would be to ask extramural grant applicants in the fiscal year preceding the change to indicate in their applications which of the future institutes or divisions best reflects the work proposed. The total dollar amount attributed in this way to a specific unit would be combined with funds granted for intramural activities. Such a procedure could then be used to determine the relative distribution of funds to the individual units. Separate funding would be provided to NIH Central and other administrative functions.

The elimination of existing NIH units will be a difficult task. The biomedical community needs to unite to provide grassroots support. We as scientists and as U.S. citizens will win if the result is a better, less fragmented, and more efficient NIH.

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ALTHOUGH VARMUS ARGUES AGAINST FURTHER proliferation of institutes (he estimates a total of 50 by 2040), his real agenda appears to be to cut the current number of institutes receiving independent appropriations from about 24 to 6. He is concerned that the larger numbers mean "less flexibility, less managerial capacity, less coordination, and more administrative burden." Varmus's reasoning for why other institutes were created seems somewhat naïve. Surely, most came about because new or rising health problems were perceived by the public and by health professionals, and Congress responded.

Contrary to Varmus's thesis, flexibility in tackling these new health problems is probably enhanced by independent budgets for the new NIH components. Varmus says that "[i]t is highly unlikely that any major industrial firm would ever choose to be organized and managed in this way." Not so. Just look at General Motors. Their divisions are not of equal size or equal budgets. They even start new divisions (such as "Saturn") with their own budgets, managers, and infrastructure.

To move to a specific area, surely the public would suffer if the National Institute of Dental and Craniofacial Research were to disappear. History has shown that when similar events occurred in other countries (for example, in Canada and Great Britain), the research work simply did not get done. How sad it would be if a small number of "mega-chiefs" set the entire NIH agenda. Would the public really be served, rather than the managerial "efficiency" of a group of bureaucrats? To use a widely revered example, has there ever been a better return on biomedical science investment than fluoride? Would it have happened without a dental institute?

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Response

REPRESENTATIVE BURR IS CORRECT THAT HIS committee held a hearing on H.R.1795 last September, but no reader of the transcript is likely to call it a real debate. NIH was invited to testify just 6 days before the hearing and offered a written statement strongly opposing the measure; all of the witnesses who appeared were representatives of the disciplines that had been lobbying for it; and no hearing was held by the Senate. It is difficult to know how many individuals actually supported the measure. The same few people regularly appeared at my door to argue for it. These few represented societies with thousands of members, but that might be different from thousands of informed opinions. Moreover, one of the points of my Policy Forum was the importance of having a broader consensus, including support beyond the affected disciplines, before creating more institutes.

"... my experience in Washington taught me the frustrations and dangers of resisting the creation of new units at the NIH."

Burr suggests that I might have offered my reorganizational plan as an alternative to earlier versions of his bill when I was working at the NIH. My experience in Washington taught me the frustrations and dangers of resisting the creation of new units at the NIH. All three that I opposed, at significant cost to my relationships with some important constituencies, were ultimately created. Unless there is widespread support for a new reorganizational plan, perhaps achievable through the National Academy study now requested by Congress, any single advocate is likely to be ineffective and subject to the displeasure of even well-intentioned legislators like Burr.

Randerath suggests that a large fraction of basic medical research should be supported by a large institute without any nominal link to disease. This is a point that surely warrants further discussion in any study of the future organization of the NIH, but it is important to bear in mind the possible consequences to this institute of a return to times of fiscal constraint. Randerath's ideas about how a transition to a new structure might be achieved are interesting and provide a useful warning about the difficulty of making changes, even if they can be agreed to.

The letter from Goldhaber and colleagues illustrates the obstacles that will be faced by any proposals to change the structure of the NIH in ways that might reduce the influence or autonomy of special interests. Goldhaber *et al.* do not want to consider nuances in this complex situation. I acknowledged that there are legitimate arguments on both sides of the organization issues, but maintained that continued expansion presents a significant danger; and I emphasized that attention needs to be given to solutions now, before the situation becomes worse, not that it was already unworkable. Furthermore, I support the idea of having flexible divisions within large institutes, an idea entirely consistent with several automotive divisions of General Motors and very different from separate companies with separate budgets of different but relatively inflexible sizes, as currently exists at the NIH.

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The Yanomamo and the 1960s Measles Epidemic

THE PORTRAYAL OF JAMES V. NEEL AND THE measles epidemic among the Yanomamo in Charles C. Mann's News Focus article "Anthropological warfare" (19 Jan., p. 416), for which I was interviewed and quoted, is disappointing. Mann's discussion could leave readers with doubts and questions where few or none exist. Therefore, I submit the following information, all of which comes from reliable sources.

First, the measles vaccination program carried out in 1968 in the Amazon was a humanitarian effort.

Second, Mann's phrasing, "Measles may have appeared in the area before the Michigan team arrived..." implies there might be some doubt that the epidemic was caused by wild measles. In fact, there are two articles in *Brown Gold* [the New Tribes Mission magazine (*1*)], two letters written by Neel before his team's departure to Venezuela, and letters written by missionaries in the area in 1967–1968, all of which establish beyond a reasonable doubt that measles was present before the team's arrival in 1968.

Third, the Edmonston B vaccine, contrary to what Mann says, was the appropriate choice for the time. Neel consulted with experts at the Centers for Disease Control (CDC) and with Francis Black at Yale University (a public health specialist who had worked with Native American vaccination programs) before selecting the Edmonston B vaccine. The available data suggested that those vaccinated with Edmonston B might develop longer-lasting immunity and thus better protection than those vaccinated with the Schwarz vaccine, for which long-term data were not yet available. Further, there was worldwide experience regarding Edmonston B's safety and, when given with gamma globulin (as it almost always was during the vaccination effort), it was similar in reactivity to the other choice, Schwarz.

Fourth, those vaccinated against measles with Edmonston B vaccine are not contagious nor at risk of dying from the vaccine.

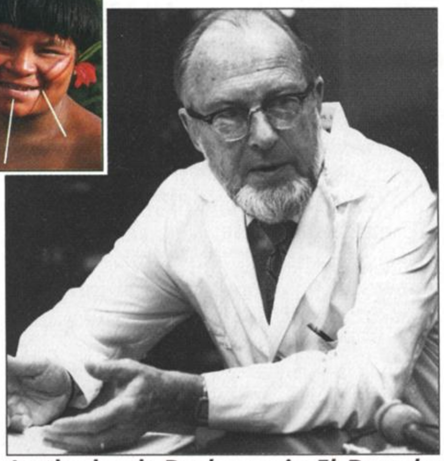
Fifth, contrary to what Mann suggests, Willard Centerwell, pediatrician and a member of the Michigan group, wrote a general vaccination protocol to be distributed to the missionaries. The urgency of trying to deal with the rapidly spreading epidemic meant that it was not always possible to follow the strategy outlined in the protocol.

Sixth, extensive medical care was provided to the sick Yanomamo. As a result of the vaccination program and medical care, the mortality rate for the epidemic was an estimated 8.8%, compared with 20 to 30% for similar epidemics when care was not provided.

Seventh, contrary to what Mann says, there were World Health Organization guidelines (*2*), updated in July 1967, which addressed general ethical considerations for carrying out scientific fieldwork among indigenous tribal groups. The 1968 expedition operated well within the guidelines.

Eighth, the blood studies my father conducted provided, among other things, information about the health status and health risks of the Yanomamo. For example, prior blood studies had indicated their vulnerability to measles, as Mann mentions in the article, which led to the vaccination program.

All of the above information is documented in expedition logs, published scientific articles, and archived letters, as well as in expert opinions and first-person accounts. I regret that Mann did not more strongly emphasize these facts.



In the book *Darkness in El Dorado: How Scientists and Journalists Devastated the Amazon*, James Neel (right) was strongly criticized for his and his colleagues' measles vaccination efforts of the Yanomamo in the late 1960s.

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References and Notes
1. *Brown Gold*, March 1968 and October 1968.
2. World Health Organization Technical Report Series No. 387, *Research on Human Population Genetics* (World Health Organization, Geneva, 1968).

AS AN ANTHROPOLOGIST WHO HAS STUDIED Amazonian peoples for many years and as a source for Mann's article about Patrick Tierney's book, *Darkness in El Dorado*, I object to Mann's depiction of my views as generally critical of Napoleon Chagnon rather than Tierney, and his depiction of the causes of the plight of the Yanomamo. South American cultures are being exterminated by miners, loggers, and land seekers who are expropriating their resource base and introducing lethal diseases. None of the scientists criticized in Tierney's book (particularly James Neel and Chagnon) is responsible for these continent-wide and centuries-old processes, and there have been thorough refutations of Tierney's accusations (*1*).

Although I welcome increased attention to the Yanomamo's tragedy, Tierney's book will only cause them harm and cause harm to its scapegoats—Neel and Chagnon—and to science in general. Indeed, the only practical consequence of the publication of *Darkness in El Dorado* to date has been a visit by Tierney to the Venezuelan congress, which resulted in the banning of medical research among indigenous Venezuelans, who could immensely benefit from such research. Mann's article seems to suggest that there is a legitimacy to such a ban.

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References and Notes
1. See, for example, www.nas.edu, www.umich.edu/~urel/darkness.html, hydra.usc.edu/iges/NeelResolution.html, and www.anth.ucsb.edu/ucsbpreliminaryreport.pdf

LIVE VIRUS MEASLES VACCINES ARE "EXTREMELY unlikely to be transmissible," writes Mann, paraphrasing me from our interview for his article. I would like to further emphasize this point by adding that in my 38 years of experience with hundreds of millions of doses of vaccine in populations with immunodeficient, immunocompromised, and immunocompetent patients, recipients have shown absolutely no evidence of transmission of vaccine virus. Surprises can always happen, and the current oral polio vaccine issue in the Dominican Republic and Haiti is cited by Mann, but this is a vaccine in which the virus is excreted for weeks after receipt by a susceptible individual. No one has ever demonstrated any excretion of measles vaccine virus by susceptible recipients.

Also in the article, my statement that "many more would have died if Neel had not been there" is a bit out of context. I intended to state that Neel's use of Edmonston B vaccine prevented a significant number of cases, not that his presence prevented deaths from the vaccine, none of which could be attributed to vaccine.

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Response

ALMOST ALL THE POINTS JAMES V. NEEL JR. makes in his letter were made at length in my article. Answering, in order, his charges: First, I did indeed describe Tierney's claim that Neel maliciously experimented with measles vaccine on the Yanomamo, and I listed the evidence, quoting Neel Jr., against those charges. In an article about those accusations, it would have been difficult to do otherwise. Second, I called attention to two sources, including a *Brown Gold* article, indicating that measles was present in the area before Neel's expedition. To my knowledge neither previously had been cited in print. Third, at the time, there were two main candidate vaccines, Edmonston B and the newer Schwarz vaccine, which was known to have fewer side effects, to be simpler to administer, and to be favored in two of three previous studies of vaccine responses in Native Americans. In the article, I quoted the conclusion from Francis Black that using Edmonston B—despite these apparent drawbacks—"was a rational thing to do." Fourth, I wrote, "Vaccine experts argue that the vaccine could not have touched off the epidemic. Measles vaccine co-developer Samuel L. Katz of Duke University says...live-virus measles vaccines are extremely unlikely to be transmissable...both Edmonston B and Schwarz vaccines, Katz says, 'have simply never been seen to be transmissible from a vaccine recipient to a susceptible contact.'" Fifth, according to Tierney, the administrator for Neel's papers, and the officials in charge of Freedom of Information Act requests at the CDC, as well as *New Yorker* fact-checkers, there is no known record of an official protocol for Neel's experiment. Sixth, I described how "Neel and his team tried to vaccinate ahead of the disease." Because they sought to move ahead of the disease and had limited personnel, in many cases they were not able to stay around after vaccination and provide care. I quoted Katz's summary: "[M]any more would have died if Neel had not been there" to vaccinate. Seventh, I did not state that Neel failed to follow contemporary standards. Instead, I made the different point that past standards are now regarded as insufficient. Eighth, I did not write that Neel's studies failed to provide data on Yanomamo health. Indeed, I cited his data on the Yanomamo's vulnerability to measles.

In response to Kim Hill, I quoted his description of Tierney's book as having "massive mistakes" and wrote that Hill "strongly disputes most" of its charges. So far as I can tell, I described all of Hill's main arguments against the book, although I did not specifically cite him every time. Instead, I quoted denials from John Tooby, Kent Flannery, L. Luca Cavalli-Sforza, Michael Price, Bruce Alberts, and Napoleon Chagnon himself, as well as devoted considerable space to other anti-Tierney evidence and arguments.

Finally, Katz writes that he meant by his quoted remark that the vaccine itself saved lives, not that Neel saved the Yanomamo from the vaccine. That is what I meant in quoting him, but if I inadvertently confused any readers on this point, this chance to clarify matters is welcome.

CHARLES C. MANN

CORRECTIONS AND CLARIFICATIONS

REPORTS: "The sequence of the human genome" by J. C. Venter *et al.* (16 Feb., p. 1304). In Table 10, the last column under the heading "Gene prediction" should have read "Total (Otto + de novo/2×)." In the References and Notes section, the authors for reference 176 should have read "A. Krogh *et al.*"; the journal name in reference 177 should have been "*Proc. Intell. Syst. Mol. Biol.*"; and in note 181, the acknowledgement list should have included after G. Edwards the names L. Foster, D. Bhandari, P. Davies, T. Safford, and J. Schira.