

Chapter 9. “A report the conclusion of which would be: magic is true”

The trio enters the track

As soon as the article was published, it was necessary to get ready for the arrival of the investigators. But, as we have already said, there was no particular anxiety about this visit. Yet, to say the least, the composition of the trio was not neutral.

At the time of the survey, John Royden Maddox was 62-year-old. Of Welsh origin, he was a physicist and a chemist. He taught theoretical physics at the University of Manchester during six years from 1949 to 1955. Then he left the university to manage the science column of *Manchester Guardian* from 1955 to 1964. He became Director of *Nature* from 1966 to 1973. He then delegated the direction to manage the Nuffield foundation which financed research projects intended to promote education. He resumed his director's position of *Nature* from 1980 and onwards. However, when he got his former functions back, the writers of *Nature* distrusted him and transmitted a petition to the direction of the journal so that J. Maddox would not be involved in the management of the scientific manuscripts. The latter explained this attitude in the following manner: “They were extraordinarily on their guard to see me come back [...] because I had acquired, not without any reason, the reputation of being stubborn, somebody who was determined, but also unpredictable.”¹

For his part, J. Randi, born Randall Zwinge in Toronto, was 60 years old in 1988. He was a very well-known stage magician in the Anglo-American world. Since the 50s, he participated in very popular television programs in the United States. He acquired an international fame in the 1970s when he accused Uri Geller to use conjurer's tricks “to twist teaspoons”. Especially, J. Randi was a founder member of the CSICOP (*Committee for Scientific Investigation of Claims of the Paranormal*). It is an association of “skeptics”, which is dedicated to demystifying and denouncing individuals who claim to be endowed with paranormal powers. J. Randi wrote in particular several books to fight popular beliefs concerning paranormal. One of his favorite targets was parapsychology, especially when performed in universities because one of his favorite theses was that scientists were very easy to fool. What he considered as one of his most great success was the “project alpha”. This project consisted in introducing two of his magician stooges within a university team which experimented in the field of parapsychology. This team had received an important legacy in 1979 to evidence paranormal effects (such as psychokinesis and telepathy). The team thus recruited individuals claiming to have unusual capacities. During several years,

both stooges of Randi who had succeeded to be selected were themselves particularly "competent" and the studies focused on them. Therefore, they made people believe they had "powers" although they used methods of conjurers and stage magicians. The mystification was revealed in 1983.

As for W. Stewart, then 43-year-old, he owed his celebrity for his investigations in several affairs of scientific misconduct. Chemist and physicist by training, researcher at the NIH, he had nevertheless no doctorate. With his boss N. Feder, he specialized in revealing frauds of other scientists: "on the campus of the NIH at Bethesda, in Maryland, where he shares a tiny office with his friend Ned Feder, his name arouses disgusted or negative reactions: he is the "informer", "the one who bites the hand that feeds him", "a bastard who strikes a blow at the credibility of science" and "tarnishes the scientific community." ²

The first affair which made W. Stewart famous was the case of the scotophobine in 1972. This biological factor was supposed to transmit learning from a rat to another one, namely the fear of darkness. W. Stewart reported that the way of selecting data (among other criticisms) was responsible for this "discovery". The article and its refutation by W. Stewart were simultaneously published in *Nature*. Another famous affair in which W. Stewart and N. Feder were involved was the case Darsee, named after a cardiologist of Boston who produced an impressive amount of experimental data with which he drafted articles, some being published in scientific first-level journals. The affair burst in 1981 and was the occasion, beyond this case of obvious fraud, to question the system of "peer review" which had missed numerous discordant results and obvious errors. In 1988, at the time of the present story, W. Stewart struggled with the Baltimore case, an extremely complex history in which the Nobel prize laureate D. Baltimore was accused of having covered made-up data. The affair gained considerable importance with hearings organized by a member of parliament, Senator John Dingell. Several committees of inquiry later, D. Baltimore as well as the researcher in cause were finally acquitted in 1996.

As the three musketeers of Alexandre Dumas, the investigators were actually four. A young man named José Alvarez accompanied J. Randi. His arrival in the laboratory of J. Benveniste had not been announced by J. Maddox who nevertheless managed the survey. The exact role of J. Alvarez during the survey remained obscure. J. Randi presented him as an assistant to whom he "taught the job". We must recognize that he did not disturb the team. Apparently in no hurry to perfect his apprenticeship, he spent the early stages of the survey sleeping in a corner of the laboratory, probably as a consequence of jet lag. Afterward, we saw him only occasionally.

In fact, José Alvarez, 19-year-old, was a friend of J. Randi and was an artist performance in Plantation in Florida, the city where J. Randi resided. With the help of this latter, J. Alvarez became famous the same year in 1988 in Australia. Indeed, at the request of an Australian television channel, J. Randi trained J. Alvarez to play the role of a “medium” named Carlos supposed to be in communication with a spirit having lived several thousand years before. The purpose was to estimate the degree of credulity of media and public. A press kit was made including numerous indications which should have put on the track of the trickery if a simple investigation had been made on the so-called medium. This one was the subject of numerous articles in press, radio and Australian television. The trickery peaked with the gathering of numerous “believers” in a room of the Opera of Sydney on February 21st, 1988. One week later, the mystification was revealed during the television program which had sponsored this “performance”.

If the coming of a “real false medium” in the laboratory of Clamart had been known at that time, it would probably have been the occasion of numerous jokes in the press which already ridiculed the presence of a “magician”. Especially, it would certainly have dealt with a severe blow to the seriousness of the “performance” organized by *Nature*. The impression of a “circus” atmosphere, which will be reproached after the visit, would have been considerably strengthened. In spite of this risk, it is surprising that J. Maddox authorized J. Randi to come accompanied with his friend. But maybe J. Maddox too did not know the recent exploits of the latter.³

The team of Clamart, eventhough they understood the profiles of the investigators a little bit better, was however not conscious that the curriculum vitae of the latter were so “heavy”. Naively, thinking that it participated in a scientific controversy where each participant was supposed to be honest and open to the opposite arguments, the researchers of Inserm U200 understood only afterward that the investigators could not return empty-handed from their trip to Clamart. Their honor was at stake. They must return from their expedition with a new trophy to add to their collection.

The last details concerning the arrival of the investigators were quickly set at the end of June. Christian Boiron himself as CEO of Boiron Laboratories sent a fax to the investigators to announce them his invitation “to study the scientific results on high dilutions carried out at the Inserm Unit 200 in Clamart.”⁴ The fact that Boiron Laboratories – first world manufacturer of homeopathic products – financed their stay did not apparently disturb the investigators. In the investigation report, J. Maddox recognized that hotel expenses had been actually paid by these laboratories. In his defense, we must acknowledge – as the developments of this text will show – that the financing of their stay by

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manufacturers of homeopathy did not influence their conclusions in a direction favorable to homeopathy! However, in the investigation report, they pretended to have discovered during their stay at Clamart that homeopathic laboratories had participated in the financing of the research of J. Benveniste.⁵

The narrative of the week

Large extracts of the internal report of Inserm U200 that Elisabeth Davenas drafted immediately after the departure of the "guests" will serve us as common thread. Sometimes transcribed in telegraphic style, this document gives nevertheless an idea of the atmosphere during this week and especially allows understanding the sequence of the experiments. Finally, in order to allow the reader to understand the various experiments performed during the week, Table 9.1 summarizes the characteristics of each experiment that was commented in the investigation report of *Nature*. The reader can refer to it in the course of reading.

Although he was not present during the famous week (he nevertheless interviewed the various protagonists afterward), the journalist M. de Pracontal described well the general atmosphere of this week:

"One imagines the atmosphere: Stewart with the finesse of a big hamburger and approximately so quiet as an aviary of parakeets, overexcited at the idea of letting a clue escape; Randi who for understandable reasons has the right to touch nothing, but who watches everything with an eye of lynx; Maddox, very phlegmatic, very British, observing the advancement of the operations as if he was a simple spectator; and Benveniste, furious of seeing that he is not at home any more in his own laboratory."⁶

Let us therefore begin the chronological narrative of this week. We remind that the protagonist belonging to the laboratory of Clamart and named "Francis" by E. Davenas is the author of the present book. We will comment the investigation in Chapters 10 to 13.

N° exp	Blood donor	Series of anti-IgE at high dilutions	Day of preparation of the experiment	Day of basophil counting	Counting by :	Comments
A	Hospital	Series n°1 of Monday	Monday (open-label)	Tuesday afternoon (open-label)	ED	Coagulation issue
B	Hospital	Series n°1 of Monday	Monday (open-label)	Tuesday afternoon (open-label)	ED	
C	Lab (BP)	Series n°2 of Tuesday	Tuesday (open-label)	Tuesday evening (open-label)	ED	
D	Lab (K)	Series n°2 of Tuesday	Tuesday (open-label)	Wednesday afternoon (blind)	ED	
E	Hospital	Series n°3 of Wednesday	Wednesday (blind)	Thursday morning (blind)	ED + FB	
F	Hospital	Series n°3 of Wednesday	Wednesday (blind)	Thursday afternoon and evening (blind)	ED + FB	Serious problem: number of cells (leucocytes) very different from one count to the other.
G	Lab (BP)	Series n°3 of Wednesday	Wednesday (blind)	Friday morning (blind)	ED + FB	

Table 9.1. Summary of the characteristics of the 7 experiments (from A to G) that were performed from July 4th to 8th during the investigation of *Nature*. The reader can refer to it in the course of reading.

Monday 4th, July

« In the morning: explanation of the experimental process to W. Stewart.

Afternoon: realization of two experiments, under control of W. Stewart.

[E. Davenas describes the preparation of high dilutions and cells from two different donors as well as the experiment itself]

6) Stop of the reaction [...] Refusal of the experts to seal both plates or to sign on the adhesive tape.

7) Counting on the next afternoon, open-label, Stewart neglecting the possibility of blind counting in spite of our request.”⁷

Tuesday 5th, July

"In the morning: realization of two new anti-IgE experiments with 2 bloods and new series of anti-IgE dilutions.

1) Blood n°1: 20 ml of blood (Bernard Poitevin) collected on 4/7/88 in the evening by Corinne [...]

2) Blood n°2: 20 ml of blood (Karine, trainee) collected on 5/7/88 in the morning by Corinne. Allergic to some drugs. [...]

3) Realization of a new series of anti-IgE dilutions, under the control of J. Maddox and, from time to time, J. Randi. [...]

5) Counting: blood n°1 (Bernard), open-label, on the evening; blood n°2 (Karine), blind, on the next evening. [...]

Comments:

- During these 2 experiments performed on Tuesday morning, Stewart made statistical analyses on the results obtained previously. J. Maddox controlled the process of the experiment. [...]

- Stewart asks me to count all the experiments performed between Monday and Tuesday but does not want that I count them blind → I count 2 experiments performed on Monday and the experiment n°1 (Bernard) performed on the morning.

The experiment n°2 (Karine) will be counted blind the next day (on Wednesday, July 6th): J. Maddox and W. Stewart did not want to seal the plate which stayed one night at 4°C; it is Stewart who put down the content under the slides of Fuchs [=hemocytometer] after I showed him how to do, namely to shake several times (but slowly) to re-suspend and not put down several times from the same well at the risk of obtaining erratic counts (no more twice). One must also pay attention to wash the Fuchs slides between each count.

Afternoon of Tuesday, July 5th, 88 (→ 10 p.m.): counting of 2 experiments performed on Monday, July 4th, 88. Counting of the experiment n°3 (Bernard) performed on Tuesday morning, July 5th, 88."

Basophils of three experiments were counted on Tuesday (until very late in the evening). The results are shown in Figure 9.1. E. Davenas noted about the first experiment: "blood with rather low degranulation even with strong concentrations. Blood n°1 was the one with microagglutination during cell wash". Indeed, for the first experiment, the profile of degranulation was rather chaotic and the first peak reached not very high degranulation percentages. The second and third experiments (experiments B and C) on the other hand were more satisfactory and corresponded to quite typical effects with high dilutions (Figure 9.2).

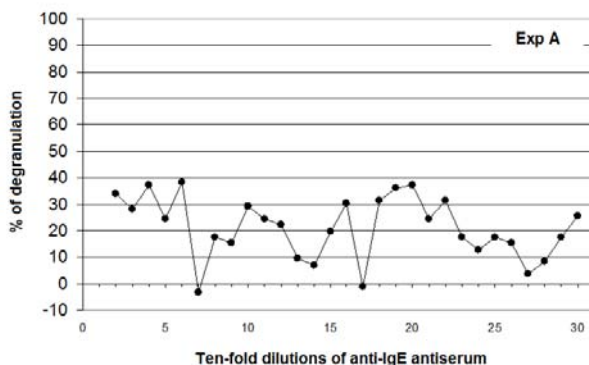


Figure 9.1. The first experiment (experiment A) performed open-label was not a complete success. There were both weak degranulation of basophils with low dilutions of anti-IgE and an unusual profile with high dilutions, probably due to “microagglutination” of cells. This problem generally occurred when the anticoagulant, which was added to blood to prevent coagulation, was inefficient (for example, because the tube had not been returned after blood sampling to favor the mixture of the anticoagulant with blood).

“On Tuesday evening, while I am counting the second experiment, Jacques tells me that the next day Stewart wants that I make 3 whole experiments with dilutions of anti-IgE from 1×10^2 to 1×10^{30} (3 different bloods). With anti-IgE completely coded. The reading will also be coded.

I protest because it is far too much. What is the point of counting, alone, the first 4 experiments: why they do not want that I count blind as we ask them?! Then, they will not want to take into account the results.

At the time, I refuse to count the 3rd experiment. Finally, I do it, Jacques tells me that we cannot refuse what the experts want at the risk of appearing “to hide” something.

I thus count but I propose that the next day I count the 4th experiment (at least!) blind and that the 3 experiments be performed not on the entire anti-IgE range, either only on a part (for example: from 10^{20} à 10^{30}) or 2 duplicate experiments on a part of the range. But not three on the entire range!

But Stewart refuses. He has said. We have to do the way he wants to. He also always refused to make an experiment where we test an anti-IgG range versus anti-IgE”.

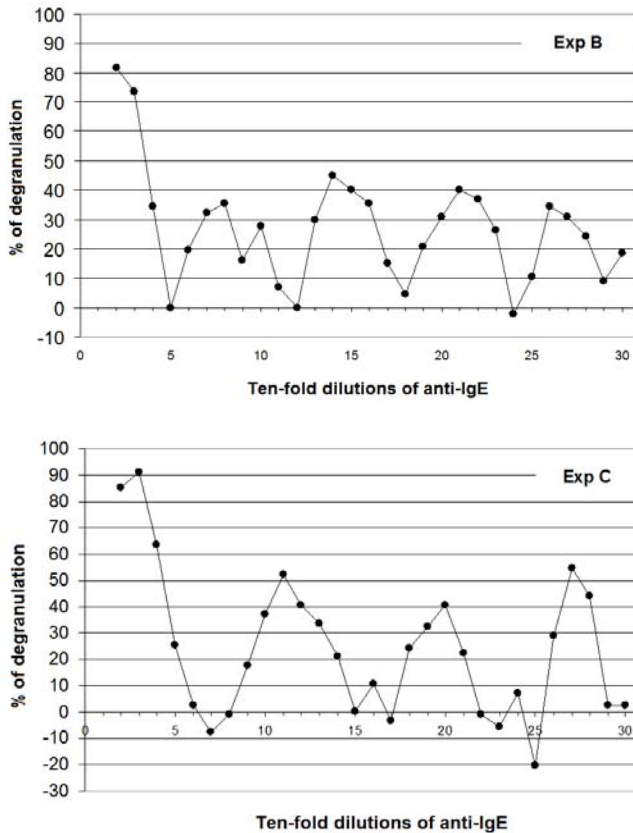


Figure 9.2. The experiments B and C were completely in accordance with the expected results. An effect of anti-IgE with high dilutions was obtained with "waves" of degranulation after the first classic peak.

Wednesday 6th, July

"Realisation of 3 experiments under constant control. [...]"

1) Blood.

Given the good result obtained with Bernard's blood (3rd experiment), W. Stewart asks to take again his blood. [...] Two bloods come from Bécère [hospital] [...].

2) Realization of dilution range and coding.

While Randi and his partner prepare a randomization of 5 ml plastic tubes + green corks in Francis' office in the new building, I perform the anti-IgE range in my lab, under the control and with the help of Stewart. I brought new 5 ml tubes, new corks, new tips ... I do not have the right to touch tubes, tips, etc.

Stewart places and numbers himself tubes from 2 to 30 on a sample rack. For each dilution, he gives me the tube and takes back the previous tube and plug it. I do 10-fold dilutions of anti-IgE with [...] duration of rotating mixer = 15 sec. Tubes are plugged with orange corks.

At the end of the dilutions (from 1×10^2 to 1×10^{30}), I point out that it is necessary to add controls [...]. Thus, Stewart adds 5 tubes numbered from 31 to 35 corresponding to controls.

When the dilutions are done, I take them and go down the stairs with Stewart and Bernard Poitevin in Francis' office where J. Randi, his partner and J. Maddox are.

The tubes in which the dilutions will be transferred are on a sample rack hidden by a sheet under the eye of a camera which recorded the previous randomization and which will record the coding.

I sign, with W. Stewart, the sheet where the code will be noted and I leave the room having left the dilutions – under the eye of the camera and the only experts. Jacques does not have the right to approach this door (to see if everything is according to the rules...). Only the experts know the code.

It is Ruth (in my absence → lunch) who brings back the dilutions in my lab with Stewart, again under the eye of the camera. The tubes have now green corks.

The code, placed in a scotch-taped and signed envelope is stuck on the ceiling of the lab by Stewart so that nobody touches it!

3) Realization of the experiments

Stewart stays permanently in my lab to watch the dilutions when I go away to centrifuge the bloods [...]. After 30 min of incubation (Stewart stayed in the lab permanently), one stops the reaction by adding 90 μ l of staining agent with the multichannel pipette.

Three plates are blocked with adhesive tape, numbered from 1 to 3 and placed in a white polystyrene box with a lid.

This box is closed by Randi with an English newspaper and with adhesive tape; under the eye of the camera, one records the "result" of the operation from every angle. The box is put in cold room until the next day [...]"⁸

The blind counting of basophils for experiment n°4 was then performed. About this experiment E. Davenas noted:

"This experiment was counted in blind conditions: W. Stewart put down [samples] in chambers. He forgot the dilution 1×10^5 . Some wells were counted as duplicates. However, if one compares with my own worksheet, one notices that 3 counts did not match with any dilution (C; D; CC; cf. photocopy). There are wells for which he was not sure: does it correspond to these counts? One will not know because Stewart left with the counts, the code, the calculations...! [...]"

The relationship between the 39 blind counts of E. Davenas and their report by W. Stewart after unblinding deserves to be described in detail (several counts can correspond to the same dilution). These data are described in Table 9.2. One notices that three accounts are missing. Indeed, W. Stewart became muddled with the codes and the lists of counts. He was unable to say to which dilutions corresponded these three accounts. Relatively to the 39 counts, it was considerable coming from an "expert" who was supposed to control the quality of the work of researchers.

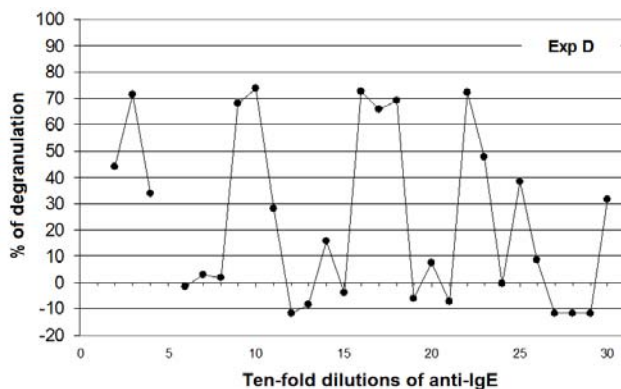


Figure 9.3. These results (experiment D) played a central role in the investigation report published one month after the inquiry. The investigators criticized the high percentages of degranulation (70%) and the fact that this positive result was obtained after a blind counting of basophils whereas the preparation of the experience was open-label (even if this preparation had been made under constant surveillance). But, W. Stewart made errors when preparing the counting chambers and for 3 counts of basophiles, he was unable to associate the corresponding well.

On the evening of July 6th, in spite of the errors due to the inattentiveness of W. Stewart, a discussion took place after the unblinding of the 4th experiment. Indeed, in spite of the errors of the latter, the success of the experiment annoyed the three investigators very much:

"Following the results obtained for the 4th experiment, there was a rather "hard" discussion with J. Maddox, J. Randi, W. Stewart, Jacques and me on Wednesday, July 6th in the evening.

- The experts recognize that they are amazed by the reproducibility of the counts in duplicate.

- They are amazed (with us) by a so high degranulation with high dilutions (may be due to the fact that Karine is allergic to some drugs: basophil hypersensitivity).

- Rather vigorous discussion about "sampling error". After reading my laboratory notebooks they do not see any note about this "sampling error" which we can expect: variability of test counts. Is the presentation of results correct? Should "negative" degranulation be reported? What is the limit of significance? Is each count not associated with the same 20% of error? They admit that it does fit with the 4th blind experiment but they do not want to take it into account because only the counting was done blind – they forget that J. Maddox watched me experimenting – they thus wait for the results of experiments done on Wednesday with all the possible and conceivable rigour [...]

- They also blame for the "too good" results of Israel and overall the "too beautiful" results reported in my lab books [...]"

N° of counting	Number of basophils	Corresponding dilution	N° of counting	Number of basophils	Corresponding dilution
A	30	10 ⁻¹⁷	U	97	Témoïn
B	58	10 ⁻⁴	V	49	10 ⁻²
C	84	UNKNOWN	W	98	10 ⁻²⁹
D	17	UNKNOWN	X	23	10 ⁻¹⁰
E	21	10 ⁻²²	Y	Not counted	
F	85	10 ⁻⁷	Z	91	10 ⁻¹⁵
G	63	10 ⁻¹¹	AA	81	10 ⁻²⁰
H	88	Control	BB	98	10 ⁻²⁸
I	63	10 ⁻¹¹	CC	27	UNKNOWN
J	93	10 ⁻¹⁹	DD	60	10 ⁻³⁰
K	94	10 ⁻²¹	EE	84	10 ⁻⁶
L	78	Control	FF	76	10 ⁻²⁶
M	94	10 ⁻⁶	GG	27	10 ⁻¹⁸
N	46	10 ⁻²³	HH	98	10 ⁻²⁷
O	84	10 ⁻²⁶	II	95	10 ⁻¹³
P	98	10 ⁻¹²	JJ	24	10 ⁻¹⁶
Q	25	10 ⁻³	KK	54	10 ⁻²⁵
R	28	10 ⁻²²	LL	88	10 ⁻²⁴
S	86	10 ⁻⁸	MM	29	10 ⁻⁹
T	74	10 ⁻¹⁴	NN	27	10 ⁻⁹

Table 9.2. This table presents raw data for experiment D shown in Figure 9.3. We note the absence of identification of 3 counts C, D and DC due to errors of W. Stewart.

Thursday 7th, July

"Counting of the two experiments performed on 6/7/88.

Protocol established by Stewart:

1) We will be two for the counting: Francis and me. Each one with a series of chambers (that implies 2, 3 even 4 pipettings in wells when these latter are counted in duplicate... It is too much for a well and can entail an erratic count ... (I had said it to Stewart, he does not want to take it into account).

Francis and me must not speak and nobody can see us or speak to us. Even – especially – Jacques.

2) It is Stewart who puts down the contents of wells in the chambers of Fuchs, under Corinne's eye, in the lab room near this one where we count. W. Stewart shakes with a 100- μ l pipette and puts down exactly 15 μ l under slides with another pipette [...]. He brings us chambers while we are counting → No pause. We are sometimes obliged to tell him to slow down because chambers dry or blush by waiting for such a long time.

From 10 a.m. to 2 p.m.: 56 counts corresponding to plate X [...] There were overall 35 wells → Most of them were counted in duplicate (56 counts).

From 4 p.m. to 10 p.m.: 72 counts corresponding to plate Y. [...] Counting very difficult, pale basophils, poor preparation, different cell densities (up to 2:1 ratio) according to chambers (we have pointed out it and showed to Stewart and Maddox). All wells were nevertheless counted in duplicate.

The counting was too long and too painful. The plate stayed all afternoon and all evening long outside. We should have stopped counting, Francis and me. It was

useless and tiring. Furthermore, given the cell preparation, it was obvious that we could say nothing, conclude nothing with this experiment."

At the risk of laboring the point, I add that I actually disturbed J. Maddox who, posted in the entrance of the room, killed time by darkening sheets with mathematical calculations of integrals. I had then made him notice the huge differences of cell densities from one well to the other one, what invalidated the experiment. He had then pointed out the fact to W. Stewart who did not deny the problem. They told me to record my comment on the counting worksheet so that it would be taken into account at the time of the analysis. Why should we then continue in these poor experimental conditions? I was told that these results would be nevertheless "useful for statistics".⁹ We will see how these remarks have been taken into account.

Friday 8th, July

"Counting of the 3rd experiment prepared on Wednesday, July 6th.

Given the time spent (and lost) counting the experiment n°2, the last experiment is counted on Friday morning.

In agreement with Jacques and J. Maddox, we refuse to count more than 40 wells (the experiment has 32 wells).

The same protocol is thus set up. For us the silence, for the others, the magic tricks of Randi. As for the previous evening.

For this experiment: 40 counts corresponding to the plate Z (= plate n°3 = Bernard). At the end of counting, as we count faster [...], W. Stewart suggests us counting the other wells so that there would be more duplicates. We refuse. [...].

Unblinding - Discussion - Results

When the countings were finished, W. Stewart makes us sign our worksheet of counts, Francis and me. Corinne also signs the worksheet which records the numbering of the wells during the blind counting. W. Stewart and J. Randi also sign.

We go downstairs in meeting room for a first assessment of the results before the opening of the code. In other words, to try and guess where controls, low and high dilutions could be [...]. We come back to get the code stuck on the ceiling with great ceremony. W. Stewart climbs on the ladder to unstuck the envelope. He is the only one who is authorized to make it. All this is filmed by Jacques and Randi. It is me who bring back the envelope downstairs (duly accompanied).

Then the great process of envelope opening with Randi, the Grand Master on the subject. It lasts 20 min overall (perhaps more). At first inspection, the brown adhesive tape which stuck the code on the ceiling is not completely as one would expect, what makes thinking that, suggests that ... Finally, we move on to the next stage: inspection of the transparent adhesive tape signed by the 3 experts, apparently everything is well. Randi wants to open the envelope without unsticking or tearing it but by cutting it with scissors → I go back up to the 2nd floor to take my scissors. After my return, Randi cuts the envelope in its right extremity, extracts delicately the content, which is the code folded in a aluminum sheet which is neither wrinkled nor torn → finally, nobody touched the code.

Randi extracts finally the worksheet of code... We are lastly about to decode... He is going to open the worksheet (folded in 4)... No! we read before a series of notes (that I had written to Jacques) concerning what I considered improper in these experiments. I have no more this sheet. The experts took it. From what I remember, there were remarks on the repeated pipetting in wells that induced errors [...] I pointed out that we could not take into account the experiment counted on the previous evening because, with cell densities so different from a chamber to the other one, it was impossible and even erroneous to make an interpretation of these results.

When we realize that W. Stewart filled not only my Fuchs but also those of Francis, and that Randi made his magic tricks during all this time... This leads dreaming... One wonders moreover with what right Randi signed the sheet of transcription of the code of countings that have been filled by Corinne and W. Stewart...

I agree for coding the tubes of dilutions, but not again the counting. Otherwise it would have been necessary that Corinne put down the contents of wells in the chambers of Fuchs. Indeed, finally, W. Stewart knew not only the code of the dilutions but also the code of the counts... And I would not be amazed if he knew the codes given his extraordinary capacity of mental calculation and to remember numbers...

It seems that they decided to code the counting when, on Thursday morning they found – dixit Randi – that the code in the ceiling maybe had been touched. It is not true because the previous evening – in the hotel of the experts during the cocktail – it had already been decided that Stewart would fill the chambers with the help of Corinne. Moreover Jacques called Corinne to prevent her.

On the other hand what was decided at the last moment was the participation of Francis for counting. We learnt that only on Thursday morning.

We learnt later also that, finally, they did not transfer my dilutions in new tubes as they had said but that they had only changed the orange corks with green corks and had erased the *[numbers of]* dilutions with alcohol and stuck labels with the code number (they filmed this episode). This was done so that we cannot, in case of negative results, impute the failure to the transfer [...].

We arrive finally at the unblinding. While Jacques reads, Stewart transcribes. There are so many numbers – counts of Francis and mine – that it is really difficult to analyze everything at first glance but what appears immediately is:

1) Very heterogeneous controls [...]

2) Very poor duplicate counts, while those of Wednesday evening for my 4th experiment, open-label for preparation but blind for counting were perfect.

3) Some discordance for some wells between my counts and those of Francis.

During the heated discussion between the various interlocutors (J. Maddox, W. Stewart, J. Randi, Jacques, Bernard, Francis and me), Randi and Stewart photocopy the codes, but extraordinarily when they leave (finally) late in the afternoon at top speed, we have no document! Jacques must retrieve the results to the hotel, at midnight, when Stewart is still there.

(My last 2 laboratory notebooks n°4 and n°5 will be got back the next day after departure of Stewart who wanted to study them again during the night although he took all photocopies – with the photocopy of the first laboratory notebooks)."

During the discussion, the main reproaches of the investigators concerned mainly the lack of statistical studies, the reproducibility of the experiments and the lack of objectivity of the experimenter that counted basophils ("does Jacques trust his collaborators?", asked J. Maddox). According to them, odd results obtained in a single system did not allow claiming such conclusions.

Furthermore, the investigators questioned the results obtained in Israel by E. Davenas. Indeed, as recorded by E. Davenas, the investigators asked about the experiments performed in the Israeli laboratories:

"Who made them? How was made the blinding? If there is no blind counting, it is possible to recognize tubes if they were marked said Randi! That takes the cake! I made dilutions in sterile conditions, from new tubes, unwrapped, under the supervision of several persons. On the other hand, in Israel, there was a double coding of tubes by 2 groups of 2 persons, therefore nobody knew. Furthermore we worked only on a part of the range and every dilution was tested in triplicate (3 wells for every tube). There were thus true triplicate counts. Here there was only a simple code that the experts were the only to know [...]. On the other hand there were no true duplicate counts but a double counting of the same well".

The issue of the multiple countings in the same well is an important point. Indeed, we knew from our own experience that they could lead to erratic counts because there were small volumes of cell suspension in each well.

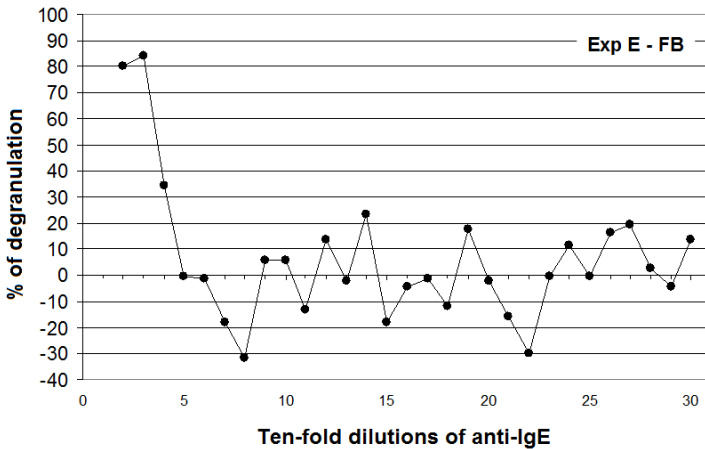


Figure 9.4. In experiment E, both preparation and counting of basophils were done blind in contrast with experiments B and C. Two experimenters – ED and FB – counted basophils. The experiment was not conclusive.

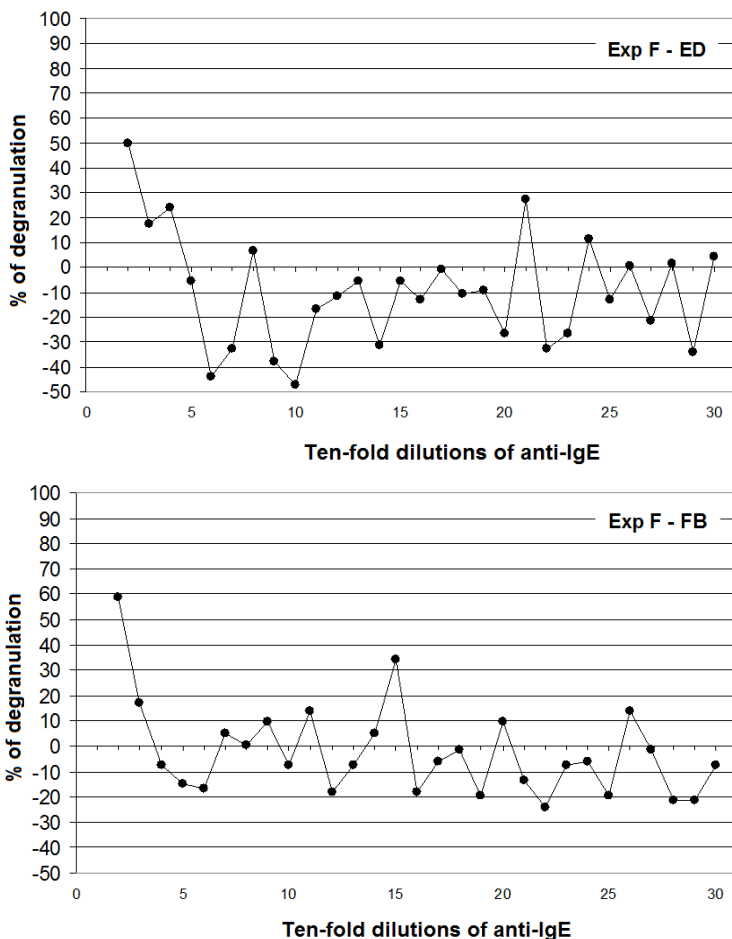


Figure 9.5. The experiment F played (as experiment D) an important role in the investigation report of the investigators. Indeed, each of the experimental points was counted in duplicate. Moreover, the same experiment was counted by both experimenters. We will see in the text how this failed experiment was exploited by the investigators. They ignored an issue despite repeated remarks during counting (and recorded in writing): the cell density varied in an unusual way from one count to the other one. Note on this matter the high negative percentages of degranulation what is completely extravagant. Several reasons could explain these poor results: repeated pipetting in the same well, poor technique for putting down the cell suspension into counting chambers (let us remind that this stage was performed by W. Stewart). A detailed analysis of this experiment is performed in Chapters 11 and 12.

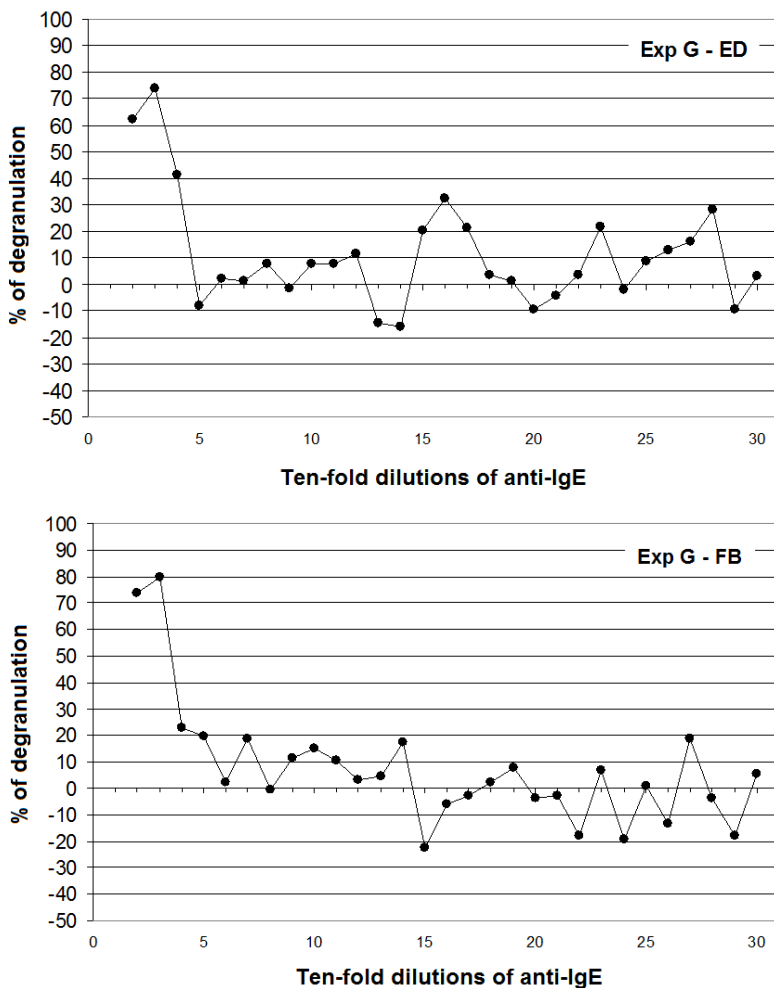


Figure 9.6. Experiment G was done blind and was a failure. Note that all blind experiments (experiments E, F and G) were performed with the same series of anti-IgE at high dilutions. An important control would have consisted in verifying that this series of high dilutions of anti-IgE was effectively effective in *open-label* experiments. This control has not been performed.

The debate with Maddox's team went on. J. Benveniste explained that "if such an experiment, performed in these conditions might cancel five continuous working years and a whole set of convergent successive experiments, then it was necessary to abandon any reasoning and any scientific approach."¹⁰

The extremely fast English language of the three investigators made sometimes the understanding difficult. The loud and high-pitched voice of J. Stewart, his poorly mastered excitement did not facilitate concentration. J. Randi learnedly explained to J. Benveniste that if he claimed holding a unicorn in his garden, it was normal that one checked this more carefully than for a simple goat. In a totally surrealist moment, the secretary of the laboratory stuck the head through the door and asked what she had to tell to Japanese television which waited for an interview of J. Benveniste.

Then, J. Maddox wrote on several paper sheets three telephone numbers which he distributed to the members of the team. It was his telephone numbers at office, at home and... in weekend. It was – he told us – in case we would have forgotten to say something. Maybe he hoped that somebody was going to admit that she/he was the one who manipulated all the experiments in the back of J. Benveniste.

Quickly, the three hunters of unicorns gathered their belongings, switched off the tape recorder which recorded the discussion and got back the numerous photocopies which they made. They left the laboratory in a few minutes to go to wait for a taxi. Along the way towards the exit of the building, they passed in front of a table stocked with bottles and surrounded with some people looking distraught. They will interpret later this scene as the anticipation of a victory. It was simply a student celebrating the end of her internship.

A few moments later, a press photographer seeking news about the inquiry saw a group of three individuals standing near the building, looking as conspirators and examining with perplexity a document, a plane ticket probably. In a professional reflex, the photographer took remote shots. Only a few moments later, he understood that he was lucky enough to hold at the end of his objective J. Maddox and his two stooges in a funny group portrait.¹¹ The idea that they were the investigators of *Nature* did not cross his mind. He had indeed wondered who these "three gangsters" (*sic*) were. The anecdote succeeded in making J. Benveniste smile, but the team was knocked out and, in meeting in the office of the latter, tried to get a grip on oneself and to review the situation which was suddenly very uncomfortable.

"I understood that we had been scammed"

This is the story of this week at the Unit 200 of Inserm. These few days were the peak of "Benveniste's affair" after what nothing will ever be as before. J. Maddox had succeeded. He was going to make "explode in mid-flight" the theme of research on high dilutions. Nevertheless, he had been just about to fail. Later, he "innocently" admitted, clearly recognizing that the fate of the laboratory of Clamart was sealed even before the first basophil had been counted:

"The experiments worked well. I was very worried that they obtained experiments so perfect from their point of view. I wondered what we were going to do if, after all, all that we had to say was that Benveniste was right. I had committed to publish the investigation report. I risked being in the situation to draft a report whose conclusion would be: magic is true."¹²

Thanks to the authority and the leading position of *Nature* in the scientific world, J. Maddox was successful thanks to an uneven balance of power to make coincide the events – even if it meant inducing them – and his vision of "true science". As J. Benveniste told:

"I had in my lab one of the men with the highest position in science, John Maddox. I was in the position of a man who meets the Pope and the Pope asks for his wallet; what was I to do? It is not easy to say no."¹³

Both stooges of J. Maddox – who in fact had been instrumented by the latter – left the scenery and, with the authority of *Nature*, J. Maddox could now draft a report where nothing would be spared to J. Benveniste and to his collaborators. He had nevertheless offered to them to come to repentance, but because of their refusal, there would be no mercy. Indeed, before the episode of the telephone numbers intended for those who would have had possible faults confessing, he had proposed to J. Benveniste to back-pedal:

"When Maddox, as soon as the code was unblinded, turned towards me by asking immediately: "you remove your paper?", I understood that we had been scammed"¹⁴

Of course, as we will see, J. Benveniste answered the criticisms and he did not hesitate in turn to attack the rough methods of the investigators. Even if *Nature's* team came back from Clamart with few objective facts in their shoulder bag, the dominant message was that the experiments were an "illusion". It was difficult in front of a truth so clearly and brutally expressed – with furthermore

the authority conferred by *Nature* – to answer by explaining some methodological subtleties. Only some clichés circulated with efficiency: the magician, the envelope stuck on the ceiling and the jokes on the water which lost the memory. The rumor made the rest and J. Benveniste was even more marginalized.

Already, on Tuesday, July 5th, 1988 in the evening, the latter participated in a meeting between scientists:

“One evening of this week, I went to a dinner at the invitation of Minister of Research Hubert Curien, together with John Maddox, with about fifteen French scientists of the highest level, with the managing director of the Inserm P. Lazar, and of ephemeral Minister for Health Léon Schwarzenberg.

By going to this dinner, I hoped to find the support of the French scientific community, which was until then sorely lacking to me. I would indeed have wished that the Minister or the politico-scientific authorities appoint a team of recognized experts in charge of advising me, determining which controls I must do and which hypotheses for the interpretation of the results I could consider. During the meal, I understood very quickly that I could expect no support, and that I had been invited to my own public execution. At one moment, I was quite simply accused by a professor of the “*Collège de France*” (who has an illustrious name but does not seem to have made discoveries justifying his position in the scientific Establishment, nor his arrogance) “to dishonor the French scientific community”. This must be understood as: to deprive some of my fellow countrymen, who were potential Nobel prize laureates, of their possible distinction.”^{15, 16}

Notes of end of chapter

¹ J. Maurice. L'hebdomadaire « Nature ». Un sanctuaire de la science en marche. *La Recherche*, juillet-août 1997, p. 120.

² P. Alfonsi. Au nom de la Science, p. 84.

³ In 2011, Alvarez was arrested and jailed because he was accused of identity theft. In 1987, he had stolen the identity of a man from New York together with his date of birth and Social Security number in order to obtain a U.S. passport. This passport allowed him travelling with J. Randi in different countries. Therefore, the true identity of the man who accompanied J. Randi in July 1988 in France was in fact Deyvi Pena who came from Venezuela in the mid-80s on a student visa. J. Randi and Deyvi Pena married in 2013 (https://en.wikipedia.org/wiki/James_Randi).

⁴ Note that the initial invitation was from July 2nd to 7th.

⁵ "[...] we were stunned to learn that the salaries of two of the co-authors of the article of Dr Benveniste were paid through a contract between INSERM U200 and French Boiron firm, a manufacturer of pharmaceutical and homeopathic products, as our notes of hotel." (*Nature*, July 28th, 1988, p. 287).

⁶ M. de Pracontal. Les mystères de la mémoire de l'eau, p. 41.

⁷ Internal report of E. Davenas, July 1988.

⁸ Randi said in these terms how the coding was performed: "All the operations took place under the control of a video camera. Elisabeth Davenas brought numbered tubes containing the dilutions in a separate room, put them on the table, then left the room. Stewart, Maddox and myself stayed in the room, windows of which we had masked with some opaque paper so that one cannot see what took place there. We also made sure that there were no microphones. Then, always in front of the camera, we erased the numbers registered on tubes, and replaced them by labels numbered according to an unpredictable code. This code was transcribed on a paper, which we put in a big envelope closed with a special adhesive: if somebody tried to open the envelope, visible tracks would be left. One could not either read the data through the envelope, because I had wrapped the envelope in a sheet of aluminum.

We then returned the tubes to Elisabeth Davenas. At this stage, none of the experimenters could know which tube to contaminate. Then, the coded dilutions were put in touch with basophils, the colouring agent was added and the preparation was placed in a cold room." (P. Alfonsi. Au nom de la Science, p. 46).

⁹ This important point is not reported in the investigation report of *Nature*. We had already reported this issue to M. de Pracontal when he collected our testimony in 1988 (cf. Les mystères de la mémoire de l'eau, p. 49).

¹⁰ P. Alfonsi. Au nom de la science. p. 34.

¹¹ This picture allowed illustrating in particular an article of *Liberation* of July 23-24th, 1988 ("La mémoire de l'eau au microscope magique" [*The memory of water under magic*

microscope] as well as an article in *Le Monde* of January 21st, 1997 of E. Fottorino (« La mémoire de l'eau. Du rêve au soupçon »).

¹² M. de Pracontal. *Les mystères de la mémoire de l'eau*, p. 42.

¹³ Martin J. Walker. *Dirty Medicine*. *Slingshot Publications*, London (1993).

¹⁴ P. Alfonsi. *Au nom de la science*, p. 33.

¹⁵ The same episode is told in nearby terms by E. Fottorino (*Le Monde*, January 21st, 1997): "One evening of this hard week of examinations, Minister of Research, Hubert Curien, invited Doctor Benveniste to a dinner. John Maddox also participated in the party, together with about fifteen scientists. Jacques Benveniste is relieved at first. He hopes that a real committee of researchers appointed by public authorities will exercise a control more serious than the pantomimes of an illusionist. It will not be the case. Professor Pierre Joliot, of the "*Collège de France*", deeply blames doctor Benveniste for dishonoring the research: "I understood this evening that I was not their man. They implicitly told Maddox: make what you want with him. (...) One left me to the dogs." "

¹⁶ J. Benveniste. *Ma vérité sur la mémoire de l'eau*, p. 70.