For clone H35, Dr. Popovic testified that it had not been "evaluated." Tr. at 2315. He stated --

> So, obviously, I put ND because the problem was again the same, there were not sufficient cells to make a good, such quality of preparation which one could interpret with a confidence and could evaluate, could put there a number.

Tr. at 2316.

Dr. Popovic further testified that not detecting the AIDS virus in some of these assays was not truly a negative result, i.e., signifying "no presence of the AIDS virus." Tr. at 2446. When discussing the IFA results reported in Ms. Read-Connole's notes [Ex. H-22 at 30], he stated --

> It says only that that particular slide which was evaluated, the person couldn't with confidence tell that it was a fluorescent cells, that is what it says. It does not says that it is negative for the virus.
>
> . . . .
>
> No. What it [the minus value] says that with the viral antigens which were in the cells, there were no reaction with that partic[ular] sera. Not that it was virus or not virus was there. The data which we obtained were inconclusive, that is what it says.
>
> . . . .
>
> You should take into consideration other reactivity which we paralleled, other data which we paralleled with this experiment. If you take in consideration together, obviously, this data are inconclusive and one cannot claim that it is negative. It is, immunofluorescence is a subjective matter. . . .

Tr. at 2446-48.

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[37](...continued)
in his opinion the two patient sera were "equivalent," which further indicated that the 6-day IFAs for these clones were "uninterpretable." Tr. at 2315.

Table 2 reports RT, IFA, and Electron Microscopy (EM) results for individual patient isolates.[38] See Appendix A for Table 2. At issue are the ND entries for patient S.N. for IFA and EM.[39]

Dr. Popovic testified that there were conflicting IFA results for patient S.N. since it was negative in one experiment and positive in another. Tr. 2317-18; Ex. H-22 at 19, lines 2 and 4. Moreover, since other data for this patient isolate were positive for virus activity, Dr. Popovic regarded it as "misleading" to report as negative the failure of one assay to detect virus activity. Tr. at 2317-18. Similarly, for the electron microscopy results, Dr. Popovic testified that, in light of the other positive data, as a "very imprecise technique . . . one can readily miss the presence of virus in the cells. . . . Obviously, I have to question the validity of the electron microscopy observation." Tr. at 2319.

The expert retrovirologists assessed the results reported as ND together with other data presented in Tables 1 and 2. Dr. Gartner testified that in the context of Table 1, the ND entries would not make any difference in evaluating the data. She testified --

> They would mean that there's no data for that, those points. I guess it wouldn't mean very much to me because when I look at a paper, the primary thing I'm looking for is whether or not the evidence presented supports the conclusions the author is drawing. So I would focus my attention on some of the other things. For example, I would look down at the reverse transcriptase activity and at day 14 I see a value of two million cpm of RT that is such definitive evidence

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[38] Electron Microscopy is the use of a high powered microscope to look for evidence of virus expression in cultured material or tissue. Thin slices of the biological material are examined using an electron microscope to look for the presence of virus particles, particularly the budding of the virus particle, and other morphological evidence. Tr. 1818-21 (Gartner).

[39] Data were inadvertently reversed for two isolates so that the ND which Dr. Popovic intended to report for patient S.N. is in the wrong horizontal column in Table 2. There is no dispute that the ND was actually intended for the EM results for patient S.N. and that result is what ORI found was falsified.

to have RT activity at that level is so unmistakable, I would say this person has something.

. . . .

It [the RT activity] is under the H9 clone, but the others are also very high. The level of activity there is unmistakable and also I'd probably go back and look at, if I look at the NDs, I'd say well why did this person get, why is there an ND here and I would notice . . . there were too few cells to evaluate would be the first thing that would come to my mind, especially since they've described here a cytopathic virus.

So I would look at the total cell number . . . the cell numbers in those particular clones [H9, H17, H31, and H35] at day six were all very low.

. . . .

. . . So I guess I'd conclude that the reason why a person couldn't evaluate this, if that was indeed what ND meant as opposed to not doing the test, that it was because there were too few cells . . .

. . . .

No, it wouldn't make any difference to me [if instead of ND those had all been just negative]. Again I would take the whole . . . . Looking at the day 14 . . . that's such definitive evidence that to me it's overwhelming that this is real.

Tr. at 1826-28; see Tr. at 1904-05 (Blattner); Tr. at 1955 (Svoboda).

Dr. Gartner testified that since the notebook entries indicated a quantification in almost all cases for all three dilutions of rabbit antisera but no quantification for patient sera for these four clones, this suggested to her that the person doing the assay was not able to come up with a definitive number for these clones. Tr. at 1831-32. She testified that whether it was appropriate to report results for these clones as ND is "an individual thing" which "depends on what one's individual terminology is, the way one keeps track of one's observations." Tr. at 1832. She also testified that the more meaningful results were against the rabbit antisera because of the variability in patient antisera with

regard to level of antibody to viral components.[40] Tr. at 1833-36. Dr. Gartner discussed in detail her assessment of the IFA results for clones H9, H17, and H31 as recorded by Ms. Read-Connole. Tr. at 1830-36; 1866-70. Her testimony overall corroborated Dr. Popovic's explanation. In addition, Dr. Gartner testified that she had concluded that the "investigator [could draw] no meaningful conclusion . . . from reading" those slides. Tr. at 1905. Because of the low cell counts for day 6, she thought that "there was difficulty in interpreting the results." Tr. at 1904-05. She affirmed that the data reported in the Table were adequate for her to evaluate the results of the experiments. Tr. at 1905.

Dr. Malkovsky testified that he construed the NDs as meaning either that the experiments could not be performed for technical reasons or if performed, the results were inconclusive and could not be interpreted. In his opinion, it did not matter how ND might be defined. Tr. at 2228. Dr. Malkovsky testified that he found Table 1 consistent with the results reported in Ms. Read-Connole's notebook since the IFA results for the clones in question had not been quantified. Tr. at 2232-33, 2250. Moreover, he stated that in light of the other data, he would expect the 6-day data for clones H9, H17, H31, and H35 to be difficult to interpret. Tr. at 2254.

For Table 2, Dr. Gartner testified that the NDs for IFA and EM results for patient S.N. in Table 2 were not "meaningful." Tr. at 1813, 1820. She stated --

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[40] ORI asserted that Dr. Popovic "ignores the fact that the rabbit serum used in the IFA experiments may contain impurities, thereby producing an anomalous result." ORI post-hearing br. at 77. ORI provided no record citations for this proposition. Moreover, in response to ORI's questions about the rabbit antisera, Dr. Gartner's testimony was that presumably the experiment would have been done in a way so that the rabbit antiserum would give a better result than the patient serum. Tr. at 1833-36; 1867-79; 1897-1901. ORI presented no evidence here that the inoculation of the rabbit was not done using standard procedures, nor any evidence that Dr. Popovic knew there was any question about impurities in the rabbit serum (which the record indicates was prepared at another laboratory) which would cause anomalous results. There is no reason to conclude that any difficulty quantifying low positive results with this rabbit antisera meant that other results were anomalous.

> . . . it's not significant. When I look at this table the point I see which distinguishes it from Table 1 is that there was isolation of this virus from more that one individual and that it was possible to detect that virus from the culture material that was used from more than one individual. So that value to me, that's insignificant to me.

Tr. at 1829.

In general, Dr. Gartner compared EM "with respect to novel virus hunt, as a needle in a haystack." Tr. at 1820. She agreed with the analogy that it was like slicing a loaf of raisin bread and if you happened not to get a raisin, it did not mean the loaf was not raisin bread. Tr. at 1820-21. Dr. Sodroski indicated that alone EM was not a very "efficient" technique since there may be a relatively low number of infected cells to search for. Tr. at 667, 672-73; see also Tr. at 1954-55 (Svoboda).

In assessing all the ND entries for both Tables 1 and 2, what is striking from Dr. Gartner's testimony is that in the circumstance here, where one is searching for evidence of a novel retrovirus, each assay or test only provides a clue which may or may not be useful. Tr. at 1785-1821 (Gartner). She emphasized that no one test or assay was "good" by itself. Rather, she testified:

> . . . . Everything is done in the context of the whole, the total. You can't evaluate any single piece of data in a vacuum when you're working in this kind of system because there are too many other potential explanations for that finding.
>
> But when you have several clues that all point to the same direction, then it's believable. But no, I would not evaluate any of these methods independently.

Tr. at 1821.

The failure to detect the presence of the virus from any one test was not particularly significant. The retrovirologists emphasized that these were subjective assays or tests, dependent in large measure on the investigator's interpretations. Based on unrebutted testimony from these retrovirologists as to the nature of IFAs or EMs when an investigator is looking for evidence of a novel retrovirus, we find that whether or not the results had been literally transposed from Ms. Read-Connole's notes (or in the case of the EM reported as negative), the tables would have conveyed the same

critical information concerning the conclusion as to whether there was persuasive proof of viral expression. Thus, we conclude that since other data reported showed that a culture was positive for virus, it was a reasonable judgment for Dr. Popovic to report as ND experimental results for tests where virus activity had not been detected but which in the context of other available data Dr. Popovic judged to be not quantifiable or artifactual.

4. The expert testimony which ORI presented did not prove that the ND entries were inaccurate, misleading, or false.

We discuss in this section why we found that the expert testimony which ORI presented did not outweigh the testimony by Dr. Popovic and his witnesses and therefore did not establish that the ND entries were inaccurate, misleading, or false. We found the testimony of ORI's witnesses deficient in the following respects:

- ORI presented no evidence from a retrovirologist directly rebutting the testimony from Dr. Popovic and his witnesses about why the ND entries were a reasonable judgment as to how to report the experimental results.

- The testimony of ORI's witnesses was based on the assumption that "ND, not done" in the tables meant that the experiments had not been performed (in the sense of not even attempted). Dr. Schaffer's testimony was premised on reading the ND entries as equivalent to not performed. Tr. at 1542, 1556, 1558-61, 1563-64, and 1636. Similarly, Dr. Berns viewed the "ND, not done" here as meaning only not performed. Tr. at 1048-49, 1054-55, and 1072.[41] This is also

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[41] Dr. Berns considered Respondent's explanation to the ORI scientific advisors that ND in his mind meant not determinable as opposed to not performed, but said the problem they had with that "was that it was clearly labeled with a result. . . ." Tr. at 1048. His reasoning was that, if one considered indeterminable the ND entries corresponding to results reported as "-" in the notebook, one would be invalidating the results for the controls, which were also reported as "-". Tr. at 1048. We did not find this reasoning persuasive, for several reasons. First, ORI presented no evidence that reading the slides for the controls would have presented the same difficulties as reading the slides for the

(continued...)

apparent from the interpretation given by Dr. Berns to Dr. Popovic's written statement in a 1986 memorandum to Dr. Gallo that the EM for patient S.N. was not done properly. Dr. Berns concluded that this statement was inconsistent with reporting this test as "not done" in the paper, which to him meant that the test was not performed at all. Ex. H-61; Tr. at 1054-57. Dr. Martin's testimony also suggests that he reviewed the notebook entries against what was reported in the table, specifically Table 1, only in terms that "not done" meant only not performed. Tr. at 1348.[42]

- ORI's experts failed to take into account the subjective and/or unreliable nature of the IFAs and EMs. As we discussed above, there was relevant testimony from expert retrovirologists that IFA assays are highly subjective in nature and that EM results were circumstantial evidence at best. Tr. at 1791, 1812-16, 1818-20, 1824-25 (Gartner); 2251 (Malkovsky); 667, 672-73 (Sodroski); 1952-55 (Svoboda). Failure to consider the nature of these results led ORI's experts to misinterpret Ms. Read-Connole's notes to mean that virus was not present.[43] In contrast, the retrovirologists indicated that if there were other tests performed at the same time that conclusively showed the presence of virus, one would conclude that virus was in fact present.

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[41](...continued)
clones, where cell death was a problem. Second, there were no conflicting data for the controls, as there were for the other results Ms. Read-Connole recorded as "-". Third, this testimony did not take into account evidence that Dr. Popovic reread the slides, and that Ms. Read-Connole was cautious in quantifying IFA results.

[42] In questioning its witnesses, ORI presumed that the ND entries meant only that no test had been performed (i.e., attempted) and therefore, that any indication from the notebook entries that a test was performed meant the use of ND in these tables was a falsification. In our opinion, this slant misfocused these experts' testimony.

[43] To the extent that ORI's experts misread "-" in Ms. Read-Connole's notebook as meaning only that no virus was present, this supports Dr. Popovic's view that to report the "-" which appeared in the notebook would have been misleading to individuals not experienced in evaluating these tests for a cytopathic retrovirus.

paper. The experts had differing opinions about: 1) whether or not these tests should be repeated (Tr. at 1351-54 (Martin); and 1561, 1678-79 (Schaffer); 1051 (Berns)); 2) whether the "+" or "-" entries could be intermingled with the percentage figures (Tr. at 1109-10 (Berns); 1542-46 (Schaffer)); 3) whether the "+" and "-" entries should have been quantified (Tr. 1543, 1673-77 (Schaffer)); and 4) whether there needed to be a footnote explanation of why the slides could not be read and therefore results could not be determined (Tr. at 506 (Richards)). We conclude therefore that these witnesses were testifying to their own personal standards, rather than to standards for reporting data commonly accepted in the scientific community at the time the Science paper was published.

5. ORI did not establish that Dr. Popovic had any motive to falsify when he used ND in Tables 1 and 2.

ORI contended that Dr. Popovic intentionally concealed the actual results of the experiments in Tables 1 and 2 by listing certain data points as ND. ORI post-hearing br. at 77-78. ORI, however, concluded that the "misreporting . . . did not affect the conclusions of the experiment." ORI Final Report at 55-56. The heart of ORI's argument here is that "ND, not done" can only mean that the experiments were not performed and, therefore, to report experiments as not being performed when they were actually performed is an intentional act of deception. Moreover, ORI reasoned that we can infer intent to deceive because the overall effect of using the NDs is to make the paper look better scientifically. ORI post-hearing br. at 78.

We conclude, for the following reasons, that ORI did not prove by a preponderance of the evidence that Dr. Popovic had any motive to falsify when he used ND in these tables:

o As we discussed above, there is no basis for us to conclude that "not done" means only "not performed." We found that Dr. Popovic used ND in these tables for the data points at issue to convey that the data was inconclusive, unquantifiable, or not determinable. Moreover, Dr. Popovic's use of the term ND to mean not determinable here certainly conveyed to the reader that the results of these experiments were not clearly positive and that they were at best inconclusive. Furthermore, Dr. Popovic satisfactorily explained that the use of ND here represented a reasonable judgment on his part because to report the results exactly as

recorded in Ms. Read-Connole's notebooks would have been misleading to the reader.

o We find it significant that there was ample testimony from expert retrovirologists stating that the NDs in the tables conveyed to them zero information. Dr. Malkovsky testified that the 6-day data was not important for interpretation of Table 1; and the NDs here conveyed zero information. Tr. at 2228. While he found this data useful to show the dynamics of the infection, he did not find this data "biologically . . . important" and stated that he would probably omit the 6-day data, showing only the "optimum" data at 14 days. Tr. at 2229. Dr. Malkovsky also testified that IFAs were the least reliable biological method and that the 6-day data was "not really adding anything to [Table 1]." Tr. at 2251. Moreover, he indicated that a reader would infer from the face of Table 1 that there might be a difficulty in determining the results of an IFA at day 6 because the paper indicates for the clones in question that the yield of cells at this time was incredibly low, which would indicate massive infection and a lot of damaged cells. Tr. at 2254; see also Tr. at 1826-28 and 1904-05 (Gartner). Dr. Gartner testified that the ND entries for the IFA and EM results are not meaningful or significant to her in evaluating the data reported in Table 2. Tr. at 1828-29.

o ORI did not present expert testimony to rebut Dr. Malkovsky's and Dr. Gartner's evaluation of the ND entries in the overall context of the data presented in these tables. Since these entries have no effect on the experts' assessment of either the 6-day data or the data as a whole, we find that ORI did not show that these entries made the paper look better scientifically.

o We do not think it likely that the definition "ND, not done," appearing as it does in extremely fine print in the middle of a long legend, could be significantly misleading to the reader. Finally, the fact that this definition did not appear until draft seven, late in the drafting process, undercuts the notion that this was a calculated attempt to mislead.

In sum, ORI did not prove by a preponderance of the evidence that the ND entries were intentionally falsified, or even untrue.

C. THE 10% ENTRY FOR THE 6-DAY IFA RESULT FOR CLONE H35

Table 1 of the Science paper reports on the response of cloned T-cell populations to infection with HTLV-III. Results for eight different clones at 6 and 14 days after infection are shown for the following: total cell number; percent of multinucleated cells; percent of positive cells as shown by immunofluorescence assay (IFA) against both rabbit antiserum to HTLV III (diluted 1:2000) and patient serum (from patient E.T.); and reverse transcriptase activity. The entry in Table 1 for the IFA results against rabbit antiserum for clone H35 at 6 days is 10%. Ms. Read-Connole's laboratory notes dated February 29, 1984 contain the following entry for 6 days after infection for clone H35:

> very few cells positive for rabbit antibody

See Appendix B. This notation is written across columns for IFA assays with dilutions of the rabbit antiserum of 1:500, 1:1000, and 1:2000.

Dr. Popovic testified that he reread the slides prepared for the IFA assay, after Ms. Read-Connole had read them, and quantified the results for the 1:2000 dilution for clone H35 at 10%. Tr. at 2320-23; see also Exs. H-48 at 49; H-157 at 20-21.

ORI determined that the 10% reported on Table 1 for clone H35 was "best characterized as falsification in reporting research." ORI found that the contemporaneous laboratory notes indicated that "substantially more than 10% were positive." ORI Report at 55-56. ORI argued that --

> Because there is no numerical or other laboratory data to support the "10%" entry in Table 1 in the face of directly contradictory data and Dr. Popovic's explanations are not credible, the "10%" entry is false and misrepresents the results of the laboratory data in Ms. Read-Connole's notebook.

ORI post-hearing br. at 89.

We conclude that ORI did not establish by a preponderance of the evidence that the 10% entry was intentionally falsified or even untrue, for the following reasons:

- Dr. Popovic's testimony that he reread the slide was credible and is corroborated by other evidence in the record.

- Contrary to what ORI suggested, the record shows that, given the imprecise and subjective nature of the IFA assay, it was appropriate for Dr. Popovic to reread and quantify this data point, notwithstanding Ms. Read-Connole's notes.

- Dr. Popovic's testimony that he was able to place a value of 10% on the results is credible since ORI presented no affirmative evidence that its true value was different and since his testimony is corroborated by other evidence in the record.

- ORI did not show that Dr. Popovic had a motive to falsify this one insignificant data point.

- ORI's reliance on its expert testimony is misplaced since that testimony relied on readings of Ms. Read-Connole's laboratory notes which are inconsistent with her own explanation of the notes and which are not the only reasonable reading of those notes.

- Under the particular circumstances here, we find no basis to draw a negative inference from the absence of laboratory notes substantiating the 10%.

- ORI's other arguments are not persuasive.

1. Dr. Popovic's testimony that he reread the slide was credible.

As discussed above, we found Dr. Popovic generally to be credible, and his testimony on rereading the slide is corroborated. Ms. Read-Connole testified that the IFA slides reported in Table 1 were from the "very first antibody to HIV and this reacted very, very nicely and it was quite spectacular and I thought [Dr. Popovic] should see it." Tr. at 2159. She also testified that it was her general practice to write notes to Dr. Popovic in her notebook concerning such results and to leave the slides in a refrigerator. Tr. at 2158-59; see Ex. H-22 at 29-31. For these IFAs, her notes singled out certain slides as a "[g]ood positive" and directed Dr. Popovic's attention to these slides. Ex. H-22 at 31; Tr. at 2158 (Read-Connole).

ORI would have us discount Ms. Read-Connole's testimony on the basis that, having collaborated with Dr. Popovic, it was in her interest to support his position. Since her testimony is consistent with her contemporaneous notes, however, which indicate that she clearly expected Dr. Popovic to look at the slides, we do not see any

reason to discount her testimony here solely on the basis of any interest she might have in seeing Dr. Popovic exonerated. Moreover, the testimony of other scientists who appeared for ORI indicated that a senior scientist like Dr. Popovic would reread the slides after a technician had read them. See, e.g., Tr. at 318-19 (Goldberger); Tr. at 476 (Richards); Tr. at 1075 (Berns). Here, where the slides in question were IFAs from the first tests against the putative AIDS virus and overall produced data which the retrovirologists who testified before us considered overwhelming evidence of the etiological agent of AIDS, we find it almost inconceivable that Dr. Popovic would not have read the slides, as he testified that he did.

Thus, we find credible Dr. Popovic's testimony that he reread the slides.

2. ORI did not show that it was inappropriate for Dr. Popovic to have reread the slides.

ORI said that it was inappropriate for Dr. Popovic to reread slides and assign a value where Ms. Read-Connole had not been able to do so. However, several of ORI's own witnesses testified that they would consider it appropriate generally for a senior scientist to reread a slide read by a technician, such as Ms. Read-Connole. See, e.g., Tr. at 318-19 (Goldberger); Tr. at 474-76 (Richards); Tr. at 1075 (Berns); accord, Tr. at 1816 (Gartner).[45]

Dr. Richards indicated that he would consider reevaluating the slide appropriate, but that he personally would feel uncomfortable doing so unless he had discussed the change with the technician or the technician agreed. Tr. at 476, 508. While Ms. Read-Connole testified that Dr. Popovic had not discussed the 10% value with her at the time, she testified at the hearing that she believed that the 10% value for clone H35 at 6 days was correct. Tr. at 2170-72. More important, she also stated that she had seen Table 1 in Dr. Popovic's handwritten draft and that in preparing the

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[45] ORI relied on Dr. Gartner's testimony that "a scientist 'wouldn't make a change in someone else's data'." ORI's post-hearing br. at 90, citing Tr. at 1881. ORI takes this statement out of context. Dr. Gartner testified that, if she independently changed a technician's reading of a slide, she would not make that change in the technician's notebook but would record it "in my own notebook." Tr. at 1881.

table Dr. Popovic would have been working from her notes as well as his own notes. Tr. at 2211-12. ORI presented no evidence that she objected to the 10% at the time.

Dr. Schaffer also raised questions about Dr. Popovic rereading the slide, based on her opinion that Ms. Read-Connole was an expert at reading IFAs and that, if she could not assign a value, neither could Dr. Popovic. Tr. at 1601-13. In effect, Dr. Schaffer inferred from the fact that Ms. Read-Connole did not assign a value that the slide was unreadable.[46] This is not a reasonable inference for several reasons. First, while Ms. Read-Connole was clearly quite skilled at evaluating IFAs, her testimony indicates that she was very conservative in assigning values when reading IFA slides. Ms. Read-Connole testified that --

> I think I'm probably one of the best people around to read them because I'm very cautious about my readings. I don't like to say oh, gee, that's positive without having some other basis for believing that the value might be correct or if something is indeed not clearcut, I don't want to put it down that it is.
>
> . . . .
>
> A certain percentage when I'm not sure as you can see by my pluses in the notebooks.
>
> . . . .
>
> No [she could not make a reading for clone H35], because as I explained earlier there was a lot of debris in that first rabbit antisera that we had and due to the cytopathic effect of the virus infecting the cells in the early stages or a short time period after the cells were infected, there was enough debris that I could not definitely determine the number of cells that were indeed positive to get a positive reading. But I was noting that there were positive cells.

Tr. at 2187-89.

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[46] We note, however, that (in contrast to her later testimony that the slide was unreadable), Dr. Schaffer initially expressed the opinion that the notes indicated that the slide was probably not "easily interpretable." Tr. at 1546.

Second, the unrebutted testimony indicates that reading an IFA slide when detecting a retrovirus involves judgment. In discussing Dr. Popovic's quantification in light of Ms. Read-Connole's notebook entry, Dr. Gartner testified that --

> . . . . But no I wouldn't have any problem with that because again, especially with some of the work I've done with cytopathic retroviruses like HIV, you don't have the luxury of the optimal situation. You have to go with what you can see and so, no, I wouldn't have any problem with it.
>
> It's a very subjective assay . . . .

Tr. at 1816. Dr. Gartner's description of what would make a slide difficult to read but still possible to evaluate when working with a cytopathic retrovirus is consistent with what both Dr. Popovic and Ms. Read-Connole said about this clone H35 slide. Ms. Read-Connole described the problem with reading such slides as distinguishing among the various greens to pick out the true fluorescing cells. Tr. at 2157-58. There was no scientific evidence presented by ORI to rebut this. Since Dr. Schaffer's testimony failed to address this, it is possible that she did not consider the judgment involved in reading IFA assays of a retrovirus with cytopathic effects, because her work was primarily with a different type of virus (herpes simplex). Tr. at 1464 (Schaffer); Tr. at 1801 (Gartner).

Third, we found persuasive Dr. Popovic's testimony that he felt comfortable assigning a value, where Ms. Read-Connole had not, because there were three wells in the slide with different dilutions of rabbit antiserum which gave him confidence in the 10% value he assigned. Dr. Popovic testified that --

> I would like also just to remind that it says positive for rabbit antibody, also those rabbit antibody was tested on three wells. So one could evaluate 1 to 500, 1 to 1,000 dilutions wells, also 1 to 2,000 dilution wells, while in the case of human, using human sera in that wells, it was only one well [per human sera].
>
> So what gave me confidence that . . . the 10 percent is correct, because I went through evaluation of and quantified each of these wells and determine that 10 percent is for the data point 1 to 2,000.
>
> . . . .

> usually there were no big discrepancy, but in this case I sit down and I evaluated myself since it was possible to come to some quantitative value in this particular case with rabbit antibodies, that's what I did.

Tr. at 2321-23; see also Tr. at 2440, 2442.

ORI asserted that Dr. Popovic's testimony on rereading the slide is not credible since the other datapoints in the table correspond to Ms. Read-Connole's notebook entries. This argument is not persuasive since only the data points Ms. Read-Connole in fact quantified in her notes are repeated in the tables exactly as recorded in the notebook. Since she did not actually quantify an IFA reading for clone H35 at 6 days, the use in the table of a result quantified by Dr. Popovic would not be inconsistent with what Dr. Popovic otherwise did in constructing the table. Moreover, his testimony provides a rational basis for why he would assign a value for rabbit antibody, where there were three different wells, but would treat other non-quantified results as not determinable.[47]

It is important to note here that this is not a circumstance where recorded experimental data was altered and the altered data reported as the true value. There was no quantification initially recorded by Ms. Read-Connole and the quantification is consistent with what Ms. Read-Connole and Dr. Popovic say about the slide.

Thus, we conclude that ORI did not prove that it was inappropriate for Dr. Popovic to reread the slide and

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[47] Ms. Read-Connole was unwilling to quantify IFA tests where the results were not clearcut. The "+" she used "designates a weak positive reaction (i.e., [less than] 10%)." Ex. H-32C at 3. Dr. Popovic stated that the "+" for clone H9 against the E.T. antisera had been based on only one unequivocally positive cell. Ex. H-157 at 20. Therefore, it is not inconsistent with the recorded notes for the IFA results that Dr. Popovic could quantify the IFA results for clone H35 against the rabbit antisera. One would conclude from Ms. Read-Connole's precision and skill at reading IFA that if it were merely a weak positive, she would have noted that. Rather, she stated that the slide was positive but did not quantify it. In context, Dr. Popovic's quantification is consistent with the other IFA results recorded by Ms. Read-Connole.

assign a value, notwithstanding the fact that his technician had not done so.

3. Dr. Popovic's testimony that the 10% value was based on a reasonable judgment at the time is credible.

ORI did not show by a preponderance of the evidence that the 10% is false or inaccurate. ORI relied solely on Ms. Read-Connole's notebook and its witnesses' interpretation of her prior statements for its assertion that the 10% was false. ORI presented no other evidence either that an accurate reading of the slide would have given a different value or that the 10% was a scientifically impossible or unlikely result. Dr. Popovic, on the other hand, presented evidence that the 10% is not an impossible or unlikely result, given results of other experiments performed at or around the same time.

While we would agree with ORI that an IFA quantification could not properly be derived from other available data, ORI's suggestion that Dr. Popovic's testimony indicates he fabricated the 10% based on other data misconstrues Dr. Popovic's point when discussing other data. As Dr. Popovic asserted, other data that is available is consistent with an actual quantification at 10% and therefore tends to support his assertion that he read the slide and appropriately assigned the 10% value. The 10% value is consistent with the other data for clone H35. Dr. Popovic testified that clone H35 was clearly positive at 6 days in light of the presence of giant multinucleated cells and the relatively high RT value. Tr. at 2323. Also, he testified that, since the 14-day data was "very clear cut and conclusive," there could not have been very low or virus negative cell population at the 6-day point. Tr. at 2323-24. Moreover, Ms. Read-Connole testified that the 10% value was in the range of the values obtained with other clones in later experiments. Tr. at 2173-78; Ex. H-24 at April 30, 1984 et seq. While we do not think that values for other clones are particularly persuasive, no evidence was presented by ORI which even suggests that 10% was an inaccurate, impossible, or unlikely value for clone H35.

4. ORI did not establish that Dr. Popovic had a motive to falsify the 10%.

Another reason why we find credible Dr. Popovic's testimony that the 10% was based on his rereading of the slide is that ORI did not establish any motive for Dr. Popovic to fabricate this data point. While ORI discounted Dr. Popovic's claim that the 6-day data had little significance, this is just what the evidence here

shows. There is persuasive evidence that the 6-day data could have been omitted from the Science paper altogether. Tr. at 2228-30, 2251-55 (Malkovsky). The data presented for 14 days was "overwhelming" evidence of viral activity in the clones. Tr. at 1828 (Gartner); Tr. at 2228-29 (Malkovsky).

Dr. Popovic said that the 6-day data was reported to show the progression of infection of the clones. Tr. at 2472. ORI argued that this gave the 10% importance and that Dr. Popovic had a motive to falsify the 10% because it made the paper seem more rigorous than if Dr. Popovic had reported the result as indeterminable (as Dr. Schaffer and Dr. Berns said it ought to have been reported based on their interpretation of Ms. Read-Connole's notes). Dr. Malkovsky testified, however, that this data was useful to show the dynamics of the infection, but unnecessary overall, and that it made no difference in interpreting the data reported on Table 1 whether there was a value reported for clone H35 at 6 days or whether it was reported as indeterminable. Tr. at 2229-30. Dr. Malkovsky also pointed out that the data for any one clone was not really important for the meaning of the table since there were eight clones reported. Tr. at 2230. Indeed, unrebutted testimony establishes not only that the 6-day data was not necessary to the conclusions of the paper, but that 6-day data other than the data for the rabbit antiserum indicated the presence of virus. Tr. at 2253-54 (Malkovsky); Tr. at 2171-72 (Read-Connole).

Thus, we conclude that ORI did not prove that Dr. Popovic had any motive to falsify this data point.

5. ORI relied on incorrect readings of Ms. Read-Connole's laboratory notes.

In asserting that the 10% entry was false and misrepresented the laboratory data, ORI erroneously concluded that Ms. Read-Connole's notation was "directly contradictory data." ORI post-hearing br. at 89. This conclusion was based on testimony, primarily by Dr. Schaffer, which was in turn based on her interpretation of Ms. Read-Connole's notebook in light of statements made by Ms. Read-Connole in a letter sent to an ORI investigator in 1992.

As mentioned above, Ms. Read-Connole's notebook entry for clone H35 at 6 days was --

very few cells positive for rabbit antibody

In her letter, Ms. Read-Connole explained that her notation was "two separate statements." One that the slide had "very few cells" was "a comment on the number of cells on the slide." She explained that this was consistent with the other information available for that slide -- the "percentage of multi-nucleated giant cells" and the "cell count" on day six. Ms. Read-Connole also evaluated the slide explaining that "the cells that were on the slide were positive with the rabbit poly-clonal antisera" but that she "could not make an adequate estimation of the number of positive cells." Ex. H-63, Letter from Read-Connole to Healy dated May 13, 1992.

Dr. Schaffer testified that --

> . . . So what this means is that if you wash off a whole pile of cells you got to guess that a lot of them that you washed off were positive and some negatives or whatever, but every one of the cells, or all of the cells that were left, were positive, so what this indicates is if you were looking at this slide you would say there were not many cells but all the cells that were there were positive.
>
> 100 percent of the cells? [question]
>
> That was the implication that certainly the great majority of the cells that were on the slide were positive.

Tr. at 1552-53.

Dr. Schaffer testified that the 10% quantification "has got to be a figment of his [Dr. Popovic's] imagination" and that her guess was that the actual value was "closer to 100." Tr. at 1553. Dr. Schaffer read Ms. Read-Connole's notes, as clarified by Ms. Read-Connole's 1992 letter (Exhibit H-63), as meaning that "whatever cells remained were all positive." Tr. at 1553.

Dr. Schaffer relied for her opinion on her own interpretation of Ms. Read-Connole's statements, without actually speaking with Ms. Read-Connole. We see no reasonable basis in the record for interpreting Ms. Read-Connole's statement as Dr. Schaffer does. Indeed, such a reading would be contrary to other recorded data about clone H35. Ms. Read-Connole's letter merely indicates that she could not definitely quantify a positive reading, but that there were positive cells. At the hearing, Ms. Read-Connole testified that while she could not "definitely determine the number of cells that were indeed positive," she was "noting that there were

positive cells." Tr. at 2189. Her testimony shows that she did not intend to suggest in her 1992 letter that all of the remaining cells were positive. Tr. at 2168-69; 2189.

ORI would have us disregard Ms. Read-Connole's testimony at the hearing on the basis that she is biased in Dr. Popovic's favor since she collaborated with him. We see no reason why we should rely on Dr. Schaffer's interpretation instead. Dr. Schaffer's interpretation of statements made by Ms. Read-Connole in 1992 is inconsistent with the full text of the 1992 letter. Moreover, we find Ms. Read-Connole's testimony reliable, for several reasons. First, it is consistent with what her letter actually said. Second, her testimony is consistent with what the expert retrovirogists said about the condition of the 6-day slides. Third, and most important, it is not inconsistent with her contemporaneous notes.

The notes themselves are the best evidence of what Ms. Read-Connole's observation was in 1984 and those notes are ambiguous as shown by ORI's expert testimony. While Dr. Schaffer read them as meaning all of the cells were positive, Dr. Berns testified that --

> [t]he way I read it when I saw it originally was that there were very few cells positive, which essentially means a negative result to me.
> . . . .
>
> That's where I would have thought not determinable would have been an appropriate notation.

Tr. at 1050-51. Dr. Richards testified that the notebook is "ambivalent. It can be read either way." Tr. at 505.

Thus, we conclude that the 10% value is not contradictory to Ms. Read-Connole's notes.

6. No negative inference from lack of primary data is appropriate under the particular circumstances here.

The 10% value is not reflected in the notebooks produced during the investigation. Dr. Popovic indicated that he would have noted his reading of the slide and that such notes would have been discarded once the Science paper had been published. Tr. at 2457-59; Ex. H-157 at 21.

ORI asserted that we should draw a negative inference here due to the lack of primary data to support the 10% value. ORI post-hearing br. at 90; ORI post-hearing

- ORI's experts failed to consider the other evidence Dr. Popovic had that there was virus expression. As we found above, the expert retrovirologists would have derived from the tables with the ND entries information adequate to assess the experimental results reported there; they considered the ND entries to be consistent with what other data in the table indicated about the slides at the 6-day point in the case of the IFAs against patient sera E.T., or about virus expression in the patient S.N. isolate as compared to virus expression in the other individual patient isolates. Dr. Schaffer testified, with respect to the IFA experiments in Table 1, that the results for those clones made a difference to her because "if the data were done, I would like to know if it's not determinable . . . ." Tr. at 1674-75. This statement, however, reflects interest in information that the expert retrovirologists testified they could derive from Table 1 as a whole.

- ORI's experts misunderstood the entries in Ms. Read-Connole's notebook.[44] Tr. at 1048, 1057, and 1072-74 (Berns); Tr. at 1347-48 (Martin); Tr. at 1540-42, 1563, 1611, and 1615-16 (Schaffer). While Dr. Schaffer was skeptical of Dr. Popovic's reading of the laboratory results for IFAs as conflicting for patient S.N., Dr. Schaffer's testimony indicates that she did not really understand the entries. Tr. at 1601-02; 1616-18. Dr. Berns indicated that he had no information from Ms. Read-Connole about what she meant by her notations of "+" and "-" and no entry for the clones H9, H17, H31 and H35, respectively. Tr. at 1067, 1111; see also Tr. at 1675 (Schaffer). He stated that negative to him meant "she didn't see anything lighting up." Tr. at 1111. This is inconsistent with what Ms. Read-Connole's testimony shows about what one would see on IFA slides with a cytopathic virus. Tr. at 2156-57; see also Tr. at 1813-15 (Gartner).

Moreover, ORI did not show that Dr. Popovic was obligated to report the results exactly as they appear on the notebook pages. ORI's experts gave conflicting testimony about how a scientist in Dr. Popovic's position should report the results of these experiments in a scientific

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[44] In contrast, Dr. Gartner, an expert retrovirologist, understood from Ms. Read-Connole's notes what the controls were; Dr. Gartner's testimony was consistent with Dr. Popovic's concerning the meaning of the results recorded for the clones in question.

reply at 31. No such negative inference is properly drawn, however. This case is clearly distinguishable from the decision in Proposed Debarment of Dr. C. David Bridges, DAB No. 1232 (1991), where the Hearing Officer concluded that it was reasonable to draw a negative inference from the lack of adequate primary data for an entire set of reported experiments since there was other persuasive evidence of plagiarism and since the research was under a grant requiring data retention. Here, there is a great deal of primary data available for review. To draw a negative inference and find misconduct because primary data is not available for one insignificant data point is unwarranted.

Other factors for why we consider such an inference unreasonable under the particular circumstances here are: (1) Dr. Popovic credibly testified that his prior practice as head of laboratories where he worked had been to rely on technicians for notetaking; (2) Dr. Popovic and Ms. Read-Connole both testified that they exchanged and maintained notes on looseleaf paper so it is unlikely that all notes would survive; (3) Dr. Popovic was working with an infectious virus under controlled circumstances requiring that IFA slides be read in the dark so that notetaking was difficult; and (4) Dr. Popovic's laboratory and office were moved several times during this time period. ORI did not assert that there were definitive standards applicable here which required maintenance of all experimental data.

We also find it significant here that the 10% entry appears in the first handwritten draft of the Science paper, since this draft was prepared shortly after these slides were first read by Ms. Read-Connole. She read the slides on February 29 and March 1 of 1984. The handwritten draft was prepared several weeks later. Ms. Read-Connole testified that she saw this table, and ORI presented no evidence that would indicate that she disagreed with the 10% at the time.

Thus, we decline to draw a negative inference from the lack of primary data here.

7. ORI's other arguments are not persuasive.

In support of its findings that the 10% entry was falsified, ORI relied on the "inconsistency of Dr. Popovic's numerous and changing explanations." ORI post-hearing reply at 37. ORI discussed Dr. Popovic's explanations made in written submissions earlier in the investigatory process that 10% was the equivalent of "very few cells" and that he had perhaps averaged his and

Ms. Read-Connole's reading to develop the 10%. Ex. H-84 at 96; Ex. H-157 at 21.

At the hearing, Dr. Popovic explained that he had attempted to explain the basis for the 10% before he had fully recalled the meaning of Ms. Read-Connole's notes. Tr. at 2456-58, 2524-26. We do not find Dr. Popovic's explanations to be numerous or changing, or to call his credibility into question under the circumstances here. It is reasonable that for one insignificant data point of the paper it would be difficult to remember the precise circumstances after seven years. We note here that Ms. Read-Connole did not clarify the meaning of her notation until May of 1992. Ex. H-63. We find credible Dr. Popovic's statement that at first he did not remember what happened with clone H35, but that later he recalled that there had been difficulty in quantifying low IFA values with the first rabbit antisera and that he had quantified the 1:2000 dilution at 10%. The original investigation here focused on allegations considered to be far more serious, so it is understandable if Dr. Popovic did not focus his powers of recall on this issue until it became clear that it might become a basis for an action against him.

In sum, we conclude that ORI did not prove by a preponderance of the evidence that Dr. Popovic intentionally falsified the 10% value for clone H35, or that it was even untrue.

D. ORI'S OTHER ARGUMENTS

1. ORI's other arguments were not properly raised here.

During the course of the hearing and in post-hearing briefs, ORI attempted to raise other issues concerning the accuracy of the Science paper. We found introduction of these issues to be inappropriate and untimely. In effect, ORI was attempting to reopen allegations of misconduct on which it had not made findings of misconduct in the final report which served as the basis for this proceeding. While ORI attempted to characterize these matters as merely going to Dr. Popovic's credibility, ORI had not even identified these matters as part of the pattern of conduct it said (in the Offer of Proof submitted several weeks prior to the hearing) it would show to prove Dr. Popovic's intent. Thus, Dr. Popovic did not have fair notice that these issues would be addressed, and we ruled that they were outside the scope of this hearing. We nonetheless discuss these

issues briefly here since ORI continued to press them and since they were raised in a public forum.[48]

2. ORI did not show that the paper lacked necessary detail.

ORI contended that the methodology in the paper was not detailed enough to be reproducible and that this lack of detail was intended to obscure the "pooling." First, we fail to see how the paper can be said to obscure the fact that Dr. Popovic pooled samples when the paragraph in question reveals that the infection of the parental cell line was done with fluids harvested from "patients with AIDS or pre-AIDS." Ex. H-5 at 498. This clearly reveals that it was not an isolate from a single patient which grew on the parental cell line.

The criticism that the paragraph lacked essential detail on methodology is based in part on ORI's erroneous view that pooling should be considered an essential part of the methodology. As discussed above, however, the paper also gives instances of infection with single patient isolates and these infections are discussed in somewhat greater detail in the legend to Table 2. The question of reproducibility was raised primarily in testimony by Dr. Martin. For reasons discussed above, we found him not to be disinterested generally. His specific testimony on this point was not persuasive since he admitted that he had not tried to reproduce the experiment, and that he was "overlaying this by what I know in 1993, in terms of how these systems worked." Tr. at 1334. Moreover, he limited his discussion of the methodology of the paper to the one paragraph on the infection of the parental cell line (apparently because, in response to an inquiry from him, Dr. Gallo had drawn attention solely to that paragraph). Tr. at 1322-23, 1361. Dr. Martin seemed unaware that further details and references on infecting a cell line through cell-free transmission and cocultivation were given elsewhere in the paper. Also, he acknowledged that, while he was a stickler for details, lack of detail is often a problem in scientific papers. Tr. at 1345-46, 1360, 1363. Many of the witnesses who testified here -- including ORI witnesses -- said that Science magazine in particular has

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[48] We have considered all of the evidence and arguments presented by the parties. If we do not specifically mention a particular argument or piece of evidence that means we consider it either covered generally in our detailed analysis or irrelevant to the issues we need to decide here.

space limitations that mean that Science papers are more apt to lack detail and be ambiguous than some other journals. See, e.g., Tr. at 792 (Sodroski); Tr. at 1642 (Schaffer).

ORI presented no evidence that any scientist who had tried to reproduce the experiment had failed, and the weight of the evidence in the record is that the methodology in the paper was sufficient. See, e.g., Tr. at 803 (Sodroski); Tr. at 2236 (Malkovsky).

Dr. Sodroski's testimony on reproducibility supported Dr. Popovic's position. Dr. Sodroski testified that one could never exactly reproduce this type of experiment since it is a "unique event . . . because the viruses that are present in that patient that particular time may not be the same viruses present in that patient in . . . two or three days" but that does not mean that attempting to establish a permanent cell line as was done here is "intrinsically unreproducible." Tr. at 777-79. He viewed the methodology of the paper as adequate and said that, if he had a problem, he would have telephoned Dr. Popovic. Tr. at 797; see also Tr. at 1642 (Schaffer). Dr. Martin admitted that he had never contacted Dr. Popovic to inquire about the methods. Tr. at 1360-61.

In sum, ORI did not prove by a preponderance of the evidence that the paper lacked sufficient methodological detail or obscured the pooling technique.

3. ORI did not prove that the pool did not in fact exist.

ORI also tried to suggest belatedly that the pool did not in fact exist (even though the ORI report discussed the pool as though it did exist). The existence of the pool is established in Dr. Popovic's notebook, however, which not only records the three infections with multiple culture fluids but also refers to pooling in the protocol for the series of experiments and to "pool" or "pooled" as a culture in the laboratory after the infection. Ex. H-19 at 16-17, 33, 34, 40, 44, 58. Although ORI implied through questioning at the hearing that maybe the notebooks were not authentic, ORI produced absolutely no evidence that would call the authenticity into question. Yet, ORI has had possession of the notebooks for years and the opportunity to subject them to forensic tests to date them. Moreover, the notebooks have various indicia of authenticity, including the interspersing of notes from Dr. Popovic and his technician Ms. Read-Connole and information obtained from others.

We also note that Dr. Popovic's descriptions of how he infected the parental cell line have been consistent, even when these descriptions were given before the investigation. See, e.g., Ex. H-42 (1985 memorandum to Dr. Gallo). Ms. Read-Connole testified that Dr. Popovic had informed her of the pool's existence in early 1984. Tr. at 2201 (Read-Connole).[49]

ORI even went so far as to argue at the hearing, however, that the Roche analysis established "conclusively" that there was no pool -- a position which would reopen the allegation of misappropriation of the French isolate. This was not only extremely prejudicial to Dr. Popovic but a misstatement of the results of the Roche analysis. The Roche analysis did not conclude that there was misappropriation of the French isolate; the Roche analysis concluded that the most likely explanation for the striking similarity between the strain called HTLV-IIIB (as grown on clone H9) and one of the French isolates (different from the one they said they were providing to the LTCB) was contamination, both in the French laboratory and at the LTCB, by a variant that was particularly virulent. ORI would apparently have us infer that Dr. Popovic intentionally contaminated the HT cell line with the French isolate and therefore would have us disregard his testimony on the pool.

Even if this were properly an issue before us, we would not draw such an inference based on what ORI presented to us, for the following reasons:

- The experts on whom ORI relied for its report did not draw such an inference.

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[49] ORI presented some evidence that pooling would not be a good technique because you might add inhibitors or viruses other than the one you are trying to isolate. Tr. at 480 (Richards); Tr. at 1506-07 (Schaffer). The mere fact that there might be some problems with the technique, however, does not mean that it was a technique Dr. Popovic would not have used, under the circumstances here. Dr. Sodroski testified that, while pooling was not optimal, "it appears that it may have been used here in a practical sense, to try to get some viruses that could grow on a cell line." Tr. at 746. This is consistent with the September 1983 protocol in Dr. Popovic's laboratory notebook, which says "If not sufficient volume of culture fluids; pool together several samples." Ex. H-19 at 16.

- Dr. Popovic did not have a fair opportunity to present evidence on whether such an inference is reasonable.

- The published article describing the Roche analysis contains qualifications and assumptions that have not been adequately addressed in this proceeding. See Ex. H-79.

- The relevant expert testimony ORI did present calls into question the reasonableness of such an inference. Dr. Schaffer testified that the problem with pooling was that what could come out of a pool might be a recombinant different from any virus that went into the pool. Tr. at 1626. Dr. Sodroski's testimony implies that some samples may not be amenable to PCR analysis. Tr. at 800. He also testified that one can initiate an infection with only one infectious particle if the right target cells are used, and that the viruses present in a sample taken at one time might not be the same as the viruses in a sample from the same patient taken two or three days later. Tr. at 777-79, 782-83. Together with the acknowledged fact that one of the patients whose sample did not yield any HIV virus in the Roche analysis seroconverted between June 1984 and June 1985, this testimony renders suspect any conclusions based on PCR analysis of aliquots of patient samples which were not the exact ones used in the pool even if they are from the same patients.

- Evidence regarding circumstances in the LTCB and contaminations elsewhere makes accidental contamination at least as likely a possibility as deliberate contamination. See, e.g., Ex. H-79; Tr. at 1089 (Berns).

- ORI did not establish any motive for Dr. Popovic to deliberately substitute the French isolate for the results of his pooling experiment, and we see no apparent reason why he would have done so, given the number of other isolates which he successfully grew on T-cell lines.

4. ORI did not prove that the paper inaccurately referred to patients with AIDS or pre-AIDS.

Finally, ORI suggested that the paper inaccurately referred to the concentrated culture fluids as being from "patients with AIDS or pre-AIDS"; ORI argued that some of the individual patient samples were from patients who were identified in the LTCB's log of patient samples solely as "hemophiliacs" or "homosexuals" and who therefore should not be considered as "pre-AIDS." This argument is based primarily on Dr. Martin's testimony that a patient has "pre-AIDS" only if the patient has certain clinical symptoms and does not include "at risk" groups. See Tr. at 1327. In our view, this testimony attempts to impose a more current definition of pre-AIDS than the definition in the paper itself. The abstract of the paper defines pre-AIDS as "signs or symptoms that frequently precede AIDS . . . ." Ex. H-5 at 497. Dr. Sodroski and others testified that, at the time the paper was written, the term "pre-AIDS" could reasonably have included persons because of their status, rather than specific clinical symptoms. Tr. at 620-22 (Sodroski); 846-47 (Hadley). Moreover, although the sample log on which ORI relied contains only cursory descriptions of the patients, the notebooks as a whole indicate that samples from a variety of patients were being provided to the LTCB either because of their diagnosis as having AIDS, their clinical symptoms, or their relationship with a patient who had AIDS. Thus, we do not find use of the term "pre-AIDS" to be inaccurate.

In sum, even if we considered these issues to be properly raised here, what evidence ORI did present and rely on for its arguments would not make a difference in our decision.

CONCLUSION

For the reasons explained above, we conclude that ORI did not prove by a preponderance of the evidence that Dr. Popovic engaged in scientific misconduct by intentionally falsifying certain methods or data reported in the

Popovic Science paper, or even prove that the methods and data at issue were untrue. Consequently, we conclude that ORI's findings are not supported and the proposed administrative actions are not justified.

Judith A. Ballard

Norval D. (John) Settle

Cecilia Sparks Ford
Presiding Panel Member