

TROPICAL MEDICINE AND HEALTH IN THE DEVELOPING WORLD*

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Let me begin by telling you that it has been a privilege to be entrusted with the affairs of the Society for these 12 months, so much so that I leave it with regret. It was my feeling at the time of Harry Hoogstraal's death, before he could serve as your president, that Franz von Lichtenberg should serve for 2 years and thereby begin a new policy of 2-year terms. The Council rejected this option and I am honored by their having selected me. Even as the only nonelected president in the Society's history—that is, as the Gerald Ford of this Society—I cannot overlook the great honor it is to be numbered among the eminent former presidents.

Despite what you might think, a pleasant prerequisite of the job has been the stimulus to review some of our history. Most of you who have listened to these perorations in the past know that reading past presidents' speeches is considered mandatory. You should also understand that this is simply considered to be standard treatment for the anxiety that is provoked when one contemplates preparation of this address.

I began this exercise at the library of the Marine Biological Laboratory at Woods Hole during summer vacation. Desks, given to the lab by Ida and William Trager, and one in memory of Fred Bang, are adjacent to the stacks containing all the volumes of our *Journal* from 1920. Not only did I find previous presidential addresses interesting, but the other papers in these volumes of our *Journal* were equally interesting. And so, overlooking the eel pond, I found a classic paper by Samuel Darling on falciparum malaria which had never been read, at least in Woods Hole, for I had to cut the pages. I also learned that James Simmons experimented on himself with allergens derived from mangoes in Hawaii and that the neurological manifestations of pellagra (par-

ticularly acrodynia) decreased following the 1917 influenza pandemic.

Not surprisingly, perhaps, I found that much of what I wanted to say about tropical medicine and health in the developing world had been said in one form or another. That is, "there is nothing new under the sun." In the light of Senator Biden's experience, I should make clear that this is not an original observation; you'll find it in Ecclesiastes 1:9. Not only are many similar thoughts expressed in these addresses, but some even carry identical titles. In his 1953 Craig lecture, Henry Meleney discussed "Unfinished Business." In those days, the Council and membership apparently moved at the same glacial speed, for Clay Huff, the president in 1963, was still dealing with "Unfinished Business" in his presidential address. In a way, I, too, am dealing with "unfinished business." What you will hear today will be to some extent old wine in a new bottle. This phrase seems particularly apt as it continues the biblical metaphor, perhaps adding strength to my message. Further, social historians observe that there is a common tendency these days to discard anything greater than 5 years old unless it contains alcohol.

Several options emerged for me to follow in developing these comments. First, I could announce or at least hail a brilliant scientific discovery. Because this was most unlikely to be a breakthrough of my own, I stress the hailing of discoveries of others. Naturally, it seemed logical to talk about the tremendous progress toward a vaccine for schistosomiasis. But then I read the address of Henry Nichols in 1920 heralding the discovery by Hideyo Noguchi of a spirochete as the cause of yellow fever.¹ I feel certain that my friends in the schistosomiasis research community will forgive me if I elect to wait for more data; perhaps a president in the coming years can announce this with greater assurance.

A second option would be to discuss a revolutionary reorganization of the Society. In fact,

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this was done well by Karl Johnson 3 years ago. Although we may be moving at that glacial speed I referred to earlier, we have undertaken several of his recommendations and, having explored several management options, I believe that we are on the right track. There is plenty of need for further tinkering, but this is not the moment to propose basic changes. Finally, I might discuss the deep philosophical underpinnings of our work and thereby provide a leaven to lift your spirits if not your scientific productivity—equally unpromising.

William Sodeman considered these same possibilities in his 1953 address, "Our Future," and, rejecting them, he decided to be brief.² I shall follow his example.

It seems to me that my president's address should make some attempt to relate my own view of tropical medicine to the work of the Society. Two events in the past 12 months have brought my thoughts into focus: the publication of the Institute of Medicine's report on *The U.S. Capacity to Address Tropical Infectious Disease Problems*³ and the creation of the Independent International Commission on Health Research for Development. These two events strengthen my conviction that, both as scientists and members of this Society, our work will be informed and advanced when it is related to and is translated into action for health in the developing world. The message is a simple one: our work is enhanced by its closer application to actual control activities involving the people affected.

The mission of our Society is spelled out in our constitution: "the advancement of tropical medicine and hygiene." This does not mean the Society should take a parochial view of tropical medicine as the laboratory study of infectious diseases. Most of us come to this Society with a particular focus on one infectious disease or on a family of agents of diseases. Last year Franz von Lichtenberg described so well the diversity in our membership, and this diversity gives us strength. But, our unifying interest is in improving the health of those living in the poorest areas of the world. This, to use Franz' phrase, is "what brings us together."⁴

From our diverse disciplinary entry points—the clinician at the bedside of the patient, the veterinarian dealing with rinderpest or Rift Valley Fever, the microbiologist or parasitologist, the entomologist or malacologist studying agents or vectors of disease—we are attempting to put

science to use in improving the health in parts of the world where health problems remain especially recalcitrant. We begin from the assumption that there are rules, laws of nature, mechanisms of biology that we *can* understand. What may be viewed as chaos can, if we are sufficiently discerning, be transformed into order. Whether it is an outbreak of once-contained yellow fever or the exponential growth of infections with human immunodeficiency virus, there are basic biological truths to be defined. The tick vector (*Ixodes dammini*) of Lyme disease did not spring up de novo to spread Lyme disease. Disease occurs in response to facets of biology and ecology which, like a kaleidoscope, come together to give the picture, the facts, we observe.

If we understand the facts, they can be used not to change rules but to alter their consequences in favor of humans. Like the kaleidoscope, the facets can be rotated into a different position to yield a finer picture, a healthful outcome. The science we apply is not simply observing, collecting data, deducing a theory. It depends instead on developing a preconception of the possibilities, building a hypothesis which can be tested. The scientist attempts to stand outside the real world as an observer in order to explain it—to explain how *Leishmania* exist within the macrophage (a principal cell of immunity), how paired schistosomes live happily in the bloodstream or, on a more macro level, how the resistance of plasmodia to the 4-aminoquinoline drugs is spreading throughout the world. In his essay "Myth and Science," Francois Jacob observes, "The actual living world, as we see it today, is just one among many possible ones."⁵ The implication of this is that if we can stand outside the real world and explain it, we can also conceive a different reality. The real world that we define through research, the new knowledge acquired, is only one of several possibilities. Having defined or explained the actual, we can take the next step to devise a different, a preferable, actuality. This Society in particular is concerned not solely with the basic science observations of biological mechanisms but also with applying this new understanding to create literally a different world.

The dimensions of the AIDS epidemic on tropical public health were defined in the plenary session by Tom Quinn. Although the predictions are often based on incomplete data, as is true for all the diseases of concern to this society, AIDS

appears to present the gravest threat in this century to health in Africa (if not the whole world). Affecting young adults in their productive years, it threatens to halt economic productivity, to exacerbate food shortages and possibly even to result in negative population growth. The method of transmission and the long latency period together with a continuous infective state make the human immunodeficiency virus uncommonly able to spread with unusual speed. The sociopolitical ramifications of this epidemic, already a dominant feature, will continue to enlarge. It is not surprising that this epidemic already is the central focus of many members of this Society; it promises to occupy many more of us. But, AIDS, though a devastating epidemic, is subject to the same biologic certainties as hookworm. The character of transmission is just as accidental, or just as evolutionary, as the other. The lesson to be learned is not a moral one. Hookworm larvae attach to and penetrate the skin of the bare foot and HIV finds a receptor on the T-4 lymphocyte; the two are part and parcel of the same broad study of mechanisms of invasion. Understanding these mechanisms and the related behavioral risk factors is necessary if control is to be established. Since HIV is not an infection primarily confined to the tropics and therefore left for the most part to this Society, we may hope that out of this will come two major steps forward in public understanding of tropical diseases. First, research capability that can deal with new patterns of disease, at least new in our experience, must be strengthened. Secondly, the health of people in industrialized countries is inextricably bound to health in poor countries. We are literally "one world."

The world that we are talking about altering is the so-called "developing world." While not all of our concerns fall into those countries fitting the World Bank definition of a developing country, most do. These are the countries where infectious diseases account for 35% of mortality, nearly four times the percent in industrialized countries. Life expectancy in 34 of the world's poorest countries has increased from 41 to 50 years since 1960. Still, in contrast, life expectancy in the industrialized world currently reaches nearly 75. The developed world spends over 8% of its entire income on health alone, more in actual dollars than the poorest half of the world can spend on all government budget items. It is unlikely that we will see a significant change in

this situation in the near future. Clearly then, the approach we need to take must include the application of existing technologies to those health problems for which there are feasible strategies. At the same time we must better define the formidable problems that remain, identify the questions that research might answer, and set out to find solutions to reduce the unequal burden of illness on these countries.

In addressing the management of health problems of the developing world, D. A. Henderson and William Foege established these priorities: immunization, control of diarrheal diseases, family planning, and treatment of common diseases and injuries through primary health centers.⁶ This strategy attacks the leading causes of preventable death. William Chandler estimates that it would cost \$10 billion—<0.002% of the world's annual economic output—to save 5 to 10 million lives each year.⁷ But, he notes, as we well know, that low cost cures to many tropical infectious diseases have been neglected and can only be expected to respond to increased research.

My message is not that we abandon our current interests and join the primary health care movement. Rather, I am suggesting that research on tropical infectious diseases can best be advanced, and the mission of the Society fulfilled, by linking our work more closely to actual control projects. Last year in his Craig lecture Frank Neva demonstrated with many examples ("Lessons from Life," he called them) how clinical illness of the patient provides the stimulus for solid research.⁸ In the same way, and on a public health level, we must strengthen the links between research projects and epidemiology and control programs in endemic areas. The quick answer to this would be to say, "Of course, and we stand ready to do it. But there aren't enough opportunities." To some extent this is true, but I believe it is the obligation of every member of this Society to seek out or establish such opportunities.

Fortunately, we have a special vehicle to help us. This vehicle is the report of the National Academy of Sciences and the Institute of Medicine entitled *The U.S. Capacity to Address Tropical Infectious Disease Problems*.³ You will recall that this report was requested by our Society under Phil Russell's leadership and was directed through a committee chaired by Bob Shope. The study was supported by four federal agencies: the U.S. Army Research and Development Com-

mand, the National Institute for Allergy and Infectious Diseases, the Centers for Disease Control, and the Agency for International Development. The Rockefeller Foundation funded a workshop in Cairo to consider issues related to international cooperation. Many members of the Society participated in the discussions and survey work leading up to the report, but the findings and recommendations are those of an independent committee appointed by the Board on Science and Technology for International Development of the National Research Council. The committee was not composed entirely of members of this Society, nor solely of infectious diseases experts. Its chairman was David Bell of the Harvard School of Public Health's Department of Population Sciences and the study director was Karen Bell, to whom we owe great thanks. I go into this detail only to substantiate that the report offers the first outside assessment of our field in 20 years. As such, it presents the logical vehicle for our Society's mission. I believe it is an optimistic report; it notes the advances that are being made in the biochemistry, immunology, and molecular biology of many diseases in the tropics. Our meeting is ample testimony to that. The full report is a compendium of current activities in tropical infectious diseases: overseas projects and federal agency roles, academic support, personnel available. You'll find out, for instance, that we are not an aging group; that is, we are not aging any more than our colleagues in the other medical and biological sciences. Our mean age is 46.9 years. However, it was noted that only 35% of those under 40 have worked in developing countries, whereas up to 65% of us over 40 have done so. While it could be that those under 40 will reach the same experience level simply with age, it is likely that opportunities for this essential experience are falling. This is only one example of the careful data collection and analysis that has been done.

It would be inappropriate (*and* impossible) for me to do more than briefly summarize the findings. Out of the study's analysis have come conclusions and recommendations which could have enormous impact. The simple fact that emerges is that the United States is not doing what it could do. First, only 8 universities within the U.S. appear to have strength in the 3 major aspects of tropical infectious diseases: biomedical research, clinical treatment, and public health manage-

ment. Secondly, we lack sustained collaborative projects with developing world scientists in both research and control activities. U.S. scientists need this opportunity if research is to be relevant and training to be adequate. At the same time, greater competence can be created among scientists of developing countries if opportunities exist for collaborative projects coupled with post-doctoral training programs for developing world scientists. There are many quite specific recommendations regarding training and career structures, disease surveillance and collaboration with the goal of strengthening the research capability of developing countries. Given the breadth of our interests and experiences, we might expect to disagree on the relative emphasis of some of these but not on the major goals.

We now have an opportunity and an obligation to spread the message that this support is needed. This means each of us must find ways to engage in public education regarding both the needs and opportunities that exist in tropical medicine. Ken Warren is fond of quoting Jonas Salk who described parasitology as a field of "great neglected opportunities." We must bring these opportunities to the attention of those whose responsibility it is to provide the leadership and the budgetary resources for our national research capability, for our foreign policy development, for our support of international development.

While we may well place the humanitarian aspects of our work foremost, our goals are more inclusive. Development cannot flourish in an unhealthy environment. Health and development are inextricably linked. A nation with a healthy population is a better candidate for economic self-sufficiency and political stability. To use the Pan American Health Organization's phrase for its Central American project, "health is a bridge to peace." Our work, then, has both humanitarian and foreign policy implications. Increasing support for research and control of tropical disease would be deeply humanitarian. It would also be enlightened self-interest.

Past Society presidents have noted with disappointment the waxing and waning fortunes of tropical medicine with military interests. While this association will undoubtedly remain a factor in support, we have at this time another source of support for the linkages between our work and health in the developing world. One month ago in Frankfurt an Independent International Commission on Health Research for Development

was launched.^{9, 10} The proposal for such a commission originated from three commonly held beliefs among persons in the international health community: there are important health needs in the developing world that are not being met; research and development activities in international health should be more effective in addressing these needs; and donors who might be prepared to support such activities are often not aware of promising opportunities.

The idea of improving international health research through the work of a Commission originated at a meeting we held at the Clark Foundation in September 1985. Following this, the Foundation joined the International Development Research Centre of Canada in providing the initial funding for this work. I am especially pleased with the reception the idea of such a Commission has received. This is more clearly demonstrated in the financial support it has now received from Carnegie Corporation, Ford and Rockefeller Foundations, and Pew Memorial Trust on this side of the Atlantic; and the Swiss, Swedish, and German Aid programs in Europe. Additionally, both the World Bank and the United Nation Development Program have provided support. This means, of course, that these donor agencies are interested in improving international health research. The group's Chairman is John R. Evans, who heads Allelix, an Ontario biotechnology firm, and is Chairman of the Board of Trustees of the Rockefeller Foundation. Deputy Chairman is Gelia T. Castillo, a professor of rural sociology at the University of the Philippines. Other members of the Commission are from the United States, Bangladesh, Brazil, Egypt, Ethiopia, India, Mexico, Nigeria, and Zimbabwe.

The immediate stimulus to the Commission's formation was the observation that the agricultural research community has done a better job of focusing attention on the possibility of reducing hunger through research and of communicating the agricultural research needs to do this. In response to this, or as part of this, a mechanism for reviewing and planning was formed by donor agencies: The Consultative Group in International Agricultural Research (CGIAR). The budget for the CGIAR is approximately \$210 million this year. The budget for just 1 of their 6 laboratories is roughly equivalent to the entire budget of the UNDP/World Bank/WHO Special Programme for Research and Training in Trop-

ical Diseases. I am not suggesting that these funds should be diverted to tropical disease research. Rather, I am arguing that a similar level should be devoted to health research. We have not done an adequate job in communicating the potential of research for solving some of the health problems.

The formation of the CGIAR and its success was preceded by a series of meetings and reports on what agricultural research could do to alleviate hunger if given the resources. Similarly, reports of commissions and the hearings on the population crisis by the subcommittee on Foreign Aid conducted by Senator Ernest Gruening in 1965-68 was followed by substantial increases in support for reproductive biology research and population control activities. We are at a similar point in tropical infectious diseases.

While the Independent International Commission is just beginning its work, we can, I believe, hope that it will: produce an independent expert judgment of current strengths, weaknesses, and gaps in research and development activities concerning health problems in developing countries; promote and advocate actions to fill gaps, enlarge existing activities, or augment research and development efforts when specific needs have been identified; and, if desirable, recommend an arrangement for obtaining periodic assessments of research and development efforts concerned with health problems in developing countries.

The secretariat for the Independent International Commission is located at the Harvard School of Public Health and directed by Lincoln Chen. But, the Commission will be composed largely of members from the developing world with experience in biomedical research, in health care delivery, and in the behavioral sciences. Although the work of this Commission is just beginning conversations with the developing world, members suggest that their priorities for early attention included maternal and child health issues, emphasis on improving the capability for biomedical research in developing countries, and in behavioral science projects that seek to assure the application of available knowledge from research conducted in the industrialized world.

These two major steps: the publication of a report by the Institute of Medicine/National Research Council defining the current scope of tropical medicine and the clear need for additional support of the U.S. efforts and the formation of

the Independent International Commission, which has promise of providing guidance from independent developing world scientists on needs, provide us with support to carry our message into a larger arena. This was, I believe, the intent of Phil Russell, who set the stage for these comments in his own 1983 presidential address "Excellence in Research Is Not Enough."¹¹ The other critical ingredient needed was our commitment to ensure that research findings are applied to disease control. This cannot happen without wider public understanding of our mission.

Our Society has discussed over the last several years how it can become more active in public education concerning issues in tropical medicine. Given that there is unlikely to be the spontaneous founding of the equivalent of a Multiple Sclerosis Society or Cystic Fibrosis Society for tropical medicine, we must find new ways to educate the public and our government representatives on the needs and opportunities that exist. Toward this end, our public affairs committee has organized a workshop during this meeting to broaden our knowledge of how we can work together to achieve this goal. We should recognize that the Institute of Medicine report represents the best educational tool we have had (or are likely to have in the next 10 years) in pursuit of the goals of the Society. The Independent International Commission offers us an umbrella organization for rallying additional support and for informing us of the broader issues in health in the developing world—a way of assuring that the excellent research conducted by members of this Society has relevance and is applied in control of tropical infectious diseases. Both as a Society and as individuals, it is important to offer it our full support.

Let me make three quite specific recommendations: 1. The Society (through its Council) should continue to offer both strong moral and budgetary support to our increasingly active public affairs committee. 2. The Council of the Society should reexamine the desirability of obtaining professional representation of our Society in the Washington area. 3. The Council should examine the question of establishing a permanent committee on strategy and long range planning.

A number of strong recommendations are contained in the Institute of Medicine report. They must now be given life, flesh on their bones, in

the form of well-wrought workable programs with cost estimates.

I recently saw this quotation by Lord Keynes, referring to the problems economists have with making predictions about future market trends, especially with taking the approach that predictions can be made "in the long run." He said, ". . . the long run is a misleading guide to current affairs. In the long run, we are all dead. Economists set themselves too easy, too useless a task if in the tempestuous seasons they can only tell us that when the storm is long past the ocean will be flat." The problem is that economists are often unwilling to deal with short-term uncertainty, preferring instead to simply say that in the long run, when uncertainty is past, one can make a safe prediction. There is a lesson here for tropical medicine. I think that tropical medicine cannot really be certain of calm seas even in the long run. There will be uncertainty with regard to funding just as there will be uncertainties concerning disease control. Let me illustrate with a personal example.

When I went to St. Lucia in 1968 to work in the Rockefeller Foundation's schistosomiasis project, I believed I was in the vanguard of missionaries for science in that windward island. Not long after I was there the accountant in our project showed me her picture taken in the late 1920s with Rolla Hill, who was then the Rockefeller Foundation's resident physician in charge of the hookworm campaign. Only days before seeing the picture, a woman had walked into my clinic with a hemoglobin of 3.0 g/100 ml from a massive hookworm infection. Next year will mark the 75th anniversary of the beginning of the Rockefeller Sanitary Commission's international hookworm campaign and there remains plenty of hookworm to campaign against. Unfortunately, there will also remain many patients with schistosomiasis in the world for the foreseeable future. Effective means of controlling some diseases, from hookworm to human immunodeficiency virus, remain uncertain.

At least two-thirds of all resources for health research are spent in the United States, Japan, and Europe, yet not more than 5% is committed to health problems of the developing world. With the Institute of Medicine report and the Independent International Commission, we can change the priorities that these figures suggest. The report provides both broad goals for U.S.

leadership and a blueprint for harnessing U.S. talent and resources to deal with health problems in the developing world. We must identify specific problems and with vision and imagination devise long-term programs with our collaborators in developing countries. The Independent International Commission promises to guide our efforts in this process. This meeting is an affirmation of the research potential that exists to deal with health problems of the developing world. The potential for tremendous advances are there; I look forward to joining you in this journey.

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