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Birch Bayh, Esq.
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1575 Eye Street, N.W.
Washington, DC 20005-1105

Dear Senator Bayh:

Attached is a list of some of the questions we propose to ask Dr. Robert Gallo during the course of his interview with the Chicago Tribune.

I hope you find these helpful.

Sincerely yours,

John Crewdson

JC/br

Enclosure

Questions for Dr. Gallo:

1. You have said you had RT and other data which suggested to you that CC was not a pure HTLV1 infection. Can you supply us with this data?

2. Is it correct that patient CC was the source of HTLV1 cell line CH and clone thereof CH-1?

3. When did you first discover that a positive EM of a virus other than HTLV1 in material from patient CC had been taken? May we have copies of this and subsequent EMs?

4. You have said that CC was cultured continuously from February of 1983. But a letter to you from John Lemp dated 12/11/85 states that "We were never able to maintain a successful culture of W5131 in vitro over the period of May 9 through July 20, 1983 -- possibly due to HTLVIII virus cytopathology." If CC was being cultured continuously in your laboratory, why did you not provide the workers at Electronucleonics with some of this cultured virus?

5. If CC was cultured continuously from February '83, may we have the culture history as well as the designations for cells cultures and cell lines infected, clones, dates of infection, RT measurements, etc.? When and how was CC characterized, and where was the characterization published? To whom was this isolate distributed, and when?

6. Given that CC has been in permanent (or continuous) culture for several months by May of 1984, why was the CC isolate not reported by name with RF the others in Table 2, Science 224:497?

7. You have said that Dr. Leibowitch brought you CC for the first time in the summer of 1982, but your lab records show this material was received in February of 1983. Is there a discrepancy here?

8. How soon after obtaining rabbit antisera to HTLV-III were you able to get serological data showing that CC had been doubly infected with HTLV1 and HTLVIII? May we see this data?

9. You have said that your initial AIDS serology reported in May of 1984 was complete by early January of 1984. But the draft declaration written by you for submission in the patent interference case states that "With H9 cell line functioning to produce HTLVIII, we were then able to examine more fully the many isolates which we had earlier placed in our laboratory freezer." If the H9 clone was not infected with HTLVIII until 2/25/84, how could the above-mentioned serology have been complete by January?

10. You have said you had hyperimmune sera "around Thanksgiving" of 1983. But Dr. Popovic's memo dated 9/6/85 states that the first antibody to HTLVIII was not obtained until Dec. 13, 1983, and the first ELISA not performed until Jan. 6, 1984. What was the reason for the delay?

11. If, as you point out, AIDS patient sera "reacts with everything," why did you perform a serology with serum from patients BRU and ET? Why did you report serology with patient ET but not with patient BRU?

12. On page 14 of the sworn declaration you gave in the patent interference case, you state that the French found only 20 percent or less of AIDS patient sera were positive for LAV, while your own laboratory reported 88-100 percent. However, we understand from several sources present at the Paris meeting on April 6, 1984, that you were advised then that Pasteur had done equally well with the blind samples from CDC. Is our understanding correct?

13. On which date did you first obtain HTLV-III anti-p24? When did you first acquire HTLV-III anti-p41?

14. If your lab had already obtained hyperimmune sera from a rabbit in November or December with HTLV-IIIb or components of that virus, why did Dr. Popovic make another HTLVpool infection on January 2, 1984 using some of the original patient materials and some new ones?

15. Even conceding that your lab grew LAV even only transiently, didn't you have enough ($10^8$) cells to make pellets for doing Western Blots? Were these blots done? Did you have enough LAV to try to raise LAV antisera in a rabbit? Was this done?

16. You point out that Pasteur did not have rabbit antisera to LAV until later, and you question their procedure of reacting their new virus with patient sera. But Pasteur did do RIPAs which showed protein homology among their various isolates. In your opinion, does this RIPA data provide support for the Pasteur claim that they were detecting the same virus in numerous patients?

17. In his October 1984 letter reporting RF as EM positive, Dr. Gonda explains that the virus was missed previously because the concentration of virus in RF cells was extremely low. How do you reconcile this low concentration of virus with what you claim were pronounced cytopathic effects and very high measurements of RT?

18. You have said that RF was "always positive" for RI, and Dr. Popovic's memo to you dated 11/28/84 states H4/RF was "consistently positive" for RT. But notes taken by E. Read dated 1/10/84 regarding RF state that "initial culture negative for P19, RT and EM. Cells co-cultured after 2 months in culture." The same notes also show RF was not even tested for RT until after 12/7/83. Are there discrepancies here?

19. A letter from Dr. Groopman to you dated 5/15/84 states that publication of the paper in question "depends on the identification of virus by Zaki in the Haitian man described in the case report." What is your interpretation of this report?

20. In your draft declaration you write that "We mass-produced two [isolates] immediately," IIIb and RF. You are quoted by Anne Fettner as saying of the RF isolate, "We mass-produced it at exactly the same time [as HTLV-IIIb]." Our records show that mass-production of HTLV-IIIb began on April 9, 1984. But Dr. Popovic's memo to you of 11/28/84 states RF "has not yet been selected for large-scale production and distribution." Can you clarify?

21. You have said that RF was in continuous culture from 1983, "even before HTLV-IIIb," but E. Read notes (p37) dated 6/28/84 state: "W7152 take out of freezer incubate 24 hours infect with H9." Is this inconsistent with RF having been placed into continuous culture before HTLVIIIb?

22. You have said you first detected the presence of HTLVIII in RF in December of 1983. But your laboratory's notes do not show antibody positivity for RF until 2/29/84 (and the isolate used is dated 2/18/84). Is there a discrepancy here?

23. You say RF was in molecular analysis and distribution "much earlier" than we know. On what date was HTLV-IIIRF deposited with ATCC? To whom was HTLV-IIIRF first distributed, and when? Dr. Popovic's memo to you of 9/6/85 states that RF was only then being cloned. Can you tell us when RF was analyzed? If RF was distributed in mid-1984, why did you wait until late 1984 before asking Dr. Hoxie for RF serum?

24. If you had what you considered positive EMs of RF from the fall of 1983, why weren't these published in May '84?

25. You have said repeatedly that "RF was patented at the same time" as HTLV-IIIb. Can you supply us with the application number for the patent on HTLV-IIIRF, or a copy of the patent if it was issued?

26. In light of the very high RT levels for RF, do you interpret that much lower readings for SN, BK and LS, the other isolates shown with RF in Table 2, Science 224:497, as positive? What was your laboratory standard for RT positivity?

27. If Dr. Gonda repeatedly reported RF as EM negative until Oct. 84, why did you show RF as EM ND in Table 2, Science 224:497?

28. When was the EM of RF published with the letter to Nature in May of 1986 actually taken? When was the HTLV-III-like particle discovered? By whom?

29. In your draft declaration you write that you had "restriction maps on numerous" of the 48 isolates (2 from the start). Which ones and when were these obtained? When were they published?

30. In your draft declaration you write that all of the 48 isolates were "stored in the freezer." But you have said elsewhere that "some" of the 48 were continuously in culture. Are these two statements inconsistent?

31. In your memo to Dr. Fischinger 8/19/85, you state that "We have numerous other isolates that have been put into permanent cell lines at the same time" as H9/HTLVIII. But a letter from Dr. Gonda to Dr. Popovic dated 12/14/83 shows that of 7 samples sent for EM only 2 -- HUT78/LAV and Ti7.4/LAV -- were then in permanent cell lines. Which other isolates were put into which other permanent cell lines as the same time as H9/HTLVIII?

32. In your draft declaration, you write of "several detections" of HIV with EM prior to Sept. 23, 1983. Which isolates were so detected, and when? Where were these micrographs published? May we see them?

33. In the galley proofs of Popovic et al (Science 224:497), SN, BK and LS are all EM negative. In the published article, they are all EM positive. When were the EMs in question obtained? When was the manuscript in question revised? Why does it carry no revision date?

34. SN is listed as EM positive in Science 224:497, but as EM negative in the table which accompanies the letter to Nature published in May of 1986. Which is correct?

35. In your draft declaration you write that of the original 48 isolates, 6 had positive EMs. Which six?

36. In Science in May 1984, you report that some of the 48 isolates "have now successfully been transmitted to the HT clones for production and detailed analyses." Which isolates, when were they transmitted and to which clones? When did production and analysis begin? Where were the results of these analyses published?

37. What is the culture history of the isolate called HTLV-IIIz, referred to in Arya et al (Science, August 84?) The citation he gives is to Science 224:500, but we cannot find a further reference to HTLV-IIIz in this or any other paper.

38. In your letter of March 5, 1984 to Ian Munro, you report that you have 40 new isolates of the AIDS virus that are being characterized. Which isolates, and of what did these characterizations consist? Were these characterizations ever published?

39. Mr. Salahuddin says he made "6 or 7" isolations of HTLV-III, of which micrographs were taken, before May of 84. Which isolates, and what happened to the micrographs? May we see them?

40. You have offered to supply us with the dates of the several isolations and micrographs taken of JS prior to March 30, 1984. We accept your offer.

41. In Table 2, which accompanies the article by Popovic et al (Science 224:497), SN is shown as RT 63x103 and RF as 2.5x103. However, your laboratory's notes show that these values have been reversed. Which values are correct?

42. In Table 2, which accompanies the article by Popovic et al (Science 224:497), SN and LS are shown as EM positive while in your laboratory's notes both are shown as EM negative. Which values are correct?

43. You have said you had "other independent isolates" of HTLV-III "at the same time" as HTLV-IIIb, "cloned and given out to people and we can prove that." Can we have the details?

44. You have said that "some" of the 48 isolates were mass-produced? Which ones, and when?

45. You have offered to give us data showing which isolates besides IIIb were placed in permanent culture in your lab, and when. We accept your offer.

46. You have said that you had "at least five" isolates in "continuous culture" before March 30, 1984. Can you supply us with the designations for these isolates (NOT patient names) and the culture histories for each, including the dates of original infection and the cell lines infected?

47. You say that the "5 or 6" viruses shown by name in Dr. Popovic's paper (Science 224:497) were placed in continuous culture and were also molecularly analyzed. Where has this data appeared? When was the analysis performed? Which isolates were involved?

48. If RF, SN, BK, LS and WT were among those isolates in continuous culture, why did Dr. Shaw not use any of these named May 84 isolates (from Table 2 in Dr. Popovic's paper) in his December 84 paper in Science?

49. Is C359/SN/H identical with the isolate SN you report in Science in May 84? If not, how was this isolate designated in your laboratory notes? Are we correct that C359 represents a cord blood cell culture and not a permanent T-cell culture? If yes, how do you interpret data showing C359/SN to be 16% positive for P19?

50. You are quoted in the American Medical News as having said that you "mass-produced and molecularly cloned and sequenced six isolates and shipped them to other labs around the country for confirmation." Which six isolates? Can you supply us with the dates on which mass-production began? On which cloning and sequencing were completed? Where were these sequences published? When were these six isolates shipped to other laboratories, and to which laboratories?

51. There is a graph in one of your May 84 papers showing five months of continuous RT positivity of an isolate in HT clone H4 (ie, in continuous culture since Oct. 30, 1983). However, Dr. Popovic's memo to Gallo dated 11/28/84 states that H4 was not infected with RF until after 12/29/83. Moreover, Dr. Popovic's handwritten notes appear to show that H4 was not infected with any other isolate until Feb. 25, 1984. The only infection of any permanent T cell line shown in your laboratory's notes during October 1983 are the infections of HUT78 and Ti7.4 with LAV. Can you explain these apparent discrepancies?

52. Can we see the documentation and data, including dated micrographs, that support your claim of EM positivity for SN, BK and LS in Table 2 which accompanies Popovic et all (Science 224:497). The micrographs which we were furnished under FOIA do not contain any dates. Is there supporting correspondence available from Dr. Gonda or whichever lab took these micrographs that does contain the dates they were made?

53. Your letter of May 1986, contains a table listing four HTLV-III isolates received from Dr. Redfield, three with accompanying micrographs, all of which are dated 2/4/84. Why were none of these isolates reported by name in your May 84 papers in Science? Why were these micrographs not published in those same papers? Are these "Redfield isolates" among the 48? If yes, we point out that your own laboratory notes dated 4/10/84 show rabbit antibody scores of RR=8%, KE=15% and SB=7%. Do you consider these scores to have been positive under the criteria established and reported for the 48 isolates?

54. The scientific history published in Nature reports that isolates SN, BK, CS and WT were all grown in H4. We cannot find any mention in the original Science papers of an isolate called CS. Moreover, those papers seem to indicate that isolate WT was grown in H9, not H4. Are there discrepancies?

55. What is the evidence for your statement that a sequence variation of I-2% as shown in comparisons of HTLV-IIIb and LAVbru is within the "known range of variation" of HIV? Do you still adhere to this statement?

56. In your draft declaration you state that Dr. Sarngadharan took "the cell line producing HTLVIII" to Paris in June of 1984. But Dr. Montagnier has said that all he ever received from Dr. Sarngadharan was cell lysate, not live virus. Is Dr. Monagnier mistaken?

57. You have said your principal regret is that you did not analyze the LAV given you by the French before May of 1984, a procedure which would presumably have established that both it, and HTLV-III, were isolates of the same virus -- if not the same virus. But a memo from Dr. Popovic to Dr. Streicher dated 8/19/85 does state that "We later tested our reagents against this cell line and eventually showed that LAV and HTLVIII had cross-reactive major properties." When was the above-mentioned analysis conducted? Were the results ever published? If yes, when and where?

58. In a memo to Gallo dated 11/26/86, Dr. Popovic states that Ti7.4/LAV "was used for comparative studies with H9/HTLVIIIb (protocols will be provided at a later date from Dr. George Shaw and Dr. Beatrice Hahn)." When were these comparative studies performed? What was the result? May we have the protocols mentioned?

59. You have said that H9/HTLVIIIb and LAV were maintained in "the same laboratory." But Dr. Popovic's memo of 9/6/85 states that "the development of H9/HTLVIIIb was almost entirely confined to the tissue culture room 6B03A where no LAV was ever used." Is Dr. Popovic incorrect?

60. In his HIV sequence paper in Nature, Dr. Ratner cites unpublished RE data from Dr. Shaw showing that an isolate of HTLV-III did not change over a period of several months in culture. Were these data ever published? If so, where?

61. In your memo of 8/19/85 to Dr. Fischinger, you say there are 144 nucleotide differences between LAV and BH10, which "probably represents the similarity within the known range of variation viruses isolated from different patients at different times." As a basis for this statement you cite data from Dr. Reitz, Dr. Shaw and Dr. Wong-Staal. Was this data ever published? May we have the citations?

62. You report 144 nucleotide differences between HTLV-IIIb BH10 and LAVbru. We count only 87 nucleotide differences between these two isolates. Is our analysis correct?

63. Did you ever ask or adivse Dr. Murray Gardner to withhold or delay publication of data showing close homology between HTLV-IIIb and LAV?

64. You have said previously, including in your sworn deposition given in the patent interference case, that you have two pairs of isolates which show greater sequence homology than LAV and BH10 (including one pair which differs by only 80 nucleotides). In your draft declaration you mention having "four pairs which are very similar." In our most recent conversation you mentioned only a single pair, taken from two gay men in New Jersey (the "New Jersey pair.") What became of the three other pairs? When do you plan to publish your data on the New Jersey pair?

65. On what basis do you propose that BH10, BH8, HXB2 and HXB3 were originally independent isolates from different individuals rather than from a single patient? Do you still agree with this interpretation?

66. At the 3d International Conference in Washington last year, you presented data showing an RE map of JY-1, an isolate which you said then was closer to HTLV-IIIb than is LAV. This map, later published in Yourno et al, shows that HXB2 and JY-1 share two HindIII sites in the first 2600 base-fragment, and one HincII site at the end of the same fragment. However, the sequence of the first 2653 bases of JY-1 contains NO HindIII restriction sites, and no HincII site at the point indicated on the published map. In view of these discrepancies, is there a possibility that the original JY-1 could have been a contaminant of HXB2?

67. What is, or was, HTLV-IIIa?

68. You write in Nature (317:375) that HTLV-IIIMN and HTLV-IIISL differ from each other by less than LAV differs from HTLV-IIIb. The sequence of MN has now been published. May we see and compare the sequence of SL?

69. You have said you have data and photographs showing that all 10 of the supernatants pooled by Dr. Popovic induced cytopathic effects in vitro. Of what does this data consist? May we have copies?

70. We understand from a member of your staff that when 10 patient materials originally pooled by Dr. Popovic were subsequently tested for antibody to HTLV-III only one was positive. Is our information correct?

71. In your memo to Dr. Fischinger dated 8/19/85, you state that the material put by Dr. Popovic into H9, and from which H9/HTLV-IIIb was isolated, "was pooled from several patients who showed high RT activity in primary culture." However, your own laboratory's notes show that only one of the 10 pooled samples (W6233) was ever positive for RT. Is there a discrepancy here?

72. You have suggested that the nine supernatants identified as RT negative by us were defective in pol and were thus complemented by the one supernatant which was RT positive. Did you establish that in fact all or any of the nine other supernatants contained virus with defective polymerase? If so, has this rather rare event been reported?

73. You have previously pointed out that patient BRU from which the French team first isolated LAV did not have AIDS at the time, and that the French workers could therefore not claim a link between LAV and AIDS. We note, however, that your own laboratory's notes show that among the materials pooled by Dr. Popovic in his efforts to isolate HTLV-III were samples taken from patients with the following diagnoses: ARC and lymphocytopenia (W6233); diagnosis unknown (W6592); hemophilia (W7644); ARC (W7645); "probable pre-AIDS" (W7647), and chronic lymphatic leukemia (W7675). Can you explain how any virus isolated from a pool which included material from the above patients, such as HTLV-III, could have evidenced a clearer etiology to AIDS than LAVbru?

74. You have suggested that the striking homology between LAVbru and HTLV-IIIb can be explained by the fact that patient BRU and the Popovic pool patients "got infected at the exact same time." When were the 10 patients whose material was pooled by Popovic infected with HIV? Where were they at the time of infection? When and where was patient BRU infected?

75. In your draft declaration you state that the biopsy of BRU's lymph node occurred "in early 83," and that "HTLV-III was obtained from a New York homosexual in January 1983." Is it now your position that HTLV-III was isolated from one New York homosexual rather than from pooled patient material? May we have the sample control number or patient designator for this man? Is it correct that HTLV-III was obtained in January, 1983, as stated by you?

76. In your letter to Science published in April of 1986, you offer an explanation of the sequence homology between BH10 and LAV by stating that "many of our earliest HTLV3 isolates were all from specimens obtained in late 1982 or early 1983 from the East Coast of the United States." Was H9/HTLV-III1b/BH10 isolated, in whole or in part, from patient material obtained by you in late 1982 or early 1983? If yes, may we have the sample control numbers or patient designators for these patients?

77. In light of the data published by yourself and others showing gross variability in some portions of the HIV genome, and particularly in light of recent data in Nature showing variation over time in serial isolates taken from the same patient, what support now exists for the idea that a virus acquired by BRU in 1979 in New York would have remained essentially indistinguishable nearly four years later from viruses acquired by other patients at the same time and in the same place?

78. As part of the agreement which led to your name being listed in the company's Prospectus, you gave a speech to Cambridge Bioscience in early 1983. Can you supply us with a copy of this speech? How many speaking fees did you ultimately collect from Cambridge Bioscience?

79. In your memo to Dr. Fischinger dated 8/19/85, you say you proposed the involvement of a retrovirus in AIDS at Cold Spring Harbor in February of 1982. What was the occasion and to whom did you make this proposal?

80. In Scientific American for January 1987, you credit Dr. Essex with having "shown that a retrovirus called feline leukemia virus (FeLV) can cause either leukemia or immune deficiency in cats." Is this attribution correct?

81. Will you provide us with your tape of the CSH Banbury Conference?

82. Did the Kai Krohn patent ever issue? Can we have a copy?

83. What are the source and nature of the "independent lines of data" you have mentioned regarding the Kai Krohn peptide?

84. In Science (May 4, 84) you say that LAV was never successfully transmitted into a permanent cell line by your laboratory. But in his memo to Howard Streicher, Dr. Popovic says that he did succeed in transmitting LAV into permanent cell lines. Is there a discrepancy here?

85. Do you still subscribe to your published statement that RT measurements of 11,000 CPM is considerably less than the level necessary to infect a permanent T cell line (Science, 8 Nov 84 p643)? If so, how is it that permanent T cell lines could have been infected by SN, LS and BK with the RT readings shown in Table 2?

86. According to documents filed with the court, Dr. Popovic acknowledged receipt of the second sample of LAV from Institut Pasteur on Sept. 23, 1983. However, his handwritten notes show that he was culturing LAV on Sept. 22. Is this a dating error, or was he working with the LAV delivered to your laboratory in July of 1983?

87. You say some workers in your lab referred to the AIDS virus as LAV because no other name was known to them. Does this apply to handwritten notes for January 19, 1984 which show hybridizations of a virus called "LAV" against HTLVI and II? Or to the author of another note, dated 2/23/84, which records "DNA being obtained from LAV producer cells"?

88. In your draft declaration you state that "We could not keep LAV growing" in your laboratory but the handwritten notes from your laboratory also show references to LAV dated 4/3/84, 4/5/84 and 4/6/84 -- all several days after your four papers had been submitted to Science reporting the isolation of HTLV-III.

89. Dr. Popovic has said that HUT78/LAV was put into the freezer in December 1983. But a letter from Dr. Gonda to Dr. Popovic dated 2/24/84 shows that HUT78/LAV was sent for EM on 2/15/84. Moreover, a letter from Gonda to Popovic dated 2/22/84 shows that several cultures infected with LAV had been sent for EM: HTU/31, H/9, HUT, HUT78, P17.1/H31. If LAV was in the freezer, how could it have been used to infect these cultures?

90. In your letter to Dr. Chermann of June 15, 1984, you say your lab "did not mass produce" LAV because you "did not want to cross-contaminate our lines." Elsewhere, you say you were unable to get LAV to grow properly in any T cell line. Which statement is correct?

91. You are quoted in Science magazine for April 27, 1984 as saying that you were first able to mass-produce HTLV-III about 10 months before, which would have been in June of 1983. Is this date correct?

92. In the same letter to Dr. Chermann, you state that "The material you sent to us was inadequate for comparative studies." But Dr. Popovic's memo to Howard Streicher dated 8/19/85 reports that Ti7.4 was infected with LAV for the purpose of performing comparative studies. Were such studies ever in fact performed? Why does he omit to mention the simultaneous infection of HUT78 with LAV?

93. Why does Dr. Popovic write in the same memo that LAV was transmitted to Ti7.4 in December, 1983, when his own handwritten notes and other documents show that both HUT78/LAV and Ti7.4/LAV were sent to Dr. Gonda on Nov. 15, 1983, and were infected even earlier than that?

94. You wrote to Ian Munro on March 5, 1984 that "no one has been able to work with their (the French) particles..." But handwritten notes show that LAV cDNA was being used in comparative studies in early 1984. Is there a discrepancy here?

95. You say in your Science papers of May 1984, that LAV could not be grown in sufficient quantities in your laboratory to permit characterization. But handwritten notes obtained under FOIA show that "LAV" was being grown in liter quantities in March and April 1984 at Biotech Research Laboratories. Is there a discrepancy here?

96. Dr. Popovic's notes for 11/22/83 show that HUT78 and Ti7.4, already infected with LAV, are renamed MoV -- a designation you have said stands for MO variant. Are all subsequent references to MoV or Mo variant therefore in fact to HUT78/LAV and Ti7.4/LAV?

97. In Dr. Popovic's memo of August 1985 to Dr. Streicher, he reports that Ti7.4/LAV yielded reproducible but low-level LAV in December 1983. However, Dr. Popovic's notes show a 70% viability on 11/10/83 for Ti7.4, and 65% viability for HUT78. Are these figures consistent with a low-level virus yield?

98. You say you chose IIIb for mass-production over RF because the IIIb EMs were better than those of RF. You have offered to show us copies of all EMs of HTLV-IIIb which predate March 30, 1984. We accept your offer.

99. You published a letter in Science reporting that the composite photo of HTLVI, II and III actually contains micrographs of LAV, not HTLVIII. The same composite has appeared elsewhere: in JAMA (251:22, 2903), Hematology and Blood Transfusion (1985:29,318), AIM (103:5,681), Int. Symp. Prin. Taka. Can. Res. Fund. (1984:16), and the Cold Spring Harbor proceedings (Sept. 83). Were letters of correction also sent to these other publications?

100. What is the isolate which served as the source of the micrograph which appears with the first Science paper in May of 1984 (224:497)? Why was this source not identified with the micrograph?

101. You say you have proof that the redacted Gonda letter didn't come from your lab. May we also have the proof?

102. In your letter to Science published April 18, 1986, and also in an interview with Anne Fettner, you say you sent additional EMs of HTLV-IIIb not published in Science in May of 84 to Annals of Internal Medicine and The Lancet. However, the micrograph which accompanies the AIM article by Shaw et al appears to be the same one which appears in the second Science paper (224:500), except upside down. Editors of The Lancet say they never received from you any micrographs of HTLV-III. Is there a discrepancy here?

103. If it was decided at NIH not to publish the EMs with the Science letter of April 1986, why did the same micrographs appear two months later with the Nature letter? Who made the initial decision not to publish? Who at NIH advised you that it was "better" not to run the micrographs with the Science letter?

104. In your draft declaration you suggest the Pasteur lawyers obtained the redacted Gonda letter and EMs "by illegal entry and theft." Is there evidence to support such a charge?

105. You say that Dr. Fischinger corrected the erroneous micrograph which appeared with his paper in Current Problems in Cancer, though the correction w ade in the literature. How and where was this correction made?

106. In Table 2 of Science 224:497, you list SN, BK, LS and WT as EM positive. We have copies of correspondence from Dr. Gonda showing that, like RF, these samples repeatedly were negative for EM. Are there other Gonda letters showing positive EMs for these isolates which we do not have?

107. In your review article in Cancer Research v 45 (Sept. 85) you say you had both RT and "suggestions (by electron microscopy)..." of a new retrovirus in November 82. Which EMs were taken in November 82?

108. In the sworn declaration you submitted in the patent interference, you say your laboratory first detected in December of 1982 a new retrovirus in AIDS patient sera which appeared different from HTLVI and HTLVII, and that it is this new retrovirus which was ultimately called HTLVIII. According to your own laboratory's records, the only patient who scored better than RT negative during December 1982, was G. Watson, received by you from M.D. Anderson in Houston. When and how did you ultimately determine that G. Watson was infected with HTLVIII?

109. The table which accompanies your letter to Nature of May 1986, shows the above-mentioned isolate, GW, as positive for RT. However, we understand from a member of your staff that GW never tested positive for RT until it was removed from cold storage and re-tested in early 1986. Is our understanding correct?

110. In Cancer Reserch 45:4527s, you say you had five "isolates" of HTLV-III by February 83. Which five?

111. You have said you obtained "better data" in February of 83 than the data you acquired from GW in December of 1982, and that this data included evidence of cytopathic effect and higher levels of RT. Can we have this data?

112. You have said that "by February of 1983 we had isolates [of HTLV-III] that we could prove." How many and which ones? Given the absence of reagents at that time, how were these isolates "proven" in February of 1983? When did you report these isolates that were proven in February of 1983?

113. Logs maintained by Mrs. Richardson for the period of February 1983, and for several months in either direction, show no isolates that scored better than RT plus-minus. The Markham/Salahuddin summary provided by you shows no isolates as early as these. Did you have other isolates that scored better than RT plus-minus during this period? If so, which ones?

114. From which "several patients" did you, as you have said, detect a novel human retrovirus in February of 1983? Logs kept by Mrs. Richardson and Dr. Markham/Mr. Salahuddin show NO patient materials which were RT positive and also negative for both p19 and p24 during that period.

115. You have agreed to share with us data from Dr. Sarin's lab showing at least three isolations of a new, novel retrovirus in February of 1983. We accept your offer.

116. In the sworn declaration given by you in the patent interference case, you say you detected a new retrovirus in February of 1983 with positive RT, negative for p19 and p24, and positive EMs. Can you supply us with dated EMs, and confirming correspondence from the microscopist(s) involved, showing detections by EM of a lentivirus or other novel retrovirus of which you were aware in February of 1983?

117. In your letter to Nature of May 1986, you list isolate GW, dated December 1982, as RT positive, EM not done, and negative for both p19 and p24. However, the logs kept by Mrs. Richardson show that p24 was also "ND." Which is correct? Also, do you believe, in the light of Dr. Haynes's published data on the insufficiency of testing for anti-p19, that negativity to anti-p19 alone is sufficient to establish the absence of HTLV?

118. According to many of those present at the Park City meeting in February of 1984, you did not mention HTLV-III in your talk. However, the paper by you published in the Park City proceedings does mention HTLV-III. Is this the same paper you gave at Park City? If not, why did you substitute another paper?

119. The proceedings of the Cold Spring Harbor meeting of September 1983, contain a paper by you which refers to HTLV-III. Is this the paper you delivered at Cold Spring Harbor? If not, why did you substitute another paper?

120. The proceedings of the New York Academy of Sciences meeting of November 1983, contain a paper by you entitled "Correlation between Exposure to Human T-cell Leukemia-Lymphoma Virus-III and the Development of AIDS." Is this the paper you delivered at this meeting? If not, why did you substitute another paper?

121. Does your laboratory use any of the RT that is currently being produced by Genetics Institute under that company's collaborative agreement with NIH?

122. What was the date you received the Genetics Institute stock "in lieu of a speaking fee?"

123. Can you supply us with copies of the speech or speeches you delivered to Genetics Institute in return for the stock in question?

124. You have said that four papers published in Science v. 224 were "in progress" in January of 1984, and that the papers themselves were "ready to be communicated" at the time of the Park City meeting. But a memo to you from Dr. Popovic dated 9/6/85 shows that the central event reported by those papers, the transmission of HTLVIII to HT clone H9, as well as to the other HT clones shown in Table 1 of Science 224:497, did not occur until February 25, 1984. Furthermore, the above-mentioned table contains data ostensibly collected on the 14th day after infection, which would be March 10, 1984 -- more than one month after the Park City conference. Finally, your own laboratory's notes show that serology performed with the rabbit, BRU and ET antisera was not positive until 2/29/84 for isolate RF and not until 3/1/84 for isolates LS and BK, all of which are reported in Table 2 of Science 224:497. How could data described above have been known to you by the time of the Park City meeting on Feb. 7, 1984, or the papers themselves have been "ready to be communicated" at that time?

125. You have said that you began mass-producing IIIb in December 1983. However, the Washington Post reports that the first shipment of IIIb to Frederick was taken there by Dr. Arthur on April 9, 1984. Why did you wait four months befor sending the virus to Frederick for large-scale production?

126. At Mrs. Heckler's news conference of April 23, 1984, you said you had by then been mass-producing HTLV-III for six months (ie, since October 23, 1983). The only infection of any permanent T cell line with the AIDS virus during October of 1983 that is reflected in your notes was the transmission of LAVbru to HUT78 and Ti7.4. Is this the virus which you then mass-produced for six months?

127. In your draft declaration, you say Dr. Popovic had succeeded in using H9 to produce "relatively large amounts" of virus by November of 1983. But Dr. Popovic's notes show that H9 was not infected with HTLVIII until 2/25/84. Is there a discrepancy here?

128. Why does the genetic structure shown in Dr. Gonda's visna virus paper, which was accepted by Science magazine on 6 December 1984, report that the sequence of HTLV-III was not yet complete, even though this same sequence was submitted to Nature on 26 November 1984? Why does the Gonda paper not show the 3'-orf feature that is also contained in the Ratner paper mentioned above?

129. Why was the structure of BH8 showing 3'-orf not included in the paper by Chang et al submitted to Science on 21 December 1984, considering that the Ratner paper reporting this feature of BH8 was submitted to Nature on 26 November 1984?

130. You have said that an effort to determine the cause of AIDS and involving Dr. Popovic was begun in your laboratory as early as 1982. However, the Popovic notes made public under the Freedom of Information Act show no entries dated earlier than 9/10/83. If earlier notes exist, may we have them?

131. Page 69 of Dr. Popovic's handwritten notes is dated 12/28/84. Is this date correct?

132. In your letter to Dr. Monagnier dated 7/3/84, you state that "We have several isolates of HTLV-III which are cloned..." What are the designations of these isolates and clones, and when was each obtained?

133. When did your lab begin cloning HTLV3? When was the initial clone made?

134. You say your discovery that your laboratory had isolated the AIDS virus was "kept as quiet as humanly possible for more than one reason." What were the reasons? When did you tell "the higher-ups in government" that you thought you had found the cause of AIDS? Who did you tell, and when?

135. Can you supply us with the precise dates that work was begun and completed on each of the four Science papers published in May of 1984?

136. What was the date of the March 1984 celebration of the isolation of HTLV-III? Why was this date chosen for such a celebration? Who was present at the celebration? Where did it take place?

137. On the strength of what evidence did you add the line to Dr. Montagnier's first paper suggesting that LAV was closely related to HTLV?

138. In your memo of 8/19/85 to Dr. Fischinger, you say you "encourage[d] publication" of the first Pasteur paper together with your own and Dr. Essex's paper, "although this mean[t] holding up Dr. Gallo's and Dr. Essex's articles for several weeks." The Pasteur paper was submitted to Science on April 19, 1983. The Essex paper was submitted March 24, but revised and submitted again on April 11, eight days before submission of the Pasteur paper. The paper by Gelmann et al was submitted March 3, 1983, but revised and resubmitted on April 18, one day before the Pasteur paper. The paper by Gallo et al was not received by Science until 19 April, the same day as the Pasteur paper. In view of this sequence of events, for how long were each of the above papers held up?

139. You have said that the first Pasteur paper was initially rejected by Science, only to be accepted after you arranged for review by yourself and some of your associates. However, Dr. Montagnier says he dispatched it to you [not to Science] via a Pasteur staff member on April 15, 1983. The paper was accepted on April 19, 1983. Given this time sequence, at what point was this paper rejected by Science? If such a rejection had taken place, would not Science have been bound to print the earlier submission date and to mark the paper as having been revised before acceptance?

140. What is the date of the first isolation of HTLV-1b?

141. On January 18, 1984, you submitted a paper to Science by Shaw et al reporting the isolation of an HTLV variant, HTLV1b, from an American homosexual with AIDS. The abstract submitted by Dr. Shaw for the Park City meeting refers to the same isolation, from cell line MC, from a young male homosexual with AIDS. However, Dr. Shaw's paper as published in the Park City proceedings reports the isolation of HTLV1b, clone MC-1, from a Zairian man with T-cell leukemia. The paper published later that year by Hahn et al also reports the isolation of HTLV1b, clone MC-1, from a Zairian man with T-cell leukemia. Is there a discrepancy here?

142. The Park City paper mentioned above also refers to isolate CH-1 as having been taken from patient CC. The paper published by Ratner et al in Journal of Virology identifies clone pCH as derived from cell line CR-1 (apparently HTLV-1cr). Is there a discrepancy here?

143. Why do RE maps of HTLV1b as published with the paper by Hahn et al mentioned above differ in 9/34 restriction sites from those shown in Dr. Shaw's laboratory notes?

144. You say you accepted "travel money once" from Immuno for an appearance at the University of Vienna. However, our records show that you have made more than one trip to Vienna on behalf of Immuno. Are our records incorrect? Did you receive no honorarium or speaking fee from Immuno on this occasion or any other?

145. In your opinion, does your participation in Immuno's research on wild-caught chimpanzees violate NIH policy?

146. Under what auspices did you, an officer of the U.S. Public Health Service, appear at a hearing before officials of the Viennese government in 1986 to testify on behalf of Immuno AG, a private corporation, about the need for vaccine research on chimpanzees?

147. In your interview with the New Scientist of Aug. 21, 1986, you state that you were "asked to see about doing some collaborative research with Immuno." Did that request come from someone at NIH? If so, who? What does the collaboration involve?

148. A handwritten document from your laboratory dated 10/24/83 shows that LAV was transmitted into HUT78, Ti7.4, SR2, CL7 and HOS. What are, or were, SR2, CL7 and HOS?

149. Dr. Popovic's handwritten notes show that Ti7.4/LAV and HUT78/LAV were renamed MO(v) on 11/22/83. Dr. Wong-Staal's handwritten notes show that the virus called Mo(v) on 3/29/84 was renamed HTLVIIIb on 4/12/84. In view of correlary data which suggest that it was the same virus in both cases, not different virus (two batches were pooled +(A-) and the concentration for both is 3.2x1010), were Ti7.4/LAV and HUT78/LAV simply renamed HTLVIIIb?

150. In your draft declaration, you state that any infection of Ti7.4 with LAV in your laboratory was "only transitory in nature." But a memo from Dr. Popovic to you dated 11/26/86 says that Ti7.4 with LAV in your laboratory was used for comparative studies with H9/HTLV-IIIb. Since H9/HTLV-IIIb did not exist until 2/25/84, does this mean that LAV continued to grow from mid-October 1983, until late February 1984?

151. A memo from Dr. Popovic to Dr. Streicher dated 8/19/85 states that "We were...never sent a cell line producing virus" by the Pasteur team. But we have copies of correspondence from you to Dr. Montagnier, dated July 1984, acknowledging the receipt of his BJ cell line containing LAV. Is there a discrepancy here?

152. A memo from Dr. Popovic to Dr. Streicher dated 8/19/85 states that Ti7.4 was infected with LAV in December 1983. But one of the documents released by your laboratory under FOIA shows that Ti7.4 and HUT78 were infected with LAV on October 24, 1983. Furthermore, Dr. Popovic's own notes show that HUT78 and Ti7.4 were infected in November 1983. Are there discrepancies here?

153. Where are Dr. Popovic's notes showing that LAV infections of HUT78 and Ti7.4 were frozen?

154. What were the "serious problems" you mentioned with the infected Pasteur BJ line?

155. You say you did not realize how different HTLV1 and HTLV3 were until the actual nucleotide sequence of HTLV-III was available. Did you have RE data showing dissimilarity before this? Was anyone in your laboratory performing hybridizations to 1, 2 and 3 in February and March which showed little homology with 1 and 2?

156. In the December 1984 paper by Shaw et al published in Science, "significant homology in the gag-pol region" between HTLV-III and HTLV-I is reported. Do you still believe this data is accurate?

157. If you agree now that the conditions of hybridization used by Dr. Arya in his August 1984 paper were too permissive, why did you not question these conditions at the time of the paper's submission?

158. What was the scientific basis for your assertion to Dr. Deinhardt and others in the Spring of 84 that your new AIDS virus is "closer to II than II is to I?"

159. You say one of the things that misled you early on about the role of HTLV-I in AIDS was the number of doubly-infected patients whose material you were receiving. Who, besides CC, proved to be doubly-infected?

160. You say you "never thought HTLV1 or 2 were the cause of AIDS." But we have a tape of you speaking live on French television from the CSH RNA-TV conference series in May of 1983 in which you clearly say the cause of AIDS could be <u>either</u> HTLV1 or a variant of HTLV1. Is there a discrepancy here?

161. If Dr. Robert-Guroff's data on antibody to HTLV-I core proteins caused you to doubt the Essex data, why did you hold up publication of her paper for nearly one year?

162. In May of 1983 you reported having found HTLV-I provirus in 2/33 AIDS patients tested. How many blots were looked at overall in your search for HTLV1 provirus? What efforts were made to ensure that these were not laboratory contaminations? Were tests performed for core protein antibody as well?

163. Dr. Popovic's memo to you of 9/6/85 states that "to avoid confusion we designated the cell line susceptible to and permissive for HTLVIII as HT." Of what confusion was Dr. Popovic speaking?

164. Dr. Popovic's memo of 9/6/85 states that "HT cells...were infected with...W6233, W6592 and F6367." But Dr. Popovic's handwritten notes show that it was HUT78, not HT, that was infected with HTLVpool. [The first reference in the handwritten notes to HT is 1/19/84.] Is there a discrepancy here?

165. If, as you have said, the cell lines in your freezer were so jumbled and mislabled that none of them were identifiable as HUT78, how is it that Dr. Popovic was able to infect a line that he identified as HUT78 with LAV?

166. You have said that when you published your first HTLV-III papers in May of 1984 your laboratory had already performed a comparative chromosomal analysis of HUT78 and HT and that there was no way of concluding they were the same. Why were these analyses performed? When were they performed? At whose behest and for what purpose? Why is the DNA fingerprint analysis currently being performed?

167. You have said that HUT78 and HL60 were available to the world "just like H9 is available to the world." Why has uninfected H9 not been provided to ATCC more than four years after it was cloned?

168. In Dr. Popovic's memo to you of 9/6/85, he reports that HT was cloned twice on 11/9/83. But Dr. Popovic's handwritten notes for that date reflect the "cloning of HUT78." Is there a discrepancy here?

169. Speaking in plenary session at the Fourth International Conference in Stockholm last June, you reported data showing a one-base difference between lymphotropic and monocytropic HIV. Has this data been published? Do you plan to publish it?

170. Did you originally publish the sequence of Fernando Gomez as SBL6669? Was this the result of a laboratory contamination?

171. May we have a copy of the original draft of the Ratner HTLV3 sequence paper as submitted to Nature on 26 November, 1984? Of the Shaw-Hahn paper on HTLV1B as submitted to Science in January, 1984?

172. In a memo to Dr. Streicher, Dr. Popovic reports that LAV was found to be negative for HTLVI-p19 and HTLVI-p24 in the fall of 1983. Is this correct? May we see this data?

173. Is the cell line CH-1, taken from patient C. Chardon, infected with HTLV1 and also HIV?

174. In early 1985 you and Dr. Montagnier both presented data on HIV serology at a meeting in Denver organized by Marvin Rich. May we have a copy of your data?

175. In early 1984, Institut Pasteur sent LAV cDNA to your laboratory. What was done with this material? Were any comparative studies performed using LAV and HTLV1, HTLV2 and/or HTLV3? What was the result of these studies?

176. Your laboratory notes show a "digest of AIDS and ATL DNA" in August of 1983. Where did you get AIDS DNA in August, 1983? May we see the results of this digest?

177. May we have a draft of the Gallo-Chermann agreement?

178. When we spoke on Sept. 26, 1988, you said the Pasteur group had published data reporting 40 percent of AIDS patients positive for LAV in July, 1984. But the paper to which you refer, published in Science 225 at 321, contains a note added in proof reporting ELISA data showing 95 percent positivity in LAS patients and 70-95 percent in AIDS patients. Is there some reason not to consider the ELISA data in assessing the accuracy of the Pasteur test?

179. During the same conversation, you said the Pasteur/Science paper reported homology "acrossthe genome" between LAV and HTLV2 and extensive homology between HTLV1 env and LAV. The only such references we can see are attributed to a paper in preparation by R. Narayanan et al, which apparently never was published. Are there other such references that are based on the work done at Pasteur?

180. During the above conversation, you said the Pasteur/Science paper mentioned above referred to LAV as a D-type virus. We can find no such reference in that paper. Can you supply it to us?

181. During the same conversation, you said the first Pasteur paper on LAV [Science 220;868] referred to LAV as an oncornovirus. I can find no such reference in that paper. Can you supply me with the reference you recall?

182. In an interview with BBC correspondent Martin Redfern on April 10, 1984, you made the following statement regarding HTLV-III: "We've characterized the virus partially, it clearly belongs to the HTLV family, it's related to both HTLV1 and 2, it hybridizes across the genome--that is, there's nucleic acid similarities across the whole genome of HTLV1 and 2." What evidence then existed to support the statement that HTLV-III "hybridized across the genome" with HTLV1 and HTLV2? Was this data ever published? If yes, where?

183. In this same interview, you made the following statement: "We made what I would call a reasonable advance about eight or nine months ago. We developed a cell line in our laboratory which is susceptible to infection by these viruses and which mass-produces this virus and doesn't have the cytopathic effect as a consequence. Therefore, for the first time we could mass-produce it, we could develop reagents, antibodies, molecular probes, and go back and analyze all the ones that we couldn't grow and we typed them as the same." Since the interview took place on April 10, 1984, you appear to be saying that a permissive cell line for HTLV-III was developed by you during the period July-August of 1983. However, the first reference to HT in your own laboratory's notes is Nov. 9, 1983 (Popovic notes, page 59), and the first reference to clone H9 is dated Jan. 27, 1984. What t cell line or lines were infected with AIDS virus by your laboratory during the period July-August, 1983?

184. You have maintained in published review articles that HTLV-IMB and HTLV-ICR are "highly related" to one another. How do you interpret the reported 17% hybridization of DNA from fresh leukemic cells of HTLV-I patient MB with DNA from HTLV-I patient CR, in light of a control value of 10-13 percent reported for hybridizations with normal human DNA? Why was HTLVcr DNA never hybridized with DNA from fresh leukemic cells from patient CR? Why was HTLVmb DNA never hybridized with DNA from fresh leukemic cells from patient MB?

185. In Reitz et al (PNAS 78:1887) tables 1-4 contain the results of 48 hybridizations using HTLV-1cr, including 7 to human tissue. But any hybridization to fresh leukemic cells is missing. You are quoted as having previously explained that you did not have enough probe to perform this hybridization. Is this explanation correct?

186. You have said that HTLV-Icr was isolated in 1978, though not reported until 1980. However, it appears to us that cell line CTCL-3 was in fact not set up until after May of 1979, when patient CR returned to the hospital. Is there a discrepancy here?

187. In Kalyaranaman et al (J virol 38:906) it is stated that "nucleic acid hybridization studies have identified HTLVcr related sequences in the DNA of fresh neoplastic lymphoblasts from two patients, one a child with T cell acute lymphoblastic leukaemia and the other a woman with the leukemic phase of Sezary syndrome." The citation given is to "Poiesz et al submitted for publication." The paper by Poiesz et (Nature 294:268) reporting the isolation of HTLVmb does not contain hybridization data to DNA from a child with T-cell leukaemia. Where is this hybridization data published?

188. Do you recall asking Mrs. Ersell Richardson to throw out the culture that subsequently became HL60?

189. What is your current view of the results reported in Nature 229:927, Gallo, Yang and Ting: RNA dependent DNA polymerase of human acute leukemic cells.