

Chapter 16. "It is the same girl, still as beautiful"

"At this moment the Unicorn sauntered by them, with his hands in his pockets. 'I had the best of it this time!' he said."

Lewis Carroll. *Through the looking glass.*

As Russian dolls, which are placed one inside the other, each of the "crucial" demonstrations that J. Benveniste hoped definitive to convince the scientific community resulted inevitably in a new "nested experiment". Constrained to a permanent headlong rush in the quest of the experience that would be convincing for all, J. Benveniste dreamed about the last Russian doll which still remained inaccessible. Thus, after the investigation of *Nature*, he intended to demonstrate that in strictly blinding conditions while maintaining acceptable experimental conditions, the effect of high dilutions persisted.

The opportunity to wipe out the disastrous consequences of the *Nature* gate came from Alfred Spira, an epidemiologist, director of the Unit 292 of Inserm. The latter, during summer 1988, wrote to J. Benveniste to express his support after the "mockery of evaluation" of *Nature* because, he wrote, "our survival as researchers with scientific ethics is at stake".¹

He then confirmed this "declaration of faith" one year later in an open forum of *Le Monde* – a few days after the decision of P. Lazar concerning the Unit 200 and his director – where he explained the reasons of his commitment with J. Benveniste:

"The results on the high dilutions are inexplicable? Let us try to explain them! The researchers made a mistake, we have been deceived? Let us give ourselves the means to show it! [...] Personally, this is what I decided to do since a year. It is necessary to clarify the problem we are faced with, which is the possible transmission of information by non molecular supports. [...]"

Scientific errors are more frequent than big discoveries and maybe we are confronted once again with error. It is not in the logic of the research to give up a problem at the middle of a crossing point [...] I will thus continue to work with Jacques Benveniste as long as we will have not demonstrated that his results are false or exact."²

Owing to the rarity of such a public statement and personal commitment, J. Benveniste could not neglect them. Another important point is that P. Lazar, the director of Inserm, and A. Spira are both epidemiologists having belonged to the same "school", that of Daniel Schwartz who introduced the use of statistics in the area of health and biological sciences in France in the 50s. The direction of Inserm can only favourably look – if it did not arouse or at least encourage – this collaboration between U200 and U292 which could allow forming an opinion, peacefully, on the reality of the controversial results. A. Spira took nevertheless precautionary measures and he asked to check his work by a statistician from Inserm who remained in the shadows.

This conjunction of interests led to the collaboration for the design of an experimental protocol. This new attempt of reproduction of the effects of high dilutions should be carried out without methodological criticisms and must take into account the lessons of the past. A protocol of 23 pages minutely describing step by step the experiments was drafted. This new expertise was aimed to be diametrically opposite of the investigation of *Nature*. Thus, the blind procedure was systematic. A. Spira thought for a moment to call for a bailiff and then gave up preferring to consider his participation as a normal scientific collaboration.

The protocol planned to define *a priori* quality criteria before including each experiment in the statistical analysis: percentage of degranulation above a given value for the first peak indicating that basophils were reactive enough, absence of important variation between controls, etc. Let us insist once more that these quality criteria were applied before unblinding and statistical analysis. The selection of the experiments was hard to understand for the investigators of *Nature*. For them, this selection was leaving the door open to all manipulations. It was in fact a misunderstanding of the rules of the scientific methodology and this method was simply a quality control as practiced in numerous industrial sectors. Let us imagine a racing driver who notices before the departure of a Grand Prix that the engine of its car falters. Nobody thinks of blaming him for preferring to use another car whose engine is properly running and which is planned to prevent the breakdowns of the first one.

On the field in Clamart, Béatrice Ducot from the Unit U292 was in charge to blind and supervise the experiments which were performed by E. Davenas and by a newcomer, Sylvie Gonnord, trained to the technique of basophil degranulation. The experiments were performed from September to December 1989.

At the end of December 1989, the first results of the statistical analysis began to leak out. J. Benveniste declared then:

“Everything suggests that the trial led with Spira is going to be super positive. Once again, we find again the results published in *Nature*. It is the same girl, still as beautiful. She lacks just a little makeup. Beautiful and faithful, it is rare.”³

This information was confirmed – in a less flowery language – by A. Spira in January 1990:

“We reproduced the results published in *Nature*. Our work thus answers to the methodological arguments of the investigation of July 1988. As far as we can, the results of Benveniste cannot be explained by a rough experimental bias. If the team worked haphazardly, we would have noticed. In substance, all that I may assert is that, in the conditions of the laboratory of Clamart, the phenomenon exists. We did not prove that he does not exist. Now, it would be necessary to work on other models, in other places.”

And what about the famous error of sampling? A. Spira noticed that actually, the variability between the countings hardly varied:

“It is surprising, unusual. It cannot be related to a bias of observation, because everything was performed blind. Too big variations could always be understandable by an external factor. Here, I do not understand”.

Were the results as “super positive” as stated by J. Benveniste? Why this small reservation in his statements when he said that “she lacks just a little makeup”?

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Notes of end of chapter

¹ Letter of A. Spira to J. Benveniste of August 15th, 1988.

² A. Spira. Recherche et Vérité. *Le Monde*, July 13th, 1989.

³ M. de Pracontal. Les mystères de la mémoire de l'eau, p. 200.