Editorial & Letters

EDITORIAL

Reaching out for World Health

In this century, there have been more gains in public health than in the entire previous history of humankind. Many of the major achievements are due principally to science and scientists—John Snow, Louis Pasteur, Robert Koch, and many others rescued civilization from the dark ages of fear of the unknown and the dread shadow of diseases such as cholera and plague. More recently, the conscience and concern of scientists have often been driving forces in advancing health, enhancing the development of new tools, and stimulating international research and control of major global health problems. Consider an example that spans most of this century: poliomyelitis. Its discovery as an infectious disease, the amplification of the virus in tissue culture, the development of killed and live attenuated vaccines, the strategy for their use through an expanded program of vaccination, the introduction of national immunization days, and recent research into improved safety and monitoring methods have all led to an approaching milestone in global public health—the worldwide eradication of polio.

However, there is little reason for complacency. Victories are often temporary. Our microbial enemies are incredibly adept at developing new defenses and weaponry and at jumping to new species to create new emerging infections. The very progress of our civilization can threaten our health; for example, transportation technology moves millions of people around the world every day, facilitating the spread of epidemics. And political and economic mismanagement can increase the deprivation of populations; today, less than 10% of the global research and development budget is used to address the largest disease burden, which is found among the poorer populations of the world, and we have no effective vaccines against major scourges such as malaria and AIDS.

How can current and future challenges be met? Research is crucial. It is also essential that scientists from different specialities approach problems in an interdisciplinary way. This is a call for talented young scientists from many branches of knowledge to reach out to improve world health and for science policy-makers in governments, agencies, foundations, and industry to underwrite their mission. The stunning pace of change in the health sciences and their engagement with other disciplines such as informatics, chemistry, physics, and social science provide a new opportunity for health in the 21st century. This call is not only for scientists and policy-makers in the industrialized world but also, and perhaps more important, for those in the developing world. The potential, passion, and perception of scientists close to the major problems of world health need to be tapped. This is especially true as the distance between the laboratory and the field continues to shrink rapidly, providing the best-ever scientific opportunity to address global health needs. We should make deliberate use of it, bearing in mind that investment in health is investment in development.

Research networks that span national borders will provide essential support for intensified public health efforts. In this context, I welcome the Multilateral Initiative for Malaria (MIM) research.* The efforts by MIM to accelerate capacity building in Africa will be a cornerstone for the "Roll Back Malaria" program on that continent.† Indeed, research will be an integral part of all World Health Organization (WHO) programs, strategically placed to make a difference where it matters most: on the ground.

I will put great emphasis on the scientific underpinning of policy in a renewed WHO. I intend to establish a separate function, devoted to health information and the development of evidence-based policy. WHO will be a leading advocate for health. In addition to the scientific evidence required for policy setting, WHO will gather information on the needs of researchers and on advances made in research. These will be reported to decision-makers around the world. Health ministers need little convincing, but WHO will remind presidents, prime ministers, finance ministers, and science ministers that they are health ministers themselves, key to bringing the science of health to bear on the well-being of their people. Our message will be that healthy people help build healthy economies.

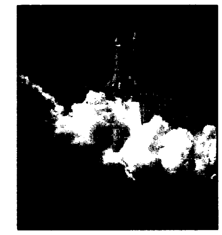
Gro Harlem Brundtland

The author will become Director-General of WHO in July 1998.

*B. Mons, E. Klasen, R. van Kessel, T. Nchinda, *Science* **279**, 498 (1998). †D. N. Nabarro and E. M. Tayler, *ibid.* **280**, 2067 (1998).

LETTERS

Into orbit



NASA

A reader advocates that scientists be considered for diplomatic careers. The work of Spacelab is defended (right, Neurolab's launch). Reconstructions of global temperature patterns are explored and clarified. The history of the "Henneman size principle" is elucidated. And methods for estimating population diversity are discussed.

Scientist-Diplomats

I read with great interest the News & Comment article "State Department sees S&T [science and technology] weaknesses" by Andrew Lawler (15 May, p. 998). The answers to resolving the serious lack of scientific expertise in U.S. foreign policy are simple and straightforward, which suggests why they have remained unfathomable within the gargantuan State Department bureaucracy. Here is a simple and easy-to-follow suggestion: The U.S. State Department should abandon the rather arrogant and somewhat unfounded assumption that the few scientists who are interested in applying for foreign service officer positions lack international affairs knowledge and experience. While it is true that, historically, the American educational system has not promoted broad intellectual development across the boundaries of the sciences, social sciences, and the humanities, this sad condition is not entirely without remedy. There currently exists in the United States a pool, albeit small, of well-trained scientists who have made the effort to develop a comprehensive understanding of foreign affairs. The entire American foreign policy process suffers if these individuals are not considered for careers in the foreign service.

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Spacelab's Worth

In the article "Research drought looms after Neurolab mission"(News & Comment, 24 Apr., p. 515) Andrew Lawler presents negative views of the NASA microgravity research program. It is suggested that metrics such as "good scientific value for the money" and "cost-per-science" be used to evaluate the program. This concept alone requires a response, but particularly so because a conversation that I had with him needs to be clarified.

In the next to last paragraph of the article, it would seem that I am endorsing the view that "good science" is hard to obtain by means of the quotes attributed to me—that "[t]here is precious little time in space for experiments" and that "it is also hard to repeat and alter experiments and to publish papers based on only one data set." In fact, I was responding to the view that very little worthwhile science has been done in spacelab missions. My point was that the shuttle has been used for many different purposes and that there has been relatively little available total time for the research community to do microgravity research. I have been fortunate to have had sufficient time for my experiments, and that research led to the awarding of seven Ph.D. and five M.S. degrees and the publication of 22 papers.

With respect to the issue of metrics, which some want to apply to the microgravity research program, I would challenge these individuals to indicate to what other research programs such metrics were applied and used as justification. How does one determine the dollar value of a science program? When Charles Townes was doing his pioneering work on lasers, did anyone, at that time, have any idea of the many applications of that work? Yes, space research is expensive, but so are many other research programs.

Lawler also says that "even [Spacelab's] supporters acknowledge there have been no major breakthroughs" in the program. Despite the relatively little experimental time allotted to microgravity research, a great deal of knowledge has been obtained about the behavior of fluids in that unusual environment. There are few, if any, other research programs that have yielded more scientific information from so little experimental time.

Microgravity research is of intrinsic scientific value in that it explores fluid and transport phenomena in an unusual environment, not unlike the ultra-high vacuum, high-magnetic field, and cryogenic environments that are used in other scientific fields. In addition, microgravity research is important because fluid and transport phenomena are inherent in many biophysicochemical systems and, therefore, it not only provides a knowledge base for the design of efficient and reliable space technologies but also can give insight into complex phenomena in industrial processes.

As more flight experiment time becomes available to a broader research community on the international space station, the exciting potential of microgravity research will be more readily achieved.

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Response: Ostrach does not appear to dispute that Spacelab provided limited experiment time for researchers and has yet to result in a major scientific breakthrough—two points accepted by program critics and supporters alike. His belief that worthwhile science has been done on the shuttle is expressed in depth by several in the article. As for the cost of science, Townes did not conduct his work in an era of the Government Performance and Results Act. All taxpayer-funded research programs by law must begin to measure their effectiveness, which clearly poses a daunting challenge for physicists as well as for life and microgravity scientists.

—***Andrew Lawler***

Global Temperature Patterns

On reading the Research commentary "It was the best of times, it was the worst of times" by P. Jones (*Science*'s Compass, 24 Apr., p. 544), the reader might see more disagreement than actually exists between that piece and a recent paper by Mann *et al.* (*1*). We take this opportunity to clarify some possible misunderstandings. The point expressed in the piece by Jones regarding the need for extensive and independent cross-validation of proxy-based reconstructions is indeed one that is wholly embraced by Mann *et al.* (*1*). The Northern Hemisphere mean temperature series shown in (*1*) is based on the calibrations which exhibited the greatest skill, that is, the fraction of instrumental variance described in both calibration and cross-validation or "verification." This reconstruction was based on all available data, which included proxy data, and the few long instrumental and historical records. However, a variety of additional independent calibration-verification experiments, although not shown, were clearly referred to in (*1*) and are described in detail on *Nature*'s supplementary information Web site (*2*), referred to by Mann *et al.* (*1*) In several of these experiments, only true "proxies," that is, natural archives, were used in the temperature pattern reconstructions. The long historical and instrumental records dating back several centuries in Europe and North America were withheld from the calibration experi-