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# THE SLOW FIX: COMMUNITIES, RESEARCH, AND DISEASE CONTROL*

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Carrying out the obligatory ritual of reading the addresses of my predecessors I learned that the Society had met once previously in Cincinnati, in 1945, before the "and Hygiene" was added to our name. Appropriately, President Rolla Dyer's address that year was titled "Medical Research in the Postwar World", and it contained hints of things to come. For example, Dyer mentioned that to promote the training of scientists the National Institute of Health (NIH) had established fellowships, some of which had been filled!

I am pleased that the venue of this meeting is Cincinnati, a city which at a personal level has much meaning for me. It is my mother's birthplace, and is where my parents met and married before they migrated to that then-distant land, Texas. One memorable summer my brother and I stayed with a much-loved uncle and aunt in Cincinnati who were devoted fans of the Cincinnati Reds, and they took us often to the ballpark. Indelibly etched in my memory is meeting the 1947 Reds team in the dugout, and being given a baseball signed by the entire team, including our hero Johnny Vandermeer (who is the only major leaguer to have pitched back-to-back no-hitters).

Cincinnati, as you may know, owes its name to Cincinnatus, a fifth century B.C. Roman statesman and military leader who achieved legendary status as a model patriot. After retiring as a general in the imperial Roman army he returned to his farm. While he was plowing the fields one day an emissary from Rome appeared with the message that he was urgently needed to lead the army on a critical mission. He left his plow, carried out the mission, and returned to his plow 16 days later. I tried, unsuccessfully, to uncover historical evidence to link Cincinnatus with tropical diseases, but I decided he was too intriguing a figure to ignore so I included him anyway.

In Franz Von Lichtenberg's 1986 presidential address I found the statement "a year of presidency is also an opportunity to gain a better understanding of the Society". I certainly can relate to this comment. In addition to the profound honor of serving as your president, my knowledge of the Society and its complex fabric has increased manyfold. Secretary-Treasurer Peter Weller merits special recognition for his skillful and patient role in educating incoming presidents, as well as for other selfless service to the Society.

As you heard from Dan Colley last year, and others before, selecting an appropriate topic for this address is a sobering challenge. After much contemplation, I have chosen two issues to explore with you, each of which I believe is vitally important if we as a Society are to effectively accomplish our mission: "to promote world health by the prevention and control of tropical diseases through research and education". The first issue concerns the role of field-oriented investigations, with an emphasis on community or population-based research. The second, and related topic, is the role of social and behavioral scientists in our Society, for I believe it is time for us to seriously address this issue.

Very early in my career, working at the Centers for Disease Control's (CDC) San Juan Laboratories in Puerto Rico, I became impressed with the power of longitudinal, community-based study designs to investigate tropical diseases. As we attempted then to identify the meaningful research questions concerning schistosomiasis in Puerto Rico, it became obvious that they could best, or only, be answered by means of long-term studies in one or more communities. These deliberations led to the creation of the nine-year Boqueron Project in eastern Puerto Rico. I will say little more about this because it is certainly not my intent today to talk about my own research.

I do not want to imply that studies in other populations such as those in hospitals, clinics and schools are not capable of generating valuable knowledge, but I will argue that research conducted in demographically defined community populations offers unique advantages. Furthermore, I will try to convince you that we should be making vigorous efforts to bring together the power of community studies with the new tools of molecular and cellular biology, and that interdisciplinary research which links the laboratory and the field is crucial to our endeavors.

Not wanting to inflict a didactic exercise in epidemiology upon this post-prandial audience, I will instead attempt to point out some advantages, perhaps obvious, of research based in communities. First, with appropriate sampling it permits extrapolation of findings to the entire community and to similar communities. Second, it permits characterization of the host, parasite, vector, reservoir, or intermediate host and their natural interactions, and the effect of environmental, seasonal, and other temporal factors. Third, morbidity and mortality (or the natural history of disease), and their determinants, can be measured and assessed. Fourth, diagnostic techniques can be evaluated in the full population at risk, not just in non-representative "cases" and "negative controls". Fifth, the impact of interventions on disease transmission, morbidity, and other outcomes can be measured. A final example, the average life span of certain parasites can be more accurately estimated. Sten Vermund and collaborators, using nine consecutive years of data from the Boqueron Project in Puerto Rico, showed that the average life span of *Schistosoma mansoni* adults was greater than 25 years, far longer than the five years previously estimated from cross-sectional studies.[1] This finding had important implications for disease control strategies as well as for the nature of the host-parasite relationship.

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- **THE STUDIES WERE DESIGNED FROM A PROBLEM-SOLVING OR HYPOTHESIS-TESTING PERSPECTIVE, AND WERE NOT SIMPLY DESCRIPTIVE SURVEYS;**
- **THE STUDY POPULATION WAS CLEARLY DEFINED AND MONITORED OVER TIME;**
- **OBSERVATIONS (NUMERATOR DATA) COULD BE DIRECTLY RELATED TO THE ENTIRE POPULATION (DENOMINATOR);**
- **HIGH QUALITY LABORATORY SUPPORT WAS UTILIZED;**
- **ETHICAL ISSUES WERE PROPERLY ADDRESSED;**
- **CLEAR INSTITUTIONAL RELATIONSHIPS WERE INVOLVED.**

FIGURE 1. Criteria for selecting examples of community-based research.

I have chosen additional examples of community-based research to expand upon some of these ideas. To help guide me in selecting good examples, I used these six criteria (Figure 1): 1) the studies were designed from a problem-solving or hypothesis-testing perspective, and were not simply descriptive surveys; 2) the study population was clearly defined and monitored over time; 3) observations (numerator data) could be directly related to the entire population (denominator); 4) high quality laboratory support was utilized; 5) ethical issues were properly addressed; and 6) clear institutional relationships were involved. The examples I have chosen also reflect a strong bias towards research undertaken by members of our Society.

Before discussing examples from developing countries, it is appropriate to note models of community-based studies in the U.S. Arnold Monto, once an active member of our Society, recently completed a valuable monograph "Studies of the Community and Family: Acute Respiratory Illness and Disease".[2] It includes an overview of the highly respected Tecumseh Study created largely by Thomas Francis, Jr. at the University of Michigan. Easily defined and located near the university campus, and with about 10,000 residents, Tecumseh was an ideal community in which to undertake a study of acute respiratory infection and illness. The study started in 1965 and operated for a total of 11 years. Ten percent of the population was enrolled in the study; weekly questionnaires to identify illness, specimen collection for agent isolation, and periodic monitoring for seroconversion were the key elements of data collection. It is noteworthy that the laboratory methods used in Tecumseh were derived from those used by Arnold Monto and Karl Johnson in the former Panama Canal Zone in the first population-based study of acute respiratory illness in the tropics.[3]

The findings of the Tecumseh study, which could be generalized to the U.S. population, permitted accurate estimates of the impact of the major etiologic agents of respiratory disease by age, and helped policy makers set priorities for development of vaccines and antivirals. The community basis of the study also made it possible to measure morbidity and rates of physician consultation and to establish accurate measures of secondary attack rates.

During the 1960s, Leonardo Mata, now an honorary member of our Society, and his colleagues at the Institute for Nutrition of Central America and Panama conducted landmark studies in the Guatemalan highland village of Santa Maria Cauque.[4] These studies, which spanned several years, were the first to critically elucidate the complex interrelationship between infection, nutrition, and growth. The prospective nature of the study clearly demonstrated the degree to which infection, particularly gastrointestinal, damaged intestinal mucosa in infants, contributing to compromised growth and development.

The Santa Maria Cauque studies spawned a number of subsequent efforts to document, in communities, the nature and impact of diarrheal disease in children in developing countries. The work of Robert Black and colleagues at the International Center for Diarrheal Diseases Research in Dacca, Bangladesh, became models for community-based studies of incidence, etiology, seasonal patterns, and natural history of diarrheal disease in children.[5]

Community-based research on diarrheal diseases by Richard Guerrant and coworkers in a rural community, Pacatuba,[6] and later in urban Fortaleza,[7] Brazil, is noteworthy for several reasons. These studies, which determined incidence, etiology, seasonal patterns, nutritional impact, risk factors, and demographic characteristics, reflect a longstanding and remarkably productive scientific and educational relationship between two universities, one in Virginia and the other in northeastern Brazil. In Pacatuba, 30 months of prospective monitoring documented a dramatic association between diarrhea morbidity rates in households, and fertility rates in the same households. Research generated by this collaboration thus has profound implications for issues as diverse as controlling population growth, as well as pathogenesis of inflammatory and noninflammatory diarrhea, and the molecular mechanisms by which cholera toxin induces secretion. To more fully appreciate the contributions of collaborative research in these Brazilian communities, I urge you to read Dick Guerrant's paper "Twelve Messages from Enteric Infections for Science and Society" published this year in our Journal.[8]

Turning to schistosomiasis, an excellent example of population-based research is the St. Lucia Project, supported primarily by the Rockefeller Foundation. Conducted during the 1970s, the project set out to describe the biological and epidemiologic characteristics of *S. mansoni* in three similar valleys of St. Lucia, and then to document the cost and effectiveness of three alternate control strategies (one in each valley): chemotherapy, snail control, and provision of safe water supplies. In addition to producing definitive data on control, the interdisciplinary nature of the research team generated valuable biological, behavioral, and environmental knowledge related to schistosomiasis and other diseases. Very importantly, the St. Lucia Project provided early field research experience for some of our most accomplished members including Joe Cook, Dan Colley, John David, and Ken Warren, among others.

The Qalyub Bilharziasis Project in Egypt, directed by Dr. Mohamed El Alamy and later by Dr. Mostafa Habib, spanned a decade starting in 1975. With eight study villages in the Nile Delta region, it conducted the first assessment of the tolerance and impact of selective chemotherapy with praziquantel at the community level, and also detected and documented prospectively the remarkable spontaneous decline

in *Bulinus truncatus* and *S. hematobium* transmission in the Nile Delta.

Kristen Weigle, collaborating with Nancy Saravia and other investigators based in Cali, Columbia, conducted an extraordinary prospective study of cutaneous leishmaniasis in 15 contiguous villages in the Department of Narino. This work, published last year in the *Journal of Infectious Disease*,[9,10] is a splendid example of the power of a longitudinal population-based investigation of a tropical disease in its natural setting. These ground-breaking studies elucidated as never before the incidence and natural history of infection with *Leishmania braziliensis* and *L. panamensis* during more than 7,000 person-years of observation. The investigators were able to measure the rate of relapses as well as of new infections, and they established demographic, behavioral, and environmental predictors of risk of infection and risk of disease. Characterization of strains by isoenzyme phenotypes or monoclonal antibody typing permitted parasite-related pathogenicity to be assessed.

The pioneering Garki project in the savannah of northern Nigeria, directed by Louis Molineaux and coworkers, was an ambitious, village-based effort to quantify major factors responsible for malaria transmission, to measure the impact of interventions, and to build a mathematical model of malaria transmission based on field-derived data.[11] The findings of the Garki project have profound implications for malaria control in Africa, and continue to influence critical thinking on the subject.

Recent community-based studies in coastal Kenya undertaken by Robert Snow, Kevin Marsh, and others have focused on severe childhood malaria and its pathogenesis, a critically important line of inquiry.[12] Precisely measuring the occurrence of severe disease by place and time, these investigators documented space-time clustering despite stable parasite rates in the community, raising intriguing questions about the pathogenesis of severe malaria.

A final example comes from the early stages of investigations of Bolivian hemorrhagic fever (BHF). Well before the epidemiology or ecology of this newly appeared, deadly disease was elucidated, Ron MacKenzie and Karl Johnson rigorously monitored and mapped the occurrence of hospitalized cases in San Joaquin, as seen in this figure extracted from a 1965 paper in our Journal[13] (Figure 2). It showed slow but progressive movement of disease through the town from south to north. These data strongly suggested that neither person-to-person nor mosquito-borne transmission were important epidemiologically, and appeared compatible with the hypothesis that humans became infected primarily in or near their houses due to contact with virus in the urine of the rodent *Calomys callosus*. Definitive proof of this hypothesis came from the following intervention in the community. During the two weeks (the incubation period of BHF in humans) immediately following rigorous rodent trapping in half of the community, the incidence of new cases declined to zero, whereas new cases continued in the part of town without rodent-trapping activities. When rodents were then trapped in the other half of town, the incidence of new cases also dropped to nil within two weeks, and no more cases occurred.

The point of this and other examples is to emphasize that certain research questions, especially those which relate to the dynamics of transmission, the nature and determinants of morbidity and disease severity, the impact of interventions, and for which extrapolation to larger populations is important, longitudinal studies in communities provide unique opportunities.

I now direct your attention to the second issue, namely the role of social and behavioral scientists in the Society. Despite its diversity, our Society has a primarily biological and medical orientation. The mention of social or behavioral dimensions of tropical diseases causes the eyes of many of our members to glaze over. We have made efforts to address this issue. Don Krogstad discussed it in his address two years ago when he reviewed the status of the initiatives generated by the February 1992 Council Retreat. Solid progress is evident for several of these initiatives, but in contrast the "Social and Economic Determinants of Health" component of the strategic plan has made little headway. At the mid-year council meeting last year, Society officers were asked to select one of the retreat issues to monitor and help advance. When the "social and economic" initiative was mentioned, deafening silence convinced me to volunteer even though interest, certainly not expertise, was what I could offer.

Regretfully, our Society's excellence in biomedical research is not complemented by excellence in "human ecology", referring to the broad concept of the co-evolving relationship of humankind and the environment. We know that in the developing world this environment is undergoing drastic changes, and that communities, urban and rural, are being radically transformed, yet we have done little to address the "human" leg of the classical "human-agent-environment" triad of the determinants of disease. Human behavior is at the heart of prevention, treatment and control of tropical diseases, and it plays a central role in the so-called emergence of microbial agents of disease, but we have yet to respond, as a Society, to this reality.

During recent years, I had the rich experience of working in Cameroon with an outstanding, field-seasoned medical anthropologist, Dr. Barry Hewlett. This collaboration spanned several years and several diseases, including schistosomiasis in the extreme north and onchocerciasis in the extreme south of the country. We learned from each other. I gained valuable insights into how the research perspective of the epidemiologist and medical anthropologist differ, and into the more holistic approach he used to understand disease in its societal context. I also learned about straightforward methodologies that can be used to collect semi-structured, yet quantifiable, data about the beliefs of indigenous people and the diseases that affect them.

Many of you have heard the allegory about the physician who describes to his friend a recurrent dream or nightmare. In this dream the physician finds himself walking along a river bank when he hears a cry for help from someone drowning. He jumps in, rescues and resuscitates the victim, but immediately another person cries out for help, and the scene repeats and repeats. Finally, the physician tells his friend, he realizes that he had been too busy pulling people out of the river to go upstream to see who was pushing them in! Much of our effort has been directed downstream, to those who have already been pushed into the river of disease. Largely in the context of cardiovascular, smoking-related, and other chronic diseases, cries have gone out in recent

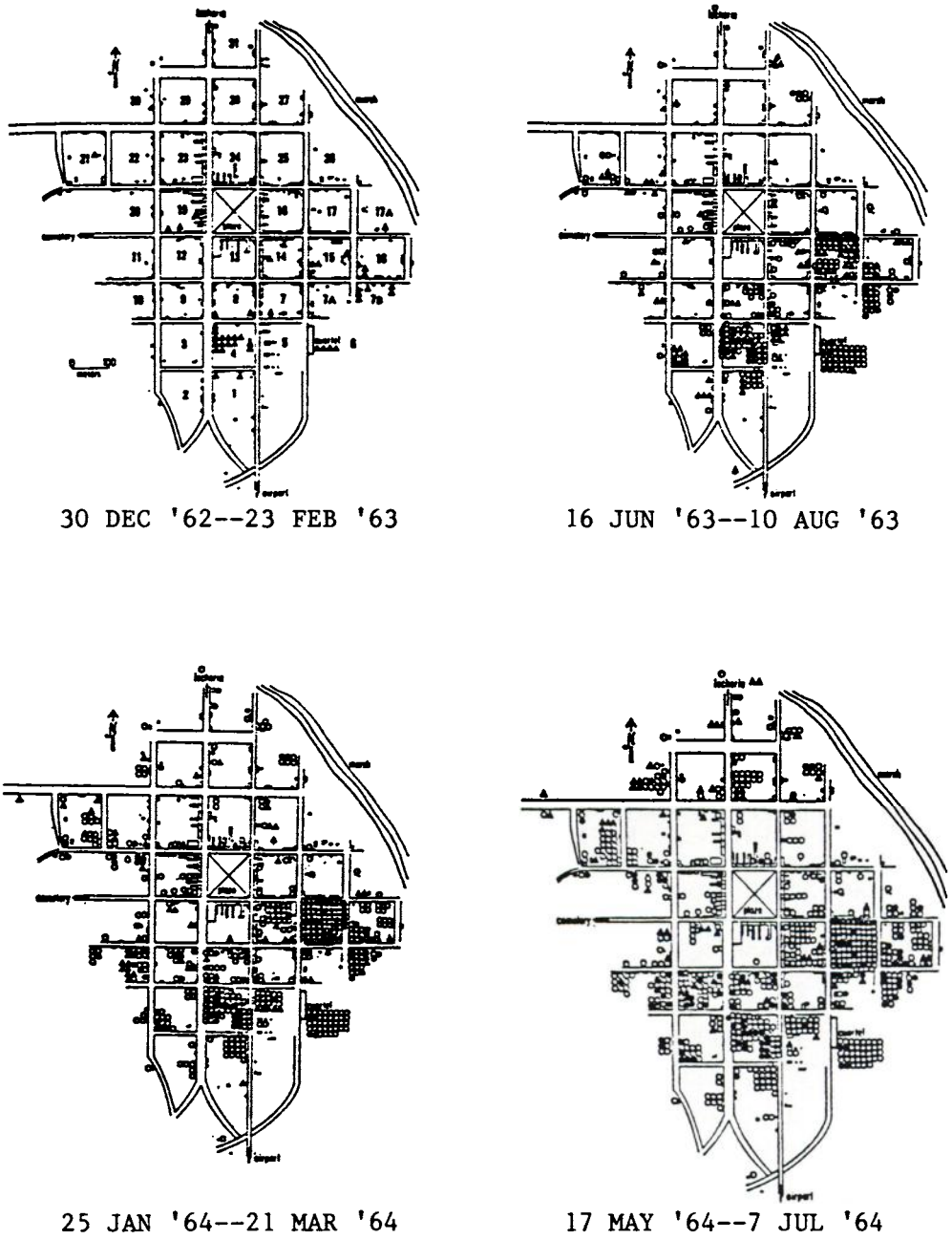


FIGURE 2. Hospitalized cases of Bolivian hemorrhagic fever in San Joaquin, Bolivia, 1962–1964. (Reproduced with permission of the American Society of Tropical Medicine and Hygiene, from Mackenzie[13].)

years urging health researchers to increasingly focus on "upstream" determinants of disease. The same is obviously true for tropical diseases. My point is that whether the focus is "upstream", for example on the social and economic determinants of the explosive HIV epidemic in parts of Asia, or "downstream" on how culture influences the perception of, and response to, illness, the contribution of social and behavioral scientists is essential.

A good example of "downstream" research is a study from West Java published last year in *Lancet*.[14] Prospective, community-based research revealed that whether or not the mother of a gravely ill child sought Western medical care was associated with the age of the child, the duration of the terminal illness, previous attendance of the mother at a community-based clinic, and the mother's response to a prospectively asked question about what care should be sought for a hypothetical one-month-old baby with signs of severe pneumonia. Contrary to common dogma, not associated with care-seeking were household income, maternal age, maternal education, and distance from the home to a government health post.

Not long ago I had a memorable, if not humbling, experience in Cameroon that I will describe to you. I was in the passenger seat of a four-door Toyota pickup, returning to Yaounde after spending some days in Sangmelina in the heavily forested onchocerciasis zone. The Cameroonian

driver was from the immediate area. In the back seat were my Cameroonian graduate student, Dr. Kollo, and a visitor from the U.K. Our luggage was in the bed of the pickup, uncovered. A short way out of town it began to rain. As the rain came down harder, I mused about how soggy clothes would be no big problem, but that rain-soaked papers and a wet laptop computer were a different matter. Just as my brain began to register resigned despair, the driver suddenly pulled off the road, jumped out, and disappeared into a nearby hut. Minutes later, as I gave up trying to figure out what was going on, there emerged from the hut the driver and an entire family, each carrying large, freshly cut palm fronds. Within moments they had covered the luggage in a criss-cross pattern, and we were on the way again. A minute later the driver stopped abruptly, gathered some heavy branches lying near the road, broke them to the appropriate length, and used the branches to firmly secure the leaves against the wind. The luggage remained bone-dry. Here was an elegantly simple, effective, low-cost, environmentally sound solution to a problem that the visiting professor could not begin to solve! On the road to Yaounde I had time to ponder this experience. Out of our own ignorance about the people we wish to serve, do we not underestimate the ability of people and communities to solve their own problems, perhaps in ways better than we can imagine? Dr. Michael Nathan, in a talk on control of vector-borne diseases, recently called for greater dialogue between social scientists, biomedical scientists, and field practitioners. He said: "...Unquestionably there has been an element of technological arrogance in our approach towards the control of vector-borne and parasitic diseases. We have naively addressed the community as an empty vessel that eagerly waits to imbibe the science-based knowledge that we preach and have expected [the community] to adapt and conform to our objectives for environmentally acceptable behaviors..."

While our Society has recognized the need to involve social scientists in our research and educational programs, we have yet to act effectively. This stems in part from our relative ignorance of the social sciences, from an erroneous perception by some that they are less rigorous than the biological sciences, and from excessive jargon on both sides. Meanwhile, largely spurred by the HIV/AIDS pandemic, the role of social and behavioral scientists has increased dramatically in recent years. For example, the social and behavioral scientists at CDC in the pre-AIDS era could be counted on one or two hands. I was surprised to learn recently that a survey of CDC workers in 1992 revealed over 150 such individuals, the majority of whom had graduate degrees in disciplines such as sociology, anthropology, psychology, criminology, economics, urban studies, health education, instructional technology, and others. I learned from Dr. Robert Hahn, a medical anthropologist at CDC, that currently some 250 professionals at CDC belong to a "Behavioral Science Working Group" linked through an E-mail network. Another resource at CDC is the Qualitative Methods Research Interest Group, headed by Dr. James Carey. Many of these social scientists have active interest and expertise in diseases of developing countries, and they represent a group of valuable potential collaborators and Society members.

Causing uneasiness among some basic researchers, a conference was held last year on the NIH campus. Titled "Disease Prevention and Research at NIH: An Agenda for All", the conference considered the need for more research addressing behavior, social issues, and cost-effectiveness of disease prevention measures. Suggestions that emerged included naming behavioral scientists to the advisory councils of all institutes, increasing the number of prevention-related professionals on study sections and advisory boards, and eliminating barriers that inhibit collaboration between NIH and agencies such as CDC.

I believe it is a major challenge and opportunity for biological scientists to collaborate actively with social scientists, and what better organization is there –with our extraordinary diversity and field seasoning – to take the lead role in this endeavor. We should, as a Society, be at the forefront of such interdisciplinary research. We must be clear and proactive in our messages and actions.

With our Journal, annual meetings, and diverse membership, we have much to offer that would attract social and behavioral scientists. A prominent medical anthropologist recently confided to me that it was more important for him and his colleagues to publish papers to be read outside the field than by the relatively small group of medical anthropologists. Our Journal can encourage submission of manuscripts that intersect with the interests of our membership. Offering symposia, such as the one scheduled tomorrow morning entitled "Human Behavior and Cultural Context in Disease Control" is an important means of creating within our Society an inviting niche for this scientific discipline. To encourage participation in our meetings, we should create an award or other means to recognize outstanding papers or posters on social science topics. Furthermore, we should expand direct efforts to identify, attract, and interact with groups such as those at CDC, the infectious disease subgroup of the medical anthropologists in the American Anthropology Association and others.

Returning now to the community, the balance of my talk focuses on the gap between laboratory and field-based investigations. I am deeply concerned that as the complexity and sophistication of the research laboratory has escalated during the past decade or so, so has its separation from people who live in the endemic communities it ultimately intends to serve. As a result we increasingly think of the "field" in passive terms, as a place where the products of science are simply evaluated, perfected, and applied.

Much of the scientific success of the community-based examples I described earlier derived from the fact that the laboratory was an integral part of the research team. Today's more sophisticated laboratory has become relatively isolated while, at the same time, support for field-based research has declined. Furthermore, field-oriented investigators often merit low marks for their efforts to communicate and collaborate with their high-tech counterparts, and vice-versa. I firmly believe that tropical medicine needs a new generation of investigators whose basic science or clinical skills are complemented with an understanding of the reality of diseases in endemic communities, and who are committed to multidisciplinary research that connects the laboratory and the field.

While many pay lip service to the need to bridge from the laboratory to the field, I see little objective evidence it

is happening, or that decision makers are grappling with this issue. One encouraging exception comes from the Request for Application (RFA) issued for the most recent round of competition for extensions and new awards for the International Collaboration in Infectious Disease Research (ICIDR) Program of the National Institute of Allergy and Infectious Disease I was delighted to see in the RFA the following statement: "It is the intent of the program to support the conduct of biomedical research which can only be performed outside the U.S., and thus, an emphasis on population-based studies would be appropriate". Although I would prefer language stronger than "would be appropriate", I believe this is an important step in the right direction if, in fact, this emphasis can be sustained through incentives and other means.

I see other encouraging examples of investigators looking to population-derived data to generate alternate hypotheses that can then be tested using the latest tools of molecular biology. In a paper to be presented in the malaria scientific session on Thursday morning, Ogobara Doumbo will describe studies which he, other Malian collaborators, and Don Krogstad undertook to determine the temporal patterns of severe and cerebral malaria in a region with hyperendemic, distinctly seasonal transmission of *Plasmodium falciparum.* The investigators reasoned that if severe malaria results from insufficient immunity, it should cluster at the beginning of the transmission season. However, they found evidence of clustering of severe cases at the end, not the beginning, of the transmission season. They suggest a series of alternate hypotheses that, to adequately test, will require, at the community level, prospective characterization of parasite clones, monitoring of transmission efficiency, and prospective measures of the evolution of the host immune response during the transmission season. I believe this is an ideal example of bridging between laboratory and field, of using the laboratory to answer questions generated by observations in endemic communities. I would like to see more of this type of collaborative research, especially when it can be sustained by means of strong, mutually beneficial institutional relationships, and stable funding.

Another example of bridging between the field and the laboratory, and of multidisciplinary research, comes from a provocative paper published recently by Paul Basch, a much admired Society member and schistosomiasis expert.[15] His paper, "Antischistosomal vaccines: beyond the laboratory", embodies much of the theme I have attempted to explore. Basch points out the need for "a compelling epidemiologic and/or economic justification" in choosing preventive strategies when alternate choices exist. He reviews the rationale of an antischistosomal vaccine, and then explores in depth the kinds of information that would be required to assess vaccine safety and efficacy in endemic communities. He addresses a series of complex issues related to design of randomized clinical trials, case definition of schistosomiasis, alternate approaches to case ascertainment, problems with determining vaccine efficacy, concerns with ability to extrapolate vaccine efficacy to other schistosome and human populations, logistical factors, monitoring for late adverse effects, and some tricky ethical issues as well! Basch lists a number of questions that need to be resolved by research before, in his opinion, candidate antischistosomal vaccines

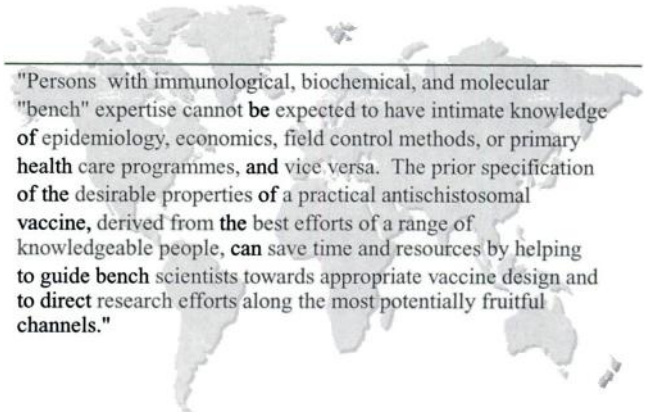


FIGURE 3. Rationale for using a multidisciplinary approach in a community setting.[15]

can be evaluated. Economic issues, including alternate (non-vaccine) approaches to schistosomiasis control, are considered.

Basch's rationale for using a multidisciplinary approach in a community setting is summarized in Figure 3. Ours is an impatient society that supports, encourages, and rewards a quick fix approach to biomedical research. More than we recognize, funding, role models, and professional incentives are driven by anachronistic images evoked by the likes of Pasteur and Fleming, of instant solutions to health problems emerging from the laboratories of towering intellects. Indeed, how many epidemiologists have been awarded the Nobel Prize in Medicine! While the promise of the modern research laboratory generates appropriate optimism, so must we be realistic about its limitations.

Some years ago malaria researchers were urged to return to the basics after elegant but empirical efforts in vaccine development failed. More recently, leading virologists have suggested the same for HIV/AIDS. No argument here, but I am convinced that the complex biological, environmental, and behavioral determinants of human disease are not best elucidated in isolation in the laboratory or in the hospital. Learning why some infected people get sick, and others do not, will challenge the finest minds using the most powerful laboratory tools, but I believe these critical questions often can best be framed and answered in the context of prospective, interdisciplinary community-based research.

I have no illusions that it is simple to alter the way we approach research, and I do appreciate how intimidating it can be to communicate with scientists in unfamiliar disciplines. We should invest far more in models like the ICIDR and Tropical Medicine Research Centers (TMRC) programs, both of which emphasize research in endemic areas. I suggest that our Society organize a workshop to explore improved models for interdisciplinary and interinstitutional collaborative research in developing countries, and create a strategic plan to advance those models.

In framing my closing remarks, I evoke the words of predecessors. Tom Weller in his 1964 Presidential Address expressed concern about "intellectual snobbery that makes research an end unto itself, that divorces the researcher from the responsibility for the useful application of his new found knowledge. . .". Three years ago, Scott Halstead challenged

us to "build an organization dedicated equally to science and to its applications. . .". The slow fix I envision responds to these concerns and challenges, urging us to use endemic communities to permit scientists of diverse disciplines to interact creatively, to formulate and address scientific questions that range from the basic biology of parasites to the upstream and downstream determinants essential to the prevention and control of tropical diseases. The slow fix I envision avoids the unproductive debate on basic versus applied research and urges it to be transformed into a basic and applied context. The slow fix I envision challenges us to expand and exploit the unique diversity of our Society to develop a clearer vision of, and effective advocacy for, optimal research strategies. The slow fix I envision also provides the setting in which the tropical public health scientists and practitioners of the future will mature and flourish.

Paradoxically, I believe a slow fix research strategy, intimately connected to communities suffering from tropical diseases, will often be more effective than a quick fix approach in advancing the mission of our Society.

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