We have repeatedly communicated our views on the violations of the rights of scientists in the USSR to your authorities, including President Aleksandrov of the Soviet Academy of Sciences. Our entreaties have elicited no response. Hence, we are obliged to take more stringent steps.

Please be assured that our high regard for our Soviet colleagues is unblemished by the circumstances that have led us to our own course of action. We are unwavering in our esteem of them and of their contributions to science. We are aware that current impediments to scientific collaboration are not of their doing. We should like to make clear also that our actions are taken independently of our government, and without any influence whatever from governmental officials or authorities. . . .

Professor M. Goodman, Professor H. Morawetz, and Dr. P. W. Morgan, each of whom concurs with the substance of this letter, have previously communicated their decisions in regard to the Tashkent meeting to the Organizing Committee.

In addition, Professor Charles G. Overberger, vice president of the University of Michigan, has canceled commitments for the meeting in Tashkent for related reasons.

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Hayflick's Reply

When one is engaged in suing, one cannot always talk. Now I wish to reply to two articles by Nicholas Wade (News and Comment, 9 April 1976, p. 125; 1 Oct. 1976, p. 41). This reply was sent to *Science* in November 1977 and was ultimately submitted by them to eight referees. I was then asked to respond to numerous questions and to document many of the statements that appear below. Wade's articles and his reply that follows these comments were (i) not sent to outside referees, (ii) not documented with references, and (iii) published without an 11-month delay. Specifically, Wade's articles discuss an investigation by James W. Schriver, director of the Division of Management Survey and Review at the National Institutes of Health (NIH), of my management of the human cell strain WI-38 and a subsequent conference held at NIH to discuss the present state of the cells. My previous silence should not be taken for acquiescence, timidity, or guilt: Wade was only doing his job, but in so doing he was led to present as fact many of the wholly unmeritorious allegations with which I was suddenly confronted. These allegations have done damage to my reputation, though fortunately most of my colleagues familiar with the truth have dismissed them. That they are unmeritorious should be quite evident from the fact that, were they true, I could have faced a suit, or criminal charges, or both. In fact it is I who have been obliged to sue.

Naturally I wish to clear my own name. I am equally concerned, however, about the general threat which the treatment I received poses to *all* federally funded scientists. Do my colleagues know that it is possible for government bookkeepers to wander freely through research laboratories, look at records, question personnel in the absence of the principal investigator, report that investigator as a felon, and then have their unsubstantiated word believed by university administrators? Do they know that unrebutted reports by such individuals, making not only moral but scientific and legal judgments, can be circulated gratis to the press and to their colleagues by a process of leakage and invocation of "freedom of information"? Or that when one demands equal time, these individuals will indeed offer to circulate one's reply—at a charge of $11 a copy? And that in consequence one must dignify such conduct with a reply in *Science*?

These circumstances can lead inevitably to the indiscriminate destruction of scientific reputations because no system of safeguards now exists. Most scientists are unaware of the extreme vulnerability of their reputations even to unfounded allegations of wrongdoing. I implore my colleagues to seek procedural safeguards that will prevent nonscientists and the press from unjustly destroying the reputations of innocent people.

Wade's choice of a title for his article about me was designed, in my judgment, to attract readers' attention by casting a dispute in an unnecessarily sensational light. Such ploys are more typical of the sort of yellow journalism that is uncharacteristic of *Science*. The word "Tragedy" used by Wade in his title is more applicable to his reporting than to my situation because he produced an incomplete and inadequately researched article. Wade's characterization of the "Fall" of WI-38 is an unfounded opin-

ion. The usefulness and level of distribution of these cells has continued undiminished in the 2½ years since he inaccurately suggested that they were then to be eclipsed.

In a separate court case, NIH lawyers maintained that original research records are the property of scientists themselves. The U.S. Court of Appeals in Washington, D.C., upheld that position. Nevertheless, Schriver, without my authorization, read, copied, and distributed copies of my laboratory records. Although I have nothing to hide, my colleagues who may resent such an intrusion should know that an NIH investigator who is not a scientist may seize their laboratory records, interpret them as he wishes, and have that interpretation amplified by the press. My colleagues may also be interested to know that, since my suit was filed in 1976, claiming a violation by NIH of the Privacy Act, investigators' documents released by them under Freedom of Information Act inquiries now have the names of individuals and other identifiers deleted. That was not done in my case.

1) Fundamental to Wade's point of view is his apparent assumption, without a shred of fact, that NIH owns the WI-38 cells. There is documentary evidence (*1*, *2*) and several witnesses can testify (*1*) that Leon Jacobs, Associate Director for Collaborative Research at NIH, told representatives of Merck Sharp & Dohme in 1974 that he had consulted with NIH counsel and had concluded that "on the advice of counsel the cells belonged to H[ayflick]" (*2*). Competent, independent legal counsel also shares this opinion. Wade's statement that "there does not seem to be much dispute that the cells were developed on an NIH contract and hence were government property" is an unqualified legal opinion by him. The contract in question has no such provision, as the then NIH Project Officer has clearly stated (*2*) and as Leon Jacobs has confirmed (*1*, *2*). The concepts and procedures used in the contract were not patented but were based on a prior art, discovered by serendipity and published by me several years earlier; hence they are in the public domain. If Wade is right in stating that title to WI-38 was vested in the government, not only are the dozens of establishments who were and still are selling WI-38 for profit selling government-owned property, they are also profiting from several other continuously propagable cell populations that have been developed with public funds. And if the cells were government property, why has NIH chosen to pay for WI-38 cultures since the cells first went on public sale in 1968? Also, in 1971 my NIH project officer widely circulated a memorandum to the effect that a charge would be made to those recipients of WI-38 not using the cells in research on aging. What Wade fails to say is that no charge was ever made for cells distributed under the terms of my contract. That contract stipulated that WI-38 would be distributed gratis to researchers in the field of aging, a provision that was scrupulously adhered to. Finally, it was I who first requested clarification of this entire matter with Ronald Lamont-Havers, then Deputy Director of NIH, in October 1974; the resulting inquiry was made at my request, Lamont-Havers' denials notwithstanding.

2) According to Wade, "it now appears that sufficient [WI-38] stocks [are available] only for the next several years" (9 April 1976, p. 125). Thousands of WI-38 cultures have been sold weekly by several commercial organizations in the 2½ years since Wade made this statement, and there is no reason to believe that this will not continue for many more years. How much credence, then, can be put in the statement attributed to the Bureau of Biologics (BOB) by Wade

in his April 1976 article that "WI-38 stocks will last only a few years at best"? A BOB representative has said that WI-38 is not in short supply and that more than 200 ampules of passage 12 are on hand at the American Type Culture Collection (ATCC) alone (*3*). This was revealed (*4*, p. 20) at the NIH meeting on WI-38 held in September 1976 and attended by Wade before he wrote his second article, but he made no mention of it. The present WI-38 stocks should satisfy the needs of vaccine manufacturers for at least 10 years, a fact that I have steadily maintained. There is no "acute" supply situation, as Wade states. An informal census taken by me indicates that about 2000 frozen ampules of WI-38 at passage levels below 17 are stored in laboratories throughout the world. For example, the Connaught Laboratories in Toronto, Canada, have sufficient ampules to meet all anticipated vaccine production requirements for at least the next 25 years (*5*), Wyeth Laboratories has sufficient WI-38 to meet their needs for at least a decade (*6*), and the Japan Poliomyelitis Institute has 500 ampules of WI-38 on hand (*7*). In view of the availability of WI-38 in so many other places, now, and for an indefinite future period, how much credence can be put in Wade's statement that a contract I negotiated with Merck Sharp & Dohme for their future purchase of WI-38 cells "might have given Merck a near monopoly on a vital world resource"?

3) Wade states that "the contamination [of WI-38] was not reported to NIH or the government vaccine authority." To inform the Division of Biologics Standards (now the BOB) of the contamination in 1968 would have been preposterous, as Wade surely could have reasoned from his description of their negative attitude toward the cells. They were totally disinterested in WI-38 and bitterly fought its use in virus vaccine production for the entire decade 1962–1972. It is therefore with some satisfaction that I note how our ultimate success in persuading them of its usefulness resulted in their active participation in the removal of WI-38 from my laboratory in 1975. The implication by Wade that the contamination of some ampules of WI-38 was kept secret by some conspiracy is absolutely false. Not only were Frank T. Perkins, then Director of the Division of Immunological Products Control in the United Kingdom, and his staff aware of the fact (they themselves first discovered it), but written notification (and subsequent written acknowledgment) was also given by me to the Principal Investigator on the NIH contract under which

WI-38 was first stored and distributed. Perkins headed the only control authority in the world which was then interested in using human diploid cells as a substrate for vaccine production. It was my judgment at that time that the key people interested in the cells were informed. It was not my responsibility to inform NIH, and, indeed, there was no one at NIH who would have been the least bit interested. Neither Perkins nor I chose to make the matter public because to do so could have produced the same kind of unnecessary alarm that Schriver's report and Wade's amplification of it almost created. Telling the new director of the BOB would have been as preposterous as telling the old director; neither cared about our work, and both fought our efforts to have the cells used for vaccine preparation.

4) The contamination of some ampules of WI-38 never has been in any way a threat to vaccines made in cell cultures prepared from sterile ampules. Wade also omits mentioning that a number of ampules of the MRC-5 cell strain, which is also used currently for the production of human virus vaccines, were found to be contaminated (*8*). This revelation was made 7 years after MRC-5 was described and at the September 1976 meeting attended by Wade, but it was inexplicably ignored by him in his October 1976 article. Wade's discussion of the WI-38 contamination makes only brief mention of this being a pioneering effort in 1962, when (i) no laminar flow cabinets existed; (ii) no antibiotics were used in the preparation of several hundred cultures; (iii) hundreds of ampules were hand-sealed by inexperienced workers in my absence; and (iv) no automatic, slow-freezing equipment was used. In spite of these handicaps, the range of contamination is not much different from that found so far in other human diploid cell strains that were produced much later under substantially improved conditions (*8*).

5) Wade says that "the general custom [in vaccine production] is always to start with initially sterile cells. This is the practice followed by the British vaccine authority, for example, with the live polio vaccine made from WI-38 in England. . . ." No vaccine has ever been knowingly prepared in WI-38 or MRC-5 cells derived from a contaminated ampule. If the practice in the United Kingdom is as Wade says, then why is MRC-5 still used for vaccine production? MRC-5, like WI-38, consists of a pool of ampules, some of which have been found to be bacterially contaminated. How then can Wade logically justify his statement that "the credit for the next generation of vaccines will go to MRC-5 instead of to Hayflick and WI-38," or his remark that "J. P. Jacobs [of the MRC] intends to use the MRC-5 cells, rather than take risks with cleaned-up WI-38 cells," when most of the WI-38 ampules used for vaccine production were original ampules opened and tested by Jacobs himself? How reasonable is it for Wade to ask, "Is it possible that vaccine makers have in the past received antibiotic-treated, not sterile and untreated, cultures of WI-38? Jacobs says there is no way to tell." Jacobs is, after all, in the best position to know, because he reconstituted and sterility tested all of the original WI-38 ampules used for vaccine production in the United Kingdom, the United States, and several other countries. In Yugoslavia, the Soviet Union, and France, original ampules were often opened there. Not only has contamination been reported in MRC-5, MRC-9, in the virus seeds used to make vaccines, and in more than 50 percent of primary monkey kidneys currently screened for use in polio vaccine manufacture, but in several human virus vaccines currently in use. Many of these are undoubtedly contaminated with bacteriophage because tests are not designed to detect all known types (*9*). In 1974, I requested that the BOB inspect my laboratory and records for the purpose of determining whether WI-38 distributed by me was being reconstituted under conditions that met requirements for vaccine production, a use not always made known to me by colleagues requesting cultures. The BOB refused my invitation.

There is nothing sinister about decontaminating cell cultures. In fact, decontamination is an accepted procedure for which a sizable scientific literature exists. Even the ATCC, where WI-38 is currently stored, distributes several "cleaned-up" cultures, including a cell line that "has been used extensively in cancer chemotherapy screening tests" (*10*). Virtually all cell cultures in use today (including those used for vaccine production) are "cleaned-up" because they or their progenitors have been grown in filtered and antibiotic-containing media. Almost every pool of bovine serum, trypsin, and synthetic media in which cells are grown is "cleaned-up" by filtration; serum is usually initially contaminated with from 10^5 to 10^8 bacteria per milliliter. Antibiotics are used at some stage in the preparation of almost all human virus vaccines, thus the use of "cleaned-up" cultures containing the cadavers of dead microorganisms is a universally accepted practice—even for the manufacture of human virus vaccine.

There is no need to reiterate this formally to specialists in the field. Nevertheless, all WI-38 used for vaccine production was sterile from the very beginning as determined by standard tests.

6) The contamination issue, is, of course, a diversionary charge. Wade implies that a statement that I made to a U.S. Senate subcommittee was false, which is far more serious. I said that "The human diploid cell strain WI-38 tested in hundreds of laboratories throughout the world has never been found to contain an indigenous contaminating microorganism." That statement is as true today as it was when I made it on 20 April 1972. Indigenous, as used by microbiologists, means that a microorganism is already present in the original cells before removal from the animal and is subsequently carried along with the cells as they are cultured (*11*). My statement was made to distinguish WI-38 from primary monkey kidney cells, in which indigenous microorganisms (viruses) *are* often found. WI-38, like most other widely used cell populations, has often become contaminated after laboratory culture of the original tissue but, as I accurately stated, it "has never been found to contain an indigenous contaminating microorganism."

7) Wade describes my record-keeping habits as "haphazard" or "incomplete," and says that they ". . . might leave something to be desired for the custodian of an important cell line. . . ." What are Wade's qualifications for making this judgment? He has never even seen my laboratory records. There are no specific guidelines for record-keeping that research scientists follow. Probably Pasteur's or Fleming's records "leave something to be desired." Absent any agreement to the contrary, the right to keep research records as one sees fit is as inalienable as is the right to do research itself. Proper peer view is the accepted standard for the evaluation of both. My research records, which always have been made available to site visit teams and project officers, have been accepted without criticism. If scientific data and research laboratory record-keeping are now to come under the scrutiny of government accountants and the press, then their influence on scientific research is more pervasive than most scientists have suspected.

The "important cell line" WI-38 reached its importance "as a valuable national and international resource" (Wade's words) only after 10 years of effort by a few dedicated people, none of whom included BOB personnel. Wade fails to note the irony and expected bias in the opinion offered that my record-keeping was "haphazard," when that judgment was probably expressed to him by the very people who fought the cells and the concepts for more than a decade. If there was concern about record-keeping of such "an important cell line," why did the BOB not ask to see those records before 1974 and refuse to inspect the records in 1974 when they were invited by me to do so?

Wade claims that "It is hard to find anyone who agrees with Hayflick's calculations" in regard to the designation of the population doubling level on the first culture prepared from frozen WI-38 at the eighth population doubling level. He goes on to say that "One cell culture specialist says his argument would be laughed out of court, another describes the reasoning as 'Jesuitical' and 'terribly feeble' " and that a vaccine scientist said that ". . . one would expect by geometric doubling to derive 100 9th passage ampules from 50 of 8th passage. . . ." James E. Shannon (of the ATCC) is quoted as saying that "the numbers specified . . . could not be fulfilled from Hayflick's stocks." Apparently Wade and the individuals he quotes are unaware of published international guidelines which state with notable precision how the ampules required by Merck can be made with ten or fewer ampules (*12*). Despite Wade's restatement of Schriver's unfounded claim, no WI-38 recipient ever received a culture from my laboratory whose stated population doubling level varied from general conventions and those international guidelines.

If Wade wishes to cite authentic misleading labeling of WI-38 population doubling levels, he would do well to look at the practices of the ATCC itself in this regard. Their labeling practices have caused WI-38 distributed by them to be assumed by many recipients over the past 10 years to be six to eight population doublings less than they actually were (*9*).

8) Wade uncritically repeats the view in Schriver's report that ". . . many ampules of the cells [WI-38] cannot be accounted for . . ." by my records, and he goes on to say that "in fact a total of at least 207 remain unaccounted for. This figure includes a discrepancy between the 339 ampules which Hayflick's records show were sent to the Medical Research Council in England and the 271 which the MRC's records show were received." Schriver's view appears to be based not on the state of my records but on his refusal to accept those records. Furthermore, Schriver relied heavily on the records of Pat Jacobs of the MRC laboratories, who, after Schriver's examination of those records, wrote to tell

me, "As I have said earlier, after our initial check (when I reported that only 131 ampules were received) we found a record book which had fallen behind a filing cabinet which showed that 100 ampules were received in 1962, a figure which we omitted from the first total shown [to Schriver] in our first inventory. Subsequently, as a result of the most thorough search possible, it is true we had to further revise our figures upwards. . . ." The fact is that all ampules of WI-38 can be accounted for.

Wade quotes Perkins as saying that, once the contamination in some pools of ampules was known, "Hayflick's response was that all the contaminated ampules would be destroyed." I did not say that to Perkins, and Wade fails to say that Perkins' laboratory at MRC itself did not destroy *their* allegedly contaminated ampules where, even now about 30 such ampules can be found. If Perkins made the statement Wade attributes to him, why would he not have destroyed his own pool of allegedly contaminated ampules? The fact that he did not is as he, himself, says "because an ampule of cells that had the bacteria killed by antibiotics was perfectly satisfactory for purposes other than vaccine manufacture. Many tests of vaccines are done on human diploid cells and a number of original isolations of viruses are done on these cells. It would have been very wasteful, therefore, to destroy the cells" (*13*).

Before the bacterial contamination was discovered, chlortetracycline was used prophylactically for 6 years to prevent mycoplasma contamination in cultures of WI-38 grown in Perkins' and my laboratories. After the bacterial contamination was discovered, both Perkins and I realized that this antibiotic had killed the bacterial contaminant during the previous 6 years, making its initial presence unknown to us or to hundreds of recipients of WI-38 starter cultures. The presence of bacterial cadavers, presumably present only in the first few subcultivations, apparently had no adverse effect on any measurable parameter during those previous 6 years. It was, therefore, reasonable to conclude that, in future, cultures treated with chlortetracycline could, at least, be safely used for purposes other than vaccine manufacture as Perkins points out (*13*).

Wade refused to wait until my 100-page rebuttal to the NIH report was prepared, although I asked him to do so. He claimed that to wait would result in his being "scooped" by others. I could not then discuss the matter fully with him: my suits were pending and common sense and a misplaced sense of profes-

sional decency prevented me from discussing the matter with the press *before* my rebuttal was sent to NIH. He instead wrote an inaccurate article, rather than wait the short time required for that rebuttal to become available. When my rebuttal did become available, Wade did nothing more than to note its existence in the pages of *Science* (News and Comment, 1 Oct. 1976, p. 41), where he did accurately state that it could be obtained from NIH for $11. It is a pity he did not add that Schriver's original, unrebutted, and seriously inaccurate report was available free from NIH to whomever asked for it.

The facts are simple. I have done no wrong, yet my reputation has been injured and my scientific work interrupted by Wade's amplification of a seriously inaccurate and incomplete report, authored by an individual who selectively excluded information in support of my position, and who, lacking credentials, reached both legal and scientific conclusions. Far more important than what has happened to me is the shadow that has been cast over American science. If my colleagues are content to see this kind of treatment accorded to one of their number, on the grounds that they personally are protected, they are living in a fool's paradise. If my experience does nothing else, it stresses the ease with which authority in science becomes abuse, and we can stop grinning smugly at events in the Soviet Union if we bear it. The *right* people to judge the conduct of scientists are their *peers*. I appeal to *their* opinion, and I appreciate the fact that *Science* has given me the opportunity to make that appeal.

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References and Notes

1. D. S. Brooks, Merck Sharp & Dohme, Inc., personal communication.
2. J. W. Schriver, "Investigation of activities relating to the storage, distribution, and sale of human diploid cell strains, WI-38 and WI-26. Working papers" (Division of Management Survey and Review, National Institutes of Health, Bethesda, Md., 1976). [These papers reveal an interview with the NIH Project Officer who said, "Since WI-38 cells were developed under a grant the passage 8 cells frozen at the Wistar Institute belonged to the investigator or investigators who developed them. . . . When the NCI contract was awarded to (the) Wistar Institute in 1962, there was no consideration that the WI-38 cells at Wistar were government property. The contract was not awarded to purchase the cell banks but to propagate, characterize and distribute the cells so that the widest possible use could be made of the cells in research. Cells up to the 8th passage at Wistar were not government property—but all cells propagated under the NCI contract became property of [the] government." No charge was ever made for cells distributed under the NCI contract.]
3. J. C. Petricciani, discussion, Joint WHO/IABS Symposium on the Standardization of Cell Substrates for the Production of Virus Vaccines, 13–15 December 1976, Geneva, Switzerland.
4. *Workshop on Cell Substrates for Vaccine Production, 9–10 September 1976, Transcript of*

Proceedings (National Institutes of Health, Bethesda, Md., 1976).
5. H. MacMorine, Connaught Laboratories, personal communication.
6. E. Rosanoff, personal communication.
7. K. Doi, personal communication.
8. P. Jacobs, in (*4*), p. 152.
9. L. Hayflick, "Mislabeling, contamination and other sins of cultured flesh," Joint WHO/IABS Symposium on the Standardization of Cell Substrates for the Production of Virus Vaccines, 13–15 December 1976, Geneva, Switzerland [*Dev. Biol. Stand.* **37**, 5 (1977)].
10. *Catalogue of Strains–II* (American Type Culture Collection, Rockville, Md., 1975), p. 27.
11. *American Heritage Dictionary of the English Language* (American Heritage, New York, 1975), p. 670.
12. *Proceedings, Symposium on the Characterization and Uses of Human Diploid Cell Strains, Permanent Section of Microbiological Standardization, International Association of Microbiological Societies, Opatija, Yugoslavia, 1963* (Institute of Immunology, Zagreb, 1963), appendix 6, p. 732.
13. F. T. Perkins, personal communication, 2 June 1978.

Despite the length of Hayflick's letter, he neglects to share with the reader some quite pertinent facts and implications. For instance, he stood to make about $1 million from sale of what the U.S. government regards as its property. Nor did he ever tell the government's vaccine regulation authority, the Bureau of Biologics, that the WI-38 cells he sold and distributed to foreign vaccine makers came from stocks which he knew to be in part bacterially contaminated. Nor did the Bureau, which with others had invested much time and money in certifying the safety of WI-38, ever discover that a mere handful of original ampules remained until it confiscated the stocks from Hayflick's laboratory in August 1975.

The central issue is Hayflick's stewardship of the WI-38 cells. The facts about this issue remain obscure to this day because Hayflick has all along followed the same tactic, that of talking only about side issues to a drumbeat charge that he is being treated unfairly. The letter above falls squarely into the old pattern. Hayflick refused to cooperate with NIH investigator James W. Schriver, despite being given some 9 months to do so. Only after the NIH report had finally been issued did Hayflick produce a rebuttal, claiming unfair treatment etcetera. The NIH prepared a lengthy counter-rebuttal in which Schriver concluded, in my opinion quite correctly, that there was no reason for him to change a word of his original report.

The Hayflick rebuttal did not address the central issue of his stewardship of the cells. Hayflick passed up another opportunity to explain this matter in February 1976 when he chose to resign from Stanford rather than exercise his right to a hearing before his peers. Hayflick was under the threat of dismissal as a result of an investigation conducted by Stanford independently of the NIH's.

Hayflick was no more forthcoming with *Science* than he was with the NIH. The article of 9 April 1976, nevertheless, includes almost everything that can fairly be said in Hayflick's defense in addition to laying out the case against him. In this regard it is baffling that Hayflick presents the comment about the inadequacy of his record-keeping as an attack when any reader can verify that it was raised as a possible defense of Hayflick against the otherwise devastating conclusions of the Schriver report. There is nothing I wish to change, add to, or subtract from the article as then written, including its title.

The side issues raised by Hayflick are not new, but are mostly the same as he has been presenting since 1975. Somewhat plausible at first hearing, the various contentions turn out to have no or very little merit. On the issue of who owns the cells, the "documentary evidence" referred to is apparently the account of a telephone call in which Hayflick contends that NIH associate director Leon Jacobs said the cells could be sold: Jacobs denies having said any such thing. Whatever the exact inventory of WI-38 cells, the point is that it is far smaller than the Bureau of Biologics had assumed. Hayflick's opinion that he had no responsibility to inform the Bureau of Biologics of the contamination of the WI-38 stocks is not shared by the Bureau of Biologics. As the *Science* article made clear, the point about the contamination is not that it occurred—that is not surprising considering the technology at the time—but that Hayflick never made the fact public. The MRC-5 situation has no relevance to Hayflick's stewardship of WI-38. Hayflick's statement to the Senate committee, despite his explanation, cannot be said to have presented the full facts of the contamination issue. The incident of the missing MRC record book was known to Schriver before he wrote his first report and does not alter his conclusions about the number of ampules unaccounted for.

One new argument in Hayflick's letter is that the allegations against him must be unmeritorious because otherwise, he says, "I could have faced a suit." The logic of the proposition requires no comment, but it so happens that Hayflick does now face suit from the government for recovery of proceeds of the sales of WI-38.

The gravamen of Hayflick's letter is that he has been unfairly treated. I believe that the opposite is the case, and that, in passing up yet another opportunity to address the basic issues, Hayflick's letter is further evidence that the findings of Schriver's initial report are correct.—NICHOLAS WADE