his defense, we could add that obtaining consistent results while the only differences live apparently in a series of 1 and 0 on a computer memory is already totally perplexing. The fact that the results do not fit the code is another question which, at this stage, remains incomprehensible.

N° of experiment	Names of recordings	Number of measurements	Maximal changes of coronary flow (%)	Blinding and sending (Mail or Internet)	Success
n°1	**C2**	3	29.2 ± 16.3	Digital ova (I)	**Yes**
	C5	2	2.4 ± 1.1	Digital water (I)	
n°2	**C4**	2	25.2 ± 0.8	Digital ova (I)	**Yes**
	C6	2	2.1 ± 0.0	Digital water (I)	*(en ouvert)*
n°3	**C7**	4	22.1 ± 12.3	Digital ova (I)	**Yes**
	C9	5	3.3 ± 1.6	Digital water (I)	
n°4	**C8**	3	16.4 ± 0.9	Digital ova (I)	**Yes**
	C10	2	3.6 ± 1.7	Digital water (I)	*(Open-label)*
n°5	**C1**	4	24.1 ± 5.8	Digital ova (I)	**Yes**
	C3	6	3.4 ± 1.8	Digital water (I)	
n°6	**C11**	2	35.3 ± 0.6	Digital ova (I)	
	C16	2	4.4 ± 1.1	Digital water (I)	**Yes**
	C25	2	6.1 ± 0.9	Digital empty tube (I)	
n°7	**C21**	6	18.1 ± 10.5	Digital ova (I)	
	C22	4	4.8 ± 2.7	Digital water (I)	**Yes**
	C23	4	4.8 ± 3.3	Digital water (I)	
n°8	**C18**	8	4.4 ± 1.1	Digital water (I)	
	C19	3	18.3 ± 6.3	Digital ova (I)	**Yes**
	C20	7	4.4 ± 1.8	Digital water (I)	

Table 15.2. "Chicago experiments" of August-September 1996.
These experiments were performed by sending the recordings via Internet. The effects of the recordings were assessed from August 27th to September 17th, 1996. For the recordings C2-C5 and C4-C6, the codes were known: in a first time, "unexpected" results had been noticed; after new open-label testings, "expected" results had then been obtained. Out of 6 blind experiments, 6 active recordings were successful (chance only would have allowed finding approximately 2.5 active recordings on average). If we consider all Chicago experiments (results of this table and those of Table 15.2), we find 14 successes out of 19 blind experiments whereas chance only would allow guessing 9. The difference is not statistically significant (moreover re-test of samples after "unexpected" results introduced an important bias. But – and it is what remains incomprehensible – whether or not the "correct" codes was found, consistent changes of the coronary flow occurred.
As for the previous table, the experiments which were included in the communication at the congress of San Francisco (see text) are in bold characters.

Anyway, J. Benveniste had now a series of "correct" experiments that would allow him communicating on "digital biology". He did not then hesitate to speak about 29 out of 29 successful experiments, forgetting incidentally the failures (even if *a posteriori* explanations were proposed) and the delivery with forceps of some results. Indeed, here is how he presented these results afterward:

> "In a few months, during summer 1996, we performed twenty seven of these blind experiments. Twenty seven times, I succeeded in determining if the signal was coming from a tube informed by ovalbumin or acetylcholine or from a tube of deionized water". [6]

As we saw, the reality was less obvious (apart from the error on "27" experiments). It is necessary to note furthermore that there was not about 29 independent successes because the computer files were recorded and tested as pairs (or triplets) always containing a single recording supposed to be active. Therefore it would be more exact to speak about 13 blind experiments including 29 measurements of activities (experiments in bold characters in Tables 15.1 and 15.2). Furthermore, if we consider all the experiments performed from April to September, the "correct" answer was thus found for 14 experiments among 19 analyzable blind experiments. Chance only could allow finding approximately a mean of 9 correct results. Overall, the calculation shows that the number of "successes" is included in chance fluctuations. Nevertheless, these results remain out of the ordinary and inexplicable since, "informed" or not, it was always the same water which irrigated the heart.

The fact that J. Benveniste persisted in not giving the identity of this American co-worker before the congress of San Francisco irritated some scientists because it was not in accordance with current practice in scientific and university milieu and because this attitude allowed all kinds of suppositions:

> "When they received the summary of a paper which will be presented next February to the congress of immunology of San Francisco, the statistician Alfred Spira and the physicist Claude Hennion reacted in a bad mood. Benveniste masked the name of the professor of Chicago associated to this transmission of electromagnetic signals via the Internet network. "In thirty years, I have never been treated in such a manner, Alfred Spira admits, hurted. How to believe what he asserts if he hides a signatory of the text?" Claude Hennion sees there a confirmation: "Benveniste demonstrates that he puts himself outside science." Benveniste is overwhelmed: "why would I expose to the knocks anybody honorable who agrees to participate in my research?" " [7]

The anonymity of the "Masked Researcher" was finally lifted during the congress of San Francisco at the end of February 1997, but this revelation and the communication made by J. Benveniste at the congress in the form of a "poster" were done in an almost complete indifference.

Notes of end of chapter

[1] J. Benveniste, P. Jurgens, W. Hsueh, J. Aïssa. Transatlantic transfer of digitized antigen signal by telephone link. *Journal of Allergy and Clinical Immunology* 1997; 99: S175.

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

[3] E. Fottorino. La mémoire de l'eau. Une vérité hautement diluée. *Le Monde*, January 23rd, 1997.

[4] Letter of W. Hsueh to J. Benveniste on June 21st, 1996.

[5] Letter of J. Benveniste "to the participants in transmission experiments" of July 4th, 1996.

[6] J. Benveniste. Ma vérité sur la mémoire de l'eau, p. 175.

[7] E. Fottorino. La mémoire de l'eau. Une vérité hautement diluée. *Le Monde*, January 23rd, 1997.