JAMES H. FLORY AND
PHILIP KITCHER

Global Health and the Scientific Research Agenda

I

In 1995, 56 million people died. Violence killed fewer than one million of them; famine contributed to about six million deaths; more than 40 million died of some form of disease.[1] Many of these illnesses were the result primarily of old age and may have been unpreventable. That is unlikely to be true, however, of over a million deaths from malaria, nearly two million deaths from tuberculosis, nearly three million deaths from AIDS, and over four million deaths from respiratory disease.[2]

To suggest that many of these deaths could have been prevented might be to make one of two distinct claims. First, in the current state of our knowledge, available techniques exist for responding to the diseases mentioned above and for saving those who suffered from them. Second, these are scourges that we might hope to overcome in the course of future biomedical research. Part of the world's burden of disease could have been alleviated if those who died had had access to drugs or treatments routinely available to others in different places. Another part could have been lightened if there had been more thoroughgoing efforts to discover methods of combating disease, methods that the actual course of biomedical research has so far not yet found.

We are extremely grateful to two anonymous referees whose helpful comments on earlier versions have enabled us to make substantial improvements; we also wish to thank the editors for their valuable suggestions. We also note that the views expressed here are the authors' own and do not reflect the position of the National Institutes of Health or of the Department of Health and Human Services.

1. C.J.L. Murray and A. Lopez "Evidence-Based Health Policy Lessons from the Global Burden of Disease Study," *Science* 274 (1996): 740–43.

2. For data see the *Full Report: World Health Report* (Geneva: World Health Organization, 1990-), not only for 1995 but for other recent years as well.

Our aim is to understand the relation between the ways that people die and the kinds of weapons against disease that medicine has acquired and that it seeks to extend. We shall try to give substance to the familiar view that the diseases that contribute to the global burden of death are not well-aligned with the dominant directions in biomedical research. This misalignment, we claim, underwrites an obligation for individual researchers to reorient their inquiries and for the institutions that support research to alter their priorities. We begin with some facts about the worldwide distribution of disease and the strategies for treating those afflicted.

II

In the poorest nations, many people die prematurely from the diseases we have mentioned. Before we think about possibilities for new technologies, it is worth asking if we could make better use of the knowledge already available. Many nations use existing biomedical techniques to provide effective control of tuberculosis and similar diseases. But most of the countries where malaria, tuberculosis, respiratory infections, diarrhea, parasitic infestations, and so forth are rampant owe their trouble not to the lack of twenty-first century medical technology but to the absence of late nineteenth century sanitation. For example, one of the most historically successful strategies for malaria control is to eliminate stagnant water sources where mosquitoes breed. Even with emerging malarial resistance to drugs, it is certain that the burden of the disease could be greatly reduced by drainage projects combined with a more pervasive and effective system of public health in the affected countries.

Tuberculosis is a similar case. Inconsistent use of antibiotics has contributed to the spread of drug-resistant tuberculosis, but even most resistant strains can be beaten by a public health system that applies existing therapies quickly and thoroughly. As matters stand, millions of tuberculosis (TB) patients receive incomplete courses of drugs, bringing temporary respite at the cost of giving the TB bacillus an opportunity to evolve antibiotic resistance.[3] If well-funded, trained clinicians reached

3. The classic example is Russia after the collapse of the Soviet system, where overcrowded prisons provided the tuberculosis bacillus with extraordinary new ecological possibilities. See Laurie Garrett, *Betrayal of Trust* (New York: Oxford University Press, 2001), p. 191.

all these patients, the emergence of resistance could be slowed and the vast majority of tuberculosis deaths prevented without a single new technological idea.

It is immediately obvious, however, that making existing technologies more broadly accessible is far from simple. In malaria, Africa has a profound source of suffering that could be greatly relieved with clinical care that employs only procedures that are routine elsewhere (the courses of antibiotics that are administered in affluent countries); in tuberculosis, the Russian federation faces the same situation. But the infrastructure needed to supply that care no longer exists in the regions in question; neither does the funding nor political will to build it.[4] For outsiders to replace this infrastructure—building clinics, training physicians and nurses, supplying policy, and doing all of this for an indefinite period of time—amounts to taking responsibility for the public health of the nations involved. Such intervention may be an enormously valuable undertaking, but it is also very expensive and politically delicate. Developing new ways to fight tuberculosis is a difficult scientific problem, but rebuilding the former USSR's tuberculosis control programs may be an even harder political one.

It would thus be an error to think that, simply because a strategy for combating a disease is available in a particular, privileged, region of the world, medical research on alternative strategies is pointless. When the task of exporting the technology to other contexts is beset with large socio-political obstacles, the best means of bringing relief may be to find treatment better adapted to those contexts. Thus, when we consider biomedical research, we'll want to consider two sorts of goals: most obviously, ways of addressing diseases for which no treatment is available anywhere, but also new methods for fighting diseases for which the only available therapies cannot be exported to the contexts in which many of the afflicted find themselves. In other words, *one can't simply declare that*

4. Garrett's discussion of an Ebola virus outbreak in Kwikwit, Zaire (*Betrayal of Trust*, ch. 3) is especially pertinent here. She describes vividly how the public health network in Zaire was unable to provide the basic resources western medicine takes for granted—clean water and electricity were both lacking at the Kwikwit hospital. Thanks to the efforts of a group of doctors, including Zairois, representatives from the Center for Disease Control and members of *Médicins sans Frontières*, what might have been a devastating epidemic was stopped. A few years later, however, the hospital was in the same appalling state in which the MSF-CDC team had found it.

a disease is sufficiently researched just because it no longer has an impact on the affluent world.

Let's now turn to the relationship between the ways that biomedical research is currently directed and the global burden of disease. Best estimates are that the total spent on health research of any kind is around $70 billion.[5] The National Institutes of Health (NIH) alone will spend almost all of a $27 billion budget on medical research next year. The pharmaceutical industry, overwhelmingly based either in the United States or the European Union, will spend about $30 billion. In all, less than $3 billion of funding originates from the poorer parts of the world—Africa, the Russian Federation, the rest of Asia, and South and Latin America.[6]

One rather obvious way to measure the alignment between disease burden and the directions of biomedical research is to compare the amount of money invested in a disease with the proportion of the total budget ($70 billion) that a disease would receive if the allotment were made strictly according to the disease's fraction of the total number of deaths due to disease (approximately 40 million). We'll define a disease's *fair share* as the product of the number of deaths attributable to the disease and $70 billion, divided by 40 million.[7] Malaria kills one million people a year—including a high proportion of children—so its fair share is $1.75 billion; in 1995, the actual figure spent on malarial research was about $85 million.[8] Tuberculosis is responsible for two million deaths each year, and so its fair share would be $3.5 billion; high estimates are that it received $33 million in 1990.[9] (So if malaria sufferers historically receive only a twentieth of the scientific attention their plight merits,

5. Sheila Davies, *10/90 Report on Health Research 2000* (Geneva: Global Forum for Health Research, 2000).

6. *Monitoring Financial Flows for Health Research* (Geneva: Global Forum for Health Research, October 2001).

7. This notion plainly construes disease burden in terms of deaths, and so ignores (or, at least, underrepresents) those diseases that give rise to long periods of pain and disability; we shall come to terms with this point below.

8. J. Anderson, M. MacLean, and C. Davies, "Malaria Research: An Audit of International Activity," *Wellcome Trust Unit for Policy Research in Science and Medicine*, 1996.

9. C. Michaud, C.J.L. Murray, and B. R. Bloom, "Burden of Disease—Implications for Future Research," *Journal of the American Medical Association*, 285 (2001): 535–39. Funding has surely increased during the 1990s, especially with the knowledge that strains of TB resistant to the major antibiotics were surfacing in New York City. But even if funding has tripled or quadrupled, it is still miniscule compared to the scope of the problem.

victims of tuberculosis may only get a hundredth.) The single largest infectious cause of death is respiratory infection, killing four million people per year; the third largest cause is the general category of diarrhoeal diseases, which take 2.2 million lives a year. Their fair shares would be nearly $4 billion and $7 billion, respectively; in fact, the estimates award them about $100 million.[10]

Little imagination is needed to come up with an explanation for the disparities we have noted. Research dollars come almost entirely from the wealthy parts of the world, and the suffering from malaria, tuberculosis, and a large number of other infectious agents happens elsewhere. Nine hundred thousand of the 1.1 million who die from malaria in any given year are in sub-Saharan Africa; malarial death rates in the United States are negligible. In 1997, tuberculosis killed between 1.4 and 2.8 million people worldwide, with the best estimate falling near 1.8 million.[11] Over one million of these deaths occurred in six developing countries: India, China, Indonesia, Pakistan, Bangladesh, and Nigeria; by contrast, in the United States tuberculosis annually kills about 1200 people. In all of Europe, tuberculosis takes an annual toll of about 60,000, which is a tiny fraction of the total even before one considers that most of these deaths occur in the poorer nations of Eastern Europe, particularly in the Russian Federation. Wealthier nations typically have death rates measured in the hundreds.[12]

Tuberculosis and malaria are only the two clearest examples of a problem that pervades disease research. The most succinct way of describing this is the "10/90 gap": 90 percent of humanity's burden of disease receives only 10 percent of the world's health research resources.[13] The skewed distribution favors the diseases of the affluent world—cancer, heart disease, diabetes. How much of the resources

10. Ibid. These causes of death may not rate the most intense research attention either because they already have good low-tech solutions (e.g., oral rehydration therapy works for some diarrheas) or because they cluster many different diseases, each taking a relatively small toll. Thus the marginal rate of return for them may not be as high as it is for a single major killer, such as malaria or tuberculosis. Interestingly, the second objection applies to cancer research.

11. *Full Report: World Health Report 2000.*

12. Christopher Dye *et al.*, "Global Burden of Tuberculosis," *Journal of the American Medical Association* 282 (1999): 677–86.

13. *1996 Report of the Ad Hoc Committee on Health Research Relating to Future Intervention Options* (Geneva: World Health Organization).

aimed at these diseases could be diverted without appreciable loss is a serious empirical question; we *suspect* (but don't claim to know) that some diversion would be possible without significantly lessening the chances of success with respect to these diseases. The cost of the current bias is that vaccines and new antibiotics that would make a tremendous difference to quality of life in the nonaffluent world continue to go undeveloped, and we are confident that there will be a modification of the existing distribution in which the expected gains from research into new vaccines and antibiotics outweigh any expected losses from the support withdrawn from cancer, heart disease, and so forth.[14]

We've introduced the concept of a disease's fair share of research resources, noted the discrepancy between the fair share (as we define it) and the actual amount assigned to those diseases that mainly afflict people in the poorest parts of the world, and have offered an obvious explanation. But there are two obvious objections to the analysis presented so far, both of which would charge our talk of "fair share" with persuasive definition. The first would emphasize that the direction of scientific research cannot simply be a matter of the urgency of the practical problems; the second would question our measure of the global disease burden. We consider each criticism in turn.

There are sometimes good reasons for giving a disease more than its proportional share of research funds on the grounds that the available avenues for investigating it are more plentiful or more easily traversed than routes for tackling other diseases. Yet if a disease is both particularly deadly and appears vulnerable to research into new forms of treatment or prevention, it would seem wrong to shortchange it. We'll now argue that both malaria and tuberculosis are promising targets of

14. If our conjecture here is incorrect, then the right strategy might be to increase the total amount of support given to biomedical research to allow both for the retention of the advantages of present approaches to affluent-world diseases, and for the additional benefits of attention to the diseases that afflict poor nations. We would also note that some funding—and it's hard to estimate just how much—goes to develop "lifestyle drugs," substances meant to alter the user's body in ways not intended to cure disease. Attempts to overcome impotence, halt hair loss, reduce obesity, and remove wrinkles attract hundreds of millions of research dollars each year. These problems are trivial in comparison with the suffering wreaked by the deadly diseases of the poor world. (See Ken Silverstein, "Millions for Viagra, Pennies for the Poor," *The Nation*, July 19, 1999, pp. 13–19.) Since our primary concern in this article will be with public funding of science, rather than with biopharmaceutical research, we shall not press this point.

research, so that the proportionality approach that underlies our concept of fair share cannot be rejected on the suggested basis.

The malarial parasite implicated in most cases (*Plasmodium falciparans*) is a complex organism with a complex life cycle that could be interrupted in many ways: it is potentially vulnerable to antibiotics, to control of its animal vector, and to vaccines. Tuberculosis is also a promising subject for research, both because of the chance of finding new antibiotics and because of possibilities for vaccines. As we've already noted, an important role for technology is to provide ways for fighting disease, even in contexts in which the public health infrastructure has broken down. New drugs that circumvent resistance can make an important reduction in the short term, although it will be crucial to ensure that the application of those drugs now doesn't sacrifice chances for a more efficacious intervention later—it would be tragic if, because of the public health deficiencies, new antibiotics were quickly to generate new resistant strains. Our greatest hope is that many underresearched diseases, including malaria and tuberculosis, might one day be controlled with vaccines, and we shall focus on the prospects for vaccine development.

An ideal vaccine—one that is effective in a single dose—can be used to control, or even eradicate, a disease everywhere in the world. The smallpox eradication campaign carried out in the 1960s attests sufficiently to the enormous potential of vaccines: for a cost of less than a billion of today's dollars, an eleven-year program completely wiped out a disease that was highly infectious and killed two million people each year. Had smallpox eradication never taken place, that disease would have killed tens of millions of people and would now be doing damage comparable to that done by AIDS, malaria, or tuberculosis. Instead, it is simply gone, even in parts of the world where other forms of public health are in tatters.[15]

The molecular revolution in biology and biomedicine offers the potential for leaps forward in both drug and vaccine design. Its most visible symbol—the Human Genome Project—is often seen by researchers as a prelude to advances in basic biology (increased abilities to map and sequence genes and to engage in comparative analysis of the functions of genes in different organisms are likely to enhance our

15. Laurie Garrett, *The Coming Plague* (New York: Penguin, 1994), pp. 41–47.

understanding of intracellular metabolism and organismal development); the public, and some outspoken medical researchers, envisage the possibility of new techniques for addressing the major diseases of the affluent world—cancer, of course, hypertension, diabetes, Alzheimer's, bipolar depression, and so forth. Yet the ability to sequence rapidly, originally demonstrated on bacterial genomes, also enables us to analyze the tricks of major pathogens. In principle, one can map and sequence the genome, hunt for proteins that are exposed on the surface of the pathogen, design a harmless vehicle that will stimulate the production of antibodies that would neutralize the pathogen, and so prepare the immune system against its attack. There could easily be a huge (and not desperately expensive) project to analyze pathogen genomes and to generate potential vaccines (which could, at least sometimes, be tested quite rigorously on animal models).

High technology efforts wouldn't always work, of course. The example of AIDS—one of the few infectious diseases threatening enough to the affluent world to attract billions of research dollars—has been the target of a very intensive research effort. Not only the price and difficulty of distributing drugs, along with fears of HIV's developing drug resistance, lead many researchers to believe that the only long-term solution to AIDS will be a vaccine. The fact that our detailed knowledge of the HIV genome hasn't yet translated into success in making a vaccine demonstrates that there's no *guarantee* that sequencing pathogens will deliver the goods. But one might also conclude, from the half-century that has elapsed since the discovery of the molecular basis of sickle-cell anemia, that molecular techniques are hopeless in the battle against genetic disease. To use an analogy one of us has deployed elsewhere, we should think of the pathogen sequencing project as giving us a vast number of tickets in a number of lotteries; we can't predict where we'll be successful, and how big the prizes we'll win, but we'd be profoundly unlucky to come up completely empty.[16]

In the case of malaria, several vaccine candidates are at or near Phase I human trials, but it's estimated that a useful malaria vaccine is at least

16. See Philip Kitcher, *The Lives to Come: The Genetic Revolution and Human Possibilities* (New York: Simon & Schuster, 1996). Our view is that the chances are actually significantly higher from the pathogen sequencing/vaccine development project than from the standard ways of applying information from the Human Genome Project.

ten years away.[17] This is probably optimistic. Without more funding, it's likely that promising leads won't be explored, so that the ten years will balloon into twenty or more. In the case of tuberculosis, vaccine research is hardly off the ground. Because of the high expense of treating the disease, and its propensity to become drug resistant, a tuberculosis vaccine is particularly desirable. But tuberculosis vaccine research receives even less funding than malaria vaccine research, and vaccine development has lagged accordingly. No new tuberculosis vaccines are in clinical trials today.[18] Thanks largely to the recent sequencing of the TB bacillus genome, promising new avenues for research are open, but the research is only in the earliest stages, and the funding needed to carry it out with a serious chance of success isn't yet available. Indeed, at current levels of funding, a vaccine may never be developed.

We've tried to defuse one line of objection, one that stresses the need to take research promise into account in the apportionment of funding. We now turn to the second, which charges that we've operated with too crude a measure of suffering by concentrating on numbers of deaths. It would be correct to note the possibility that some diseases inflict more suffering for a given mortality rate than do others: a disease that kills some number of people late in life may cause much less suffering than one that kills the same number of people but strikes earlier and inflicts long periods of disability in many nonfatal cases. Extensive work has been done, largely by the World Health Organization, so that these considerations can figure in a more nuanced calculation of the burden of disease.

Instead of simply assuming that mortality rates correspond directly to the amount of suffering, the most common practice has been to introduce a unit known as the DALY (Disability-Adjusted-Life-Year). The fundamental idea is that we should look for the number of years of life lost because of the disease. Life is lost not simply through early mortality but also through the discounted value of years of life lived with disability, where the discounting is based on widely shared views about the value one would attribute to a future year of one's life in the diseased state as

17. "Quest for Malaria Vaccine Revs Up, But Much Work Remains," *Bulletin of the WHO* 79 (2001): 1002–1004.

18. There's an old vaccine, BCG, that has been in use for over seventy years. It can provide children with some protection against some strains. For the most prevalent form of tuberculosis, adult pulmonary tuberculosis, it's quite useless.

opposed to the value one would assign to a disease-free future year. As the idea is developed, there are two main assumptions, one egalitarian and the other age-inegalitarian. The egalitarian assumption takes a year of human life to be of equal value regardless of location, race, social background, or level of education, and also incorporates the claim that death is premature if it occurs before a standard of 80.2 years for women and 80 years for men. The age-inegalitarian assumption does place a different value on years of life at different stages of a human being's existence: initially, the value of a year rises rapidly with age, peaking in the mid-twenties, and thereafter gradually declines to about a third of its maximum value by the age of 80. This assumption is explicitly based on studies of how people actually value different stages of human life; part of its rationale is the idea that adults of the most valued ages can make the greatest contributions to their societies and to the well-being of their dependents.[19]

We take the egalitarian assumption to be fully defensible, and, in particular, we regard objections to a uniform standard of premature death as ungrounded. An obvious worry is that the egalitarian assumption supposes that diseases that strike well after the end of the usual lifespan of a human being in the non-affluent world exact a large number of DALYs. Were we to revise the DALY measurement to incorporate the expected lifespan in particular regions, however, then we'd already be acquiescing in a particular burden of disease as the norm for the inhabitants of those regions, writing off a portion of their suffering as something that didn't count. Affluent people who view it as a hardship to be denied health, even in their seventies, have no reason to consider a loss of life—life unencumbered by disability, recall—unimportant in someone else. If people in poorer parts of the world can't expect long and active lives, that doesn't make it less bad for them not to have them; indeed, as we've suggested, that expectation itself represents part of the burden of disease that afflicts them.[20]

19. C. Michaud, "The Global Burden of Diseaes and Injuries in 1990," *International Social Science Journal* 51 (1999): 287–296.

20. We suspect that those inclined to make the objection we've just briefly discussed would have argued, in an earlier day, that the deaths of slum-dwellers or coal miners in their own societies couldn't be considered premature because, after all, they lived as long as they had any right to expect. We hope that noting the parallel will remove the urge to make either complaint.

We're far less confident about the age-inegalitarian assumption. For present purposes, however, we don't need to resolve the issues that it raises. Unlike the commitment to a uniform standard of premature death, the age-inegalitarian assumption doesn't increase the DALYs associated with diseases that afflict poor nations.

Our earlier conclusion was that, when human suffering is measured by raw mortality rates, the distribution of biomedical research is skewed with respect to the distribution of suffering. How do things stand when we replace mortality rates with DALYs lost? Roughly the same. Tuberculosis and malaria continue to exact the majority of their cost in the non-affluent world, and the harm they do relative to other causes is approximately the same as on our earlier (mortality-based) estimate. Tuberculosis causes 2 million out of 56 million deaths—slightly less than 4 percent of deaths; it costs about 2.8 percent of DALYs, a small reduction but by no means enough to eradicate the spectacular inadequacy of tuberculosis research funding. Malaria maintains the same significance as before, moving from causing about 2 percent of deaths to being responsible for 2.3 percent of DALY loss. In these cases the two most obvious ways of estimating disease burden yield the same conclusion: research investments and global disease burden are mismatched, and matters are no different when we turn to respiratory disease or diarrhea. Interestingly, a DALY analysis does show new diseases of poor regions that attract very little research attention—most prominently those parasitic infestations that do not kill but produce blindness or severely impaired mobility. A forceful example is onchocerciasis, also known as river blindness. This African infectious disease slowly blinds its victims without killing them; the WHO's statistical analysis of its effects therefore shows a negligible number of deaths but an annual loss of over one million DALYs.[21]

It's worth noting that we've focused only on the immediate costs of disease in the poor parts of the world. In our judgment, the consequences of tuberculosis, malaria, respiratory infections, diarrhea, and their ilk are so economically expensive that the world would be richer and more politically stable if we could eradicate them. Economists have estimated that malaria costs sub-Saharan Africa at least $1.7 billion a year.[22] In fact,

21. *Full Report: World Health Report 2000.*

22. D. S. Shepard, M. B. Ettling, U. Brinkmann, and R. Sauerborn. "The Economic Cost of Malaria in Africa," *Tropical Medicine and Parasitology* 42 (1991): 199–203.

the total burden may be higher in that nations with endemic malaria have economic growth slowed by 1.3 percent per year, suggesting that the economy of Africa might be larger by a third had malaria been wiped out two decades ago.[23] Effective technology for eliminating malaria in Africa might thus serve as a basis for ameliorating other forms of suffering. Plainly, if such socio-economic considerations were incorporated into a refined conception of a disease's fair share, they would only increase the gap that divides fair share from actual expenditure.

Before we consider the possibility that the misalignment grounds an obligation to change the ways that research is funded in the affluent world (which is, after all, where research happens), we'll close this section with a quick look at the ways that resources are actually divided. The funding for science that comes out of biopharmaceutical business roughly matches the funding provided by government. Each of the ten largest drug companies has, in theory, the capacity to direct in excess of a billion dollars a year to any biomedical research problem.[24] In total, the annual research and development expenditure of the international pharmaceutical community is estimated at $30 billion. Each year, the lion's share of drug companies' resources is directed toward the most profitable projects they can find, and the resulting research agenda is dramatically imbalanced. Drug companies aspire to revenues of around a billion dollars a year for a single drug. In the case of tuberculosis, for example, there's no wealthy market for any new drug, so the profits are unavailable, and it's hardly surprising that there are no such drugs in the pipeline of any major company. Over twelve hundred new drugs were developed between 1975 and 1997, but only thirteen were aimed specifically at tropical diseases.[25]

Plainly, the market imposes constraints on drug research; even in a merciless market, however, some corporate idealism is possible. Merck maintains a vaccine division (the only one in the major drug companies) despite its lower profitability relative to other projects. In response to political pressure, Merck also has also invested modest resources in adapting a veterinary drug, Ivermectin, for combating a form of river blindness—which infects 18 million people in the world today and has

23. J. L. Gallup and J. D. Sachs, "The Economic Burden of Malaria," *American Journal of Tropical Medicine and Hygiene* 64 (2001): 85–96.

24. Pfizer, currently the largest drug company, spends $5 billion a year on research.

25. See Silverstein, "Millions for Viagra."

blinded 270,000 of them, almost all in Africa.[26] Merck donates the drug to a project (led by WHO and the World Bank). Investors haven't punished the company for these efforts, partly because they're undertaken on not too large a scale, and partly because they bring public relations benefits. (The drug, Mectizan, features both in Merck's own promotional material and in press coverage of the ethical stances of drug companies.)

Government funding of biomedical research is virtually entirely the province of the NIH, which, as already noted, will distribute about $27 billion in research funds in 2003. The NIH clearly displays the 10/90 gap. Out of the $27 billion, $4.7 billion are allocated to cancer research, $2.8 billion to the National Heart, Lung, and Blood Institute, $1.6 billion to the National Institute of Diabetes and Digestive and Kidney Diseases, and $1.4 billion to the National Institute of Neurological Disorders and Strokes. In total, $14.7 billion, or just over half, of the NIH budget goes to departments that are clearly oriented toward solving only affluent-world problems.[27] The remaining funds go to departments that might be oriented toward the diseases that afflict the poor, but, by and large, are not.

On the borderline between affluent-world and poor-world problems is AIDS. Half of the NIH's $1.5 billion AIDS budget in 1999 went toward vaccine research (directly or indirectly); since then the NIH budget has doubled, as has the amount given to AIDS research. As the emphasis of AIDS research shifts further toward vaccine development, the NIH is now investing significantly more than a billion dollars in trying to discover a vaccine for this disease. Of course, this reflects AIDS' unusual status as an infectious disease that is a dangerous killer both in the poor parts of the world and in affluent nations.[28]

Compared to these figures, the amount of money explicitly set aside for tropical disease is tiny. The only department officially devoted to international health problems is the Fogarty International Center, whose

26. "WHO Factsheet: Onchocersiasis," World Health Organization, Geneva, February 2000.

27. In addition to those mentioned, the other pertinent departments are: NIMH, NIA, NIAMS, NIAAA, NIDCR, and NIDCD.

28. As is well known, the plight of poor people who suffer from AIDS is exacerbated by the inflated prices of the drugs used to combat HIV in the affluent world. We applaud the Bush administration's initiative to commit funds to fighting AIDS in Africa, and *hope* that this will be carried through, that the program will be extended to other impoverished parts of the world, and that it will not be accompanied by prohibitions against the use of effective methods for controlling the spread of the disease (i.e., condom distribution).

budget of $63 million is almost a rounding error (although one for which we should be thankful). The NIH budget also includes $100 million for the Global Fund to Fight HIV/AIDS, Malaria, and Tuberculosis. The examples don't exhaust the list of tropical disease projects, but they are representative. Indeed, when one factors in the medical needs of American military personnel who might have to serve in remote places, it's clear that government support of biomedical research only occasionally—and accidentally—addresses the needs of the poor.

What would happen if some of the funds currently earmarked for diseases that afflict people in the affluent world were diverted to such diseases as malaria and tuberculosis? As we have said, that is an empirical question and we don't know the answer. We do, however, believe that it would take some serious work to defend the idea that taking a few million dollars from cancer research would materially lessen the chances of making progress in fighting cancer.[29]

A final, prudential, point: some of the infectious diseases that flourish in the non-affluent world have already become drug resistant and have returned to haunt the rich. Tuberculosis is an obvious case: as resistant strains reach the United States, the death rate from the disease remains low, but the costs of controlling the infections has risen to between $700 million and $1 billion.[30] Many leading microbiologists believe that it's possible that, in seventy years, current trends may leave us facing a world in which no antimicrobial drugs remain effective.[31] Further, without more attention to the possible ways that increased clearing of tropical rain forests encourages pathogen "jumping" from nonhuman animals to our own species, we may be vulnerable to extraordinarily devastating diseases.[32] In short, it's not clear that the division of disease burden—with the protected rich and the vulnerable poor—is sustainable.

29. It should also be noted that the NIH's budget is currently growing at a rate enormously in advance of inflation; in 2003 it will have doubled in five years, from $13 to $27 billion. Thus an increase to a half-billion dollars each in the annual funding for the malarial and tuberculosis vaccine projects would do no more than slightly slow the *rate of growth* of research into chronic diseases.

30. Ruth E. Brown, Bess Miller, William R. Taylor, Cynthia Palmer, Lynn Bosco, Ray M. Nicola, Jerry Zelinger, and Kit Simpson, "Health Care Expenditures for Tuberculosis in the United States," *Archive for Internal Medicine* 155 (1995): 1595–1600.

31. Garrett, *Betrayal of Trust*, p. 577.

32. See Garrett, *The Coming Plague*.

III

There are familiar arguments to the effect that we should do something about the situation just reviewed, arguments that make the imperative moral rather than merely a counsel of prudence. From a straightforward utilitarian perspective it's not hard to argue that frivolous expenditure by affluent people should be redirected toward the alleviation of human misery, and, in the same way, one can indict the investment in research into new "lifestyle" drugs or the current neglect of many diseases.[33] Even without commitment to utilitarianism, or any form of consequentialism, it's hard to resist the conclusion that the imbalance of effort represented by the 10/90 gap imposes some form of obligation to change how global health is pursued.[34] The amount of suffering that lies behind the recitation of facts in Section II—condensed and sparse though our review is—calls out for a response, and one cannot simply dismiss the burden of disease as distant or no concern of ours.[35] If pleas for attention to global health (or global hunger) are met with apathy, that is surely the result of a sense that the case is hopeless; that no matter what we might do, the problem wouldn't be solved or even significantly alleviated.[36]

In our judgment, the sense of hopelessness arises from the conclusions that are typically drawn from the moral exhortations: although it's correct to think that the skewed distribution of the world's disease burden requires *somebody* to do *something*, we think it's important to think broadly about *what which* individuals (or institutions) should do. The standard arguments focus on ordinary citizens of affluent nations and on possibilities for giving aid to distant people who are afflicted with

33. The arguments are made with great force by Peter Singer, "Famine, Affluence, and Morality," *Philosophy & Public Affairs*, 1 (1972): 229–243; Peter Unger, *Living High and Letting Die* (New York: Oxford University Press, 1996).

34. Thus we won't try to offer a moral theory to buttress the claim that we have a prima facie obligation to respond to the plight of afflicted people in poor countries, because we don't see any plausible moral theory that fails to endorse any such obligation. The real issue, we maintain, is whether the size and severity of the challenge undercuts the view that what appears to be obligatory is genuinely demanded of us.

35. Unger, *Living High*, makes the case against evasion on such grounds, and does so with enormous thoroughness and clarity.

36. Unger, *Living High*, refers to this as "futility thinking," and we agree with him both that such thinking lies behind the evasion of moral responsibility and that it is fallacious. As we'll explain in the text, we think that the fallacy is a bit more complicated than Unger takes it to be.

treatable diseases; philosophical concern for the health of the poor typically doesn't focus on opportunities for *research* but rather on the *applications of existing knowledge* that are not made because the funds are lacking. In the classic example, we're invited to consider two options, responding to a request for oral rehydration therapy that will save the lives of thirty children or failing to make that response.[37] Realistic predicaments would allow a far more extensive set of options, and it's worth noting that it's not self-evident that relations among choices are always preserved when the choice set is enlarged. We don't think that this is more than a philosophical quibble in the case at hand; our aim is to explore a much more systematic strategy for responding to the global burden of disease, not to dispute what appears to us to be uncontroversial—to wit, that *if* our choices were restricted to sending the money or not sending it we'd have an obligation to do the former.

Numbing apathy sets in, we believe, because the lack of any systematic strategy makes plausible a skeptical response.[38] This response has three components: the first focusing on the size of the problem, the second on the costs of addressing it, and the third on our obligations to those who stand in special relationships to us.

The skeptic starts from the idea that sending the money to cover the costs of oral rehydration therapy is simply applying a band-aid to a very serious condition. Considered from one perspective the analogy is clearly fallacious: the relievable suffering of an individual child doesn't stand to the total affliction of people in poor countries as a minor cut stands to the overall condition of a person with grave damage to major organs and systems. The skeptical comparison, however, is intended to draw attention to the systematic causes of suffering in impoverished regions of the world, causes that are not in any way removed or muffled by sporadic efforts to give oral rehydration therapy to a very few children. The skeptic will point out that we may help a child who needs oral

37. See Unger, *Living High*; Singer, "Famine, Affluence, and Morality," uses similar examples.

38. This might be called the Malthusian Response, after the author of the *Essay on Population*, with its attempt to vindicate the gospel dictum that the poor will always be with us. The name is useful, because the standard diagnosis of Malthus's mistake views him as overlooking the possibility that technological developments can disrupt his algebraic assumptions, but it also has misleading connotations, and so we've used the less specific "skeptical response."

rehydration today because she has had diarrhea from drinking contaminated water, but she will continue to live in a place where the water supply is always untrustworthy, where the chances for many forms of parasitic infestation are high, where malaria and tuberculosis are endemic, where there is no chance for education, where women are treated like commodities, where there are interminable tribal squabbles that flare into hostility, and so on and so forth. Oral rehydration today is a band-aid in that it fails to remove any of the many causes of her continuing distress. Thus, it isn't the case that, all by itself, the action of giving will genuinely reclaim the lives of those toward whom the aid is directed.

One might dispute the point, arguing that the assessment that the child saved today will be vulnerable tomorrow is unduly pessimistic.[39] Yet the skeptic will remind us of the full range of the suffering reviewed briefly in Section II. Every month, a million children suffer and die of the same conditions we have attempted to alleviate, and for this vast majority the world remains unchanged. To all appearances, when we and the few children we have saved are gone, this tide of mortality will still roll in unabated. Anybody studying it will see our efforts as a gesture, a tiny island of generosity, not a solution.

Once we are aware of this point, we can appreciate a second skeptical concern. To recognize the obligation to give now would produce a situation so little different from the initial state that we would have an equally pressing obligation to go on giving. The skeptic invites us to consider two possible futures, the Status Quo and the Bleak World. In the Status Quo, children in poor nations suffer and die somewhat earlier than they do in the Bleak World; in the Bleak World their lives are prolonged, thanks to generous responses from affluent people. In the Bleak World the entire population of the affluent world does what is viewed as their obligation—they give and they keep giving—but the scale is so large that the donors find themselves committed to a policy that withdraws funds from many, indeed most, of the enterprises that give pleasure to people in the affluent world: opera houses and theaters close, sports tournaments are cancelled, ancient monuments are allowed to decay,

39. Unger (*Living High*, pp. 146–149) suggests that affordable contributions would give a child in a poor country a 90 percent chance of reaching the age of 21, so he would presumably make the charge of pessimism. The skeptic might respond that the success criterion that Unger uses is still too weak.

any natural preserves that could be turned to purposes of alleviating the suffering of the poor are commandeered to such ends, and so on. The benefits gained by this reallocation of resources are only slight: the lives rescued from disease and malnutrition are somewhat longer and somewhat less painful. In the end, however, many things that seem to make the actual world a better place are forsaken without completely solving the problem of global disease.[40]

There's an obvious reply: we're not faced with a choice between the Status Quo and the Bleak World. Perhaps we can make significant improvements to the actual world by diverting funds from rich countries to poor countries and relieving the burden of suffering there. As we shall argue below, we think that this is correct, but we don't want to engage in controversy about just how large the sacrifices from continued giving would have to be; instead, we want to enlarge the space of options available. If our choices were restricted to individual giving in reaction to individual affliction, then we think that it's genuinely unclear how drastic the consequences for life in the affluent world would be.[41] Further, the skeptic reminds us that it isn't evident that alleviation of suffering trumps everything else, that there are no occasions on which it is better to permit additional suffering in order that some other good be realized.

At this point, we can introduce the third theme in the skeptical response. If the costs are as skeptics believe, then the task before us isn't simply to give up a few luxuries in order to address suffering among the distant poor, nor even to settle for a more Spartan public and cultural life. There are sacrifices we must make in the provision of goods to those with whom we stand in special relations: our children, our elderly parents, our loved ones, our friends. Perhaps in some instances it will be

40. Here the skeptic appeals to the kinds of considerations that lead Derek Parfit to identify the claim that a world in which billions live lives that are just worth living is preferable to a world in which a much smaller population enjoys lives that are full and rich as the Repugnant Conclusion (*Reasons and Persons*, Oxford: Oxford University Press, 1984, part IV). One of us has argued elsewhere that the problem posed by Parfit is insoluble; see Philip Kitcher, "Parfit's Puzzle," *Noûs*, 34 (2000): 550–77. The upshot of that analysis is that we ought to adopt a multidimensional approach to human well-being.

41. Perhaps they would not be as dire as the skeptic believes. Surely (one might think) if *everyone* in the affluent world gives to help the afflicted, the costs will be spread sufficiently thinly that much of the quality of life can be sustained. We won't try to resolve this issue because we believe that a purely reactive approach, even if effective, is not the best way to address the problem.

possible to justify expenditures on education on grounds that these will make possible a more effective response to the enterprise of giving more to the cause of global health, but all our domestic efforts to improve the lives around us are subordinated to this more urgent project. There is a genuine worry about what would become of our human relationships if all our dealings with friends and relatives were held hostage to our obligations to the many unknown people who suffer in distant places.

When we combine concerns about the lot of those who are dear to us with the previous themes in the skeptical response, it is easy to anticipate a collective action problem. Assume that if all of us do our part, then the burden of global suffering will be substantially eased; the skeptic finds this assumption optimistic, but we'll allow it for the sake of argument. If none of us contributes, the distant millions will suffer and die. You reason as follows: If all others contribute and you do not, then the lives of faraway people will go almost as well (any difference will be negligible), and those near and dear to you will benefit greatly from the funds you would have sent (your elderly parents will enjoy the best care, your children will have wonderful educational opportunities, and so forth); if all (or even the vast majority) of the others fail to contribute and you contribute, then the gains for the distant sufferers will be miniscule, while your relatives and friends will be shortchanged. Not contributing looks like a permissible course of action, given that it seems to have good effects, whatever the others do.[42]

As we have said, we don't find the skeptical response conclusive, and we've indicated some potential ways of contesting it. But we don't think that the best way of meeting skepticism consists in pursuing those options—arguing, say, that a determined policy of individual contribution really can alleviate the global burden of disease. Instead we believe that skepticism can help us think more intelligently about the problem of distant suffering; reflections on scale direct us to consider root causes and methods for eradicating them; attending to the various different kinds of things we value can inform judgments about what sacrifices we should struggle to avoid; noting that there is a looming problem of

42. We won't try to set this up formally, but there are obvious similarities with both the Prisoner's Dilemma and the Tragedy of the Commons. A principal difference, of course, is that the payoffs represent the values that a reflective, altruistic, moral agent would assign: according to these payoffs, Don't Contribute is superior to Contribute no matter which course of action the others pursue.

collective action can inspire ways of combining individual and collective efforts.

Instead of a blanket obligation to send checks to UNICEF, citizens of affluent nations have complex responsibilities that differ with their opportunities and roles. Many of them should contribute more to reactive efforts that aim to reduce the suffering of those already afflicted, but many people have more specific obligations to contribute to agendas that complement such projects. Some of the most pressing obligations and most potentially potent contributions belong to members of the biomedical research community.

IV

The skeptical trap is to frame debate by envisaging a line of worlds ranging from the Status Quo to the Bleak World: the worlds are distinguished by the amount of money affluent people send to provide such things as food, oral rehydration therapy, the drugs made available by past research, and what we can expect by continuing with the same research agenda. We'll leave it to others to combat skepticism by claiming that the scale along the line is wrong, or that even if it's right, we have some obligation to head along the line that the skeptic draws. Our strategy is to get off the line. As we noted in Section II, the creation and maintenance of public health infrastructure could substitute efficient preventative measures for recurrent reactive treatments. Further, if the scientific research agenda were modified (specifically, if far more resources were committed to studying the diseases that afflict people in poor nations) then we might obtain systematic ways of eliminating major causes of suffering, or at least diminishing their power.[43] The social change we need to see is the extension of successful public health infrastructure and practices to parts of the world that lack them; the technological change is the creation of effective drugs and vaccines that can stop third world diseases in third world conditions.

In our judgment, people in affluent countries have at least three different types of obligations. First, we're obliged to provide relief for those

43. In effect, we're responding to skepticism in just the way that the historical Malthus has been answered: we propose that seemingly inevitable human suffering can be evaded by social and technological change.

in imminent danger of severe disability and death; this involves precisely the sorts of action that arguments about global suffering typically recommend, giving to organizations like UNICEF and OXFAM that respond to crises. Additionally, the affluent nations collectively have an obligation to attack the causes that make these kinds of suffering chronic. This obligation includes many projects, one of which is to bring to the poorer parts of the world the basic preconditions for preventing disease and dealing systematically with it: clean water supplies, clean air, shelter, a reliable food supply, clinics and hospitals with electricity and facilities for sterilizing equipment, schools.[44] Citizens in rich nations have the obligation to contribute taxes so that funds can be given to promote a public health infrastructure in poor countries, and even to lobby their governments to enact measures that increase taxation specifically to provide the facilities we've listed. Third, the biomedical research community in the affluent world has the obligation to modify the current research agenda so as to give much greater weight to investigations into the diseases that produce extraordinary suffering among the poor. One of the sacrifices that citizens of the affluent world should be prepared to make is the diversion of resources from inquiries into chronic disease that bring relatively small marginal chances of success. We note explicitly that the three-component program we envisage is directed at alleviating the plight of the poor *wherever they are*; this means that one part of the commitment to building public health infrastructure consists in making public health facilities available to marginalized groups within affluent societies.[45]

In the long run, building public health infrastructure is probably the most important of these three goals, and it is the most difficult to achieve. This is not, however, because such technical problems as secur-

44. We include a system of education as a component of the public health infrastructure both because of the well-documented connection between educational level and controlled birth rate, and because of the importance of understanding the social changes sometimes needed to stop the spread of infection.

45. Part of the obligation thus consists in making medical care available to all people in the United States. For a clear and powerful description of the ways in which U.S. medicine fails completely to meet the needs of many American citizens, see Garrett, *Betrayal of Trust*, ch. 4. We also note that concern for the poorer members of affluent societies might also justify a policy of protecting those areas of biomedical research aimed at diseases that disproportionately affect the poor; here we think of the impact that diabetes and sickle-cell anemia have on African Americans.

ing a clean water supply and controlling mosquito populations are unsolved, or because those solutions are prohibitively expensive. As we discussed in Section II, the obstacles are political and social: even though we know how to maintain the health of a population at a high level, it can be extremely hard to do so in nations that are politically unstable, or corrupt, or that distrust intrusion by the affluent powers. Each region, each country presents very difficult special problems of politics and culture that must be understood and energetically addressed as their infrastructure is built.

We believe that the burdens of disability and disease are themselves contributory causes to the poverty and political instability that make good public health difficult to establish, and that the apparent indifference of the wealthy nations to those burdens exacerbates these problems. For a sovereign state to allow outsiders to greatly alter the way it handles public health requires a condition of trust that is frequently lacking; one way to foster trust would be to show that the affluent world has a serious concern for the suffering that infectious diseases bring to the poor—that it is prepared to contribute emergency relief and, perhaps more importantly, that it is prepared to devote the resources of its sciences to the development of effective drugs and vaccines that will be distributed to those who need them without regard for their ability to pay. Since it is both a partial solution in its own right and the potential key to making infrastructure improvement practical, a reformed health research agenda is the third prong of our strategy, and perhaps the most urgent one at this time.

In the rest of this article, we'll focus on this third prong of our strategy. We'll argue first that individual researchers have an obligation to direct their research toward remedying the global research gap, and subsequently that the institutions that fund and direct research have a similar obligation.

V

There is a tendency to think of the obligations of scientists as very simple; to view science as a private pursuit, driven by the love of truth. To be sure, virtually everyone would concede that in seeking the truth about aspects of nature, there are certain kinds of things that can't be done: human subjects shouldn't be treated as mere means to the

establishment of a decisive experiment, for example.[46] Provided that scientists conform to some relatively straightforward directives in designing their experiments, however, a standard view is that they have no further responsibilities; they should pursue the inquiries that strike them as most promising.

In developing a more adequate picture of the role of the scientist and the obligations it brings, it's important to distinguish two different settings in which scientific research is performed. Many people use their scientific training to work on projects ultimately motivated and constrained by the marketplace; in certain ways, these scientists subordinate their role as scientists to their role as employees of profit-seeking corporations; that isn't to say, of course, that they can legitimately seek to defend findings they know to be false, or anything of a similar nature, for in both roles they are subject to obligations of honesty. We'll be principally concerned with the second group of scientists, those whose research isn't directly linked to their attempts to provide marketable products; those for whom the stereotype of disinterested truth-seekers is most plausible. So, in discussing the obligations of scientists, we'll be concerned with those who work in universities or in laboratories with no direct mandate from large corporations, not those whose research role is framed by the quest for economic gains.[47]

We think that the minimal conception of the scientific role, sketched in the first paragraph of this section, is incorrect. In becoming a scientist someone takes on a new role, and that role brings obligations.[48] To fulfill the role, scientists should devote their energies toward achieving the goals of the branch of science in which they work. What are these goals? Not simply discovering truth, for the truths about the world are too many and, for the vastly greatest part, too trivial. Were the biological community to decide en masse that all further research efforts were to be directed toward cataloging the exact number of bristles, hairs, or

46. Two well-known examples of cases in which this moral requirement was violated are the Tuskegee study of syphilis (in which Black patients were left untreated and ignorant of their condition) and the grotesque experiments of some Nazi doctors.

47. Of course, the line between the university research laboratory and the marketplace is becoming increasingly blurry. For reasons that will become obvious from our discussion, we take this to be a matter of considerable concern; see also Philip Kitcher, *Science, Truth, and Democracy* (New York: Oxford University Press, 2001).

48. Here, and in what follows, we've been influenced by Michael Hardimon's revival of role obligations; see his "Role Obligations," *Journal of Philosophy* 91 (1994): 333–63.

similar structures found in individual organisms of an enormous variety of species, we'd rightly condemn the decision. This is because the aim of the sciences is to answer *significant* questions, and there's no significant question at which the enumeration of bristles is directed.

We suppose that there are two sources of scientific significance. Some questions are significant because correct answers to them would enable us to solve practical problems, to intervene in nature in some way, or to predict its future course. Even investigations that have no direct practical benefits are sometimes undertaken because we suspect that there will be possibilities of prediction and intervention that will ultimately flow from them. Yet this is not all. Certain questions inspire scientific effort not because—or not merely because—answers would bring practical benefits (directly or indirectly) but because of prevalent human curiosity. It's hard to imagine what pragmatic payoff would result from sorting out the exact relationship among the australopithecines; even if our understanding of the processes through which zygotes develop into mature organisms would open up new possibilities for medical treatment, that understanding is still valuable independently, for its own sake.[49]

We'll say that the pursuit of science in a society is *well-ordered* when the research effort is efficiently directed toward the questions that are most significant.[50] In the contemporary world, the sciences are potentially applicable to a large number of questions that impinge on the well-being of many people, almost certainly too many for all of them to be thoroughly and systematically pursued. Well-ordered science must inevitably be selective, and we propose that it should respond to the choices that would be made by an ideally informed collection of representatives of divergent points of view, with each representative committed to learning about and taking seriously the needs and interests of each of the others. To suppose that the collection of representatives be confined to the points of view within the affluent world is, in our judgment, to make an important moral error. Hence, our conception of well-ordered science supposes that weight must be given to the needs of those who suffer the enormous burden of infectious disease in the poor regions of the world.

49. Here we summarize a line of argument developed at much greater length in Kitcher, *Science, Truth, and Democracy*.

50. This conception is articulated further in ibid., ch. 10.

Our defense of the claim that a narrower conception of the set of viewpoints represented is morally mistaken will be relatively brief. It's a matter of historical accident that some people live in societies with the resources to commit to scientific research, and, we suggest, no ideal for the direction of scientific inquiry should reflect such accidents. It's easy to extend a familiar Rawlsian thought experiment and to imagine ourselves choosing an ideal for the conduct of science without yet knowing whether we shall find ourselves in one of the societies lucky enough to support research; as we understand that thought experiment, the obvious decision is to include the perspectives of all in the framing of well-ordered science. Further, if, as we believe, the principal opposition to responding to human needs, even when they are distant, is that such responses are inevitably futile, then no such counterargument is available when the envisaged responses feature the development of the sciences. The institution of science fulfills a valuable social function because scientific knowledge brings us both intellectual and practical goods, and societies invest in and support the institution largely because they believe in the efficacy of research to address practical problems.[51] Neither the claim that scientific inquiry is futile nor that it can only address the needs of a restricted group of people has the least plausibility. Any narrowing of the class of viewpoints represented in well-ordered science thus reflects not hard-headed realism but a callous neglect of those who are poor and distant.

Having outlined and briefly defended an ideal, we now consider what obligations flow from it. Taking on the role of a scientist brings with it the responsibility of contributing to well-ordered science. That responsibility doesn't necessarily preclude the possibility of addressing issues that have purely theoretical significance, of attempting to satisfy human curiosity. Yet it would be a travesty of well-ordered science to propose that only two kinds of inquiries be pursued: those that attempt to satisfy disinterested curiosity and those that seek to meet the practical needs of citizens of affluent nations. Hence, in the presence of the 10/90 gap, there's ample reason to think that scientific research is not promoting its proper goal. In consequence, scientists have an obligation to do what they can to remedy the situation.

51. For further development of this theme about the function of scientific research, see Philip Kitcher, "The Scientist's Role" (John Wesley Powell Lecture, 2003).

For some scientists, of course, there's little that can be done directly; they work in fields that are too remote from those that bear on the neglected issues. Biomedical researchers, however, can often do much more. Some have the option of pursuing inquiries that might have value in relieving arthritic symptoms or of working on some model organism that might be useful in studying infectious disease. By taking the latter course, they can move the research community closer to a state of well-ordered science. In our judgment, they have the obligation to do so.

Further, all scientists have an obligation to engage in political activism, to campaign publicly for greater investment in research that would address the disease burden of the poorer regions of the world. But the capacity of ordinary scientists to bring about much change is probably limited; they are typically inclined and trained to discover facts, not influence political opinion. The administrators who control the flow of cash to science play on a more public stage. We turn next to their responsibilities.

Several different types of organizations fund enough research substantially to affect science policy. Governments of rich nations are obviously important, although we think it more useful to consider these as a few different, semiautonomous institutions—in the United States, for example, we might list the NIH, the NSF, and the complex of national laboratories. A second type consists of the biopharmaceutical industry, considered either as a bloc or as individual companies. Independent foundations like the Wellcome Trust and the Gates Foundation make up a third. The university research system is a fourth, although once one considers the sources of funding it becomes clear that university laboratories can't operate independently of the government institutions and a few foundations. We're going to concentrate on the American research effort, and we'll reduce the four categories to two: the first is the government, primarily in the form of the NIH and the university labs it funds; the second is the biopharmaceutical industry.

If, as we've claimed, the 10/90 gap represents a departure from well-ordered science, then while we may view scientists as having been insufficiently vocal in protesting, the root of the trouble seems to be the research priorities set by the NIH and the biopharmaceutical companies. We'll deal with the two sources of funding separately, beginning with the case of publicly funded science.

As noted in Section II, the priorities of the NIH budget are badly misaligned with the global burden of disease. In light of the promise of technological solutions to problems that cause massive human suffering—such as pathogen sequencing, with consequent attempts to construct vaccines—NIH administrators have a moral obligation to do what they can, within the budgetary constraints imposed, to modify the current grossly skewed distribution; since the constraints make it impossible even to approximate a distribution that would make research on a disease comparable to the toll exacted by that disease, they have the broader responsibility to protest the legislative guidelines. By the same token, legislators ought to repeal the existing constraints in ways that would make it possible to move much closer to well-ordered science. Citizens have the obligation not only to write their individual checks, but also to support these legislative changes.

How might these claims about moral responsibility be evaded? If we are right in claiming that the principal source of resistance to the original arguments enjoining individual contributions to disease relief lies in the sense that such contributions are futile, then the crucial issue is whether our revised moral imperative faces a similar skeptical response. Skeptics can certainly point out that changing the research agenda isn't guaranteed to bring success, either in addressing any particular disease or in creating a climate in which public health interventions would be welcomed. But, of course, we typically lack guarantees when we invest in biomedical research, and, as we noted in Section II, there's good reason to anticipate results from an ambitious vaccine development project. If the calculated risk of investing in research pays off, the scale of the potential reward is inspiring. For example, AIDS, tuberculosis, and malaria are each single causes of a million or more deaths a year, along with considerable related suffering. If we can develop and distribute an effective vaccine for just one of these diseases, the gain to human well-being is hard to express adequately. Since our hope here is to eradicate or completely control the disease, the skeptics' worry that we're committed to an indefinite sequence of draining contributions is no longer germane. Since the costs of such vaccine research are low on a global scale, and returns to world security and the world economy great, we need not worry that it will drive us toward the Bleak World or make us do poorly by our commitments. Continued AIDS vaccine research and credible attempts at malaria and tuberculosis vaccines are the least futile of enterprises.

Suppose, however, that serious investments in research on the diseases that afflict poor nations don't succeed. To what extent should we pour more funds into the effort? We think that this question is hard to answer responsibly in advance. Judgments of the promise of research strategies are rightly revised as investigators discover new things, and it might turn out that the promise we have envisaged proves unreal. Our judgment is that *given the prospects as they now appear*, a commitment to a distribution of research that assigns each disease its fair share is morally required. That, by itself, doesn't fix the level of total funding for biomedical research. We are inclined to believe that affluent nations (in particular the United States) could afford to increase the research budget so that such diseases as cancer were still funded at approximately their current levels (although this would depend on serious exploration of the likely returns from investment at the margin), and the entire research budget were constructed by indexing to these amounts by applying the fair share principle.[52] We note also that a public commitment to this investment would be a clear declaration of concern for the suffering of people in poor countries, and that undertaking the commitment might well create an atmosphere of trust that would allow the exportation of first world public health infrastructure, even if the technological solutions were less frequent than we believe likely.[53]

As we noted in Section II, the funding for science that comes out of biopharmaceutical business roughly matches that provided by government, and we'll turn now to the question of whether pharmaceutical companies have any obligations to change their ways. There's an obvious defense of Big Pharma—its business, after all, is business. The demands of the free market make it impossible for companies to devote more than token resources to unprofitable projects. Imagine that a company were to seize the moral high ground, turning a significant proportion of its research resources toward vaccines for tropical disease. Profits would fall, investors would go elsewhere, and the decline might be so rapid that

52. For a more detailed account of how levels of funding might be set under well-ordered science, see Kitcher, *Science, Truth, and Democracy*, ch. 10. We note that the level we envisage here seems not to be vulnerable to the skeptical concerns about the sacrifice of many things that are valued in the affluent world.

53. Given our account of the roles of citizens, of legislators, and of NIH administrators, it's relatively easy to see that any worries about the potential collective action problem are forestalled; in effect, we're envisaging a classical solution to the difficulties of coordination—to wit, the use of government as a coordinating mechanism.

the research would never be completed. Plainly the market constrains action.

Yet the most obvious point to make about the pressures against developing vaccines for understudied diseases is that we needn't take the forces of the market as an unmodifiable given. Even the most devout free marketer should recognize the legitimacy of questions as to whether a given domain is best organized by subjecting it to the organization of the market; it's worth bearing in mind that Adam Smith thought it important to *argue* that international trade should be governed by the market, and that he believed that there were preconditions for the operation of free markets that were best provided in other ways.[54] If half the funding for scientific research is allocated by a process that muffles (or silences) the major concerns of 90 percent of human beings, and if those concerns are not being adequately addressed, then there's a prima facie case for thinking that the market organization of biopharmaceutical research ought to give way to a different system. One obvious possibility is for government to provide incentives to companies that invest in developing drugs for underresearched diseases, perhaps by allowing those companies tax relief. To defend the status quo one would need to show that these, and other ways of trying to generate a less skewed distribution of research effort, involve sufficiently large costs in efficiency.[55] Until the argument has been given, there's a moral obligation to modify the agenda of biopharmaceutical research to provide much greater funding for vaccine projects (and similar programs aimed at reducing the disease burden on the poor).

The line of argument we have broached here leads into more general issues about the role of the market in issues of health and health care. There are serious questions about whether we should honor the alleged efficiency of the free market when it leads to skewed (often grossly skewed) distributions in the availability of health insurance and in the relative density of doctors, clinics, and hospitals in different places, as well as to inflated costs for drugs and other forms of treatment. We shall

54. See *Wealth of Nations*, books 4 and 5. The cluster of issues about the proper sphere of governmental provision continues to occupy political economy after Smith; see, for example, J. S. Mill's *Principles of Political Economy*.

55. One intriguing possibility, suggested to us by the Editors of *Philosophy & Public Affairs*, is that scientists form nonprofit organizations in which to pursue biomedical research. This seems to us an option well worth exploring.

rest content with adding the possibility that pharmaceutical companies might be required to devote a portion of their research investment to alleviating the global disease burden to a family of proposals that favor tempering the relentless pursuit of efficiency: proposals for universal health care coverage, for a more equitable distribution of medical access, and for the limitation or abolition of patents. Detailed examination of how the current practices of pharmaceutical companies could be modified to accord better with the obligations we have been highlighting must await a future occasion.

Our current biomedical science is a very long way from being well-ordered. Collectively, we have the obligation to do much better. We've argued that the obligation takes different forms, depending on the role (or roles) that a person plays in the direction of scientific research. Legislators have the responsibility to modify the conditions under which biopharmaceutical companies and NIH administrators allocate research resources; drug company executives and administrators should do whatever legislators cannot to correct the imbalances in the distribution of research effort; individual scientists should reorient their research, where they can, in ways that bear on the diseases that afflict the poor; and ordinary citizens ought to support all these efforts—even if it means that some of our personal interests are thereby sacrificed.