Responsible Biology

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Responsible conduct in science is more than simply a matter of following everyday ethical imperatives—not misreporting what actually happened in the lab, dealing honestly with colleagues, and so forth. Scientific responsibility arises because scientists play a special role, and that role brings obligations. In this article I maintain that scientists have an obligation to reflect on the ends of scientific research; that scientists should work for the public good, directing their efforts toward an ideal of well-ordered science; and that the ideal of well-ordered science should be understood in a global and democratic fashion.

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Talk of responsibility in science in general, and in biology in particular, can induce unease. It is natural to think that responsible conduct in science is simply a matter of following everyday ethical imperatives, such as dealing honestly with colleagues, not misreporting what actually happened in the lab, and so forth. Efforts to demand more are likely to descend into sermonizing—or worse, mischievous meddling by ignorant outsiders.

In this article, I want to oppose this natural reaction to discussion of scientific responsibility. Specifically, I shall defend three theses: (1) Scientists have an obligation, individually and collectively, to reflect on the ends—not just on the means—of scientific research; (2) scientists should conceive of themselves as artisans working for the public good, whose efforts are directed toward an ideal of well-ordered science; and (3) this ideal of well-ordered science should be understood in a global and democratic fashion.

I start from the view that, in becoming a scientist, a person takes on a new role, and that role brings with it special obligations. What might that role be? Conceptions of the functions and goals of the sciences have been variable over time. Shortly after the birth of the Royal Society in 1660, its patron, Charles II, is reported to have laughed at the thought that the "gentlemen" were devoting their efforts to weighing the air. A decade or so later, playwright Thomas Shadwell portrayed a "virtuoso" as a dotty eccentric, and the name he gave his protagonist, "Sir Nicholas Gimcrack," introduced a new word into the language.

By the late 19th century, the power of institutionalized science to uncover all sorts of facets of nature—some of them quite disturbing to everyday preconceptions—had convinced most thoughtful people that a different image of the goals and functions of the sciences was needed. Francis Galton, Darwin's cousin, introduced the idea of scientists as constituting a secular priesthood, united by devotion to the discovery of truth (Galton 1875). That image has been extraordinarily influential, leading many to view the sciences as pursuing the highest human goal, that of understanding our world. Incidentally, that pursuit may bring many and varied practical benefits, but the mundane satisfaction of ephemeral human needs is insignificant in comparison with the transcendent goal of finding the truth.

I think this image has misled us and blinded us to the real responsibilities of sciences and scientists. But I want to start by clearing out of the way a possible misunderstanding.

A modest defense of truth

In discussions many contemporary social scientists and humanists react to the quasi-religious celebration of science by insisting on the evils of scientific hegemony. It is a staple of some responses to science to oppose the power with which science has been invested ("to emancipate subjugated knowledges from...subjection," as Michel Foucault puts it [1980]), and the usual way of putting the point is to emphasize the unintelligibility of the notion of "truth," the multiplicity of "truths," or the view that science has no monopoly on "truth." For the most part, these responses are profoundly muddled.

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There is nothing wrong with claiming that the natural sciences sometimes put forward representations—in statements, graphs, equations, maps, diagrams, and so forth—that correspond to the world in various respects and to various degrees. It is quite inexplicable how contemporary biologists could do all the extraordinary technical things they manage to do unless their claims about the structures of genetic material and their maps of the arrangements of that material in particular organisms were roughly accurate (Kitcher 2001). Nor should we be beguiled by the thought that to claim that our views about heredity are approximately correct while those of some group of pastoralists in the developing world are not is to engage in a kind of intellectual chauvinism. We do know some things that others do not, not because we are more intelligent but simply because we have spent more effort in investigating the pertinent issues and have institutionalized ways for coordinating our inquiries into them; for other topics, the situation might well be reversed.

So I want to reject the standard ways of debunking science. But I also want to sympathize with the debunkers' concern about the image from Galton that lives on in the rhetoric of some of contemporary science's most prominent champions—people like E. O. Wilson (1998) and Richard Dawkins (1998). The issues are not metaphysical or epistemological, focused on truth and knowledge. They are ethical, social, and political. The question we ought to be asking is whether Galton got the function of science right, whether he correctly identified the scientist's role, and whether his account disguises obligations that are not being met. I am going to argue that the image is wrong and that we need to revise it to understand the character of responsible scientific (or biological) practice.

The clash of obligations

I will start with a relatively obvious point, one that shows that however important the disclosure of truth may be, it cannot override all other concerns. Nowadays, research scientists have come to terms with the fact that the means through which they seek information are limited by ordinary ethical concerns. There are things you cannot do to subjects in the interests of finding out what you want to know, even if there are no other ways of inquiring into the pertinent questions. The public outcry over the infamous Tuskegee experiment shows that the value of finding the truth does not override the rights of human beings, and the point is made even more graphically by the practices of the Nazi doctors. These cases show that we are not so committed to the value of truth that we are prepared to pursue it *über alles*. Applying contemporary biology to human problems would be facilitated if we could explore aspects of the nature–nurture problem by breeding special lines of children and rearing them in carefully controlled environments, but nobody is likely to propose seriously that the prospect of progress in human biology justifies such experiments.

So the image of scientists as secular priests has to be refined a bit, because the priests are not allowed to conduct human sacrifices. We are left, then, with the thought that the sciences have a wonderfully noble end, but that, because this end is one among many, there are limits on the means we can take to achieve it. Most ethical discussions of science tacitly start from this modified image, wondering, in effect, how to obtain the marvelous end in ways that are responsive to the constraints imposed by our other aims: Thus arise the relatively small-scale disputes about the uses of animals in scientific research, the need for openness in sharing information, and so on.

But I think we need to think more carefully about the end itself. What exactly is it? And why is it so valuable? When we think in this way, we will start to see how there can be other responsibilities, other ways in which scientists, individually or collectively, can fail to recognize their proper obligations.

Significant truth

But didn't I—or Galton—already explain what the end is? Isn't it truth? I agree that truth is part of the story, but it cannot be the whole story. The truths about nature, even about a tiny part of nature (this room for this hour), are too vast and various ever to be completely cataloged by human effort. The truth, the whole truth, and nothing but the truth cannot be a human aim.

Of course, most truths about nature are completely uninteresting, and to dedicate oneself to uncovering them would arouse suspicions of obsession (or worse). If we want to explain what the sciences aim at, then we will have to talk about "significant truth." But what exactly does this mean? How do we give content to the notion of significance?

Many reflective scientists and philosophers think that this is easy. The significant truths are the general laws of nature—God's rule book for the universe, as it were. Whether or not this is detheologized, it is not satisfactory, for we have no reason for thinking that there are general laws that cover all the phenomena in which we are naturally interested. This is especially evident in the life and earth sciences, where truths that are hailed as significant rarely seem to have the universality of laws. (Think of the uses of modifiers such as "normally" and "typically" in stating biological conclusions.) Researchers like generality where they can get it, but sometimes what is sought is a very specific answer to a very specific question (Levins 1968, Kitcher 2001).

I recommend a different approach. Truths are significant for a community of inquirers at a particular time, just in case those truths can provide relief from the kinds of ignorance that are properly of concern at that time. You will notice that my formulation contains a distinctly normative term—"properly"—and I will have to explain it shortly. But I hope you will see that it is much closer to everyday scientific practice than most of the more high-flown views of the aims of the sciences. My account suggests that communities of inquirers have an evolving agenda, one that develops as answers to questions previously posed suggest new lines of inquiry and as changes in the natural and social environments generate new problems.

Let me be clear that the pragmatism of that account is not a crude pragmatism that sees value only in practical payoffs. Often, to be sure, we want relief from ignorance because our inability to know interferes with our attainment of our practical goals: We want greater knowledge to be able to address the diseases that afflict us, to make the atmosphere cleaner, to improve the supply of food in places where famines recur, and so on. But there are other inquiries that we quite legitimately undertake because we hope to satisfy our human curiosity. To cite just one example, many of our ancestors were impressed by the uniformity in development from seed to plant, and contemporary molecular developmental genetics inherits from their vague, large questions much more precise forms of inquiry that seek to render the details of development comprehensible. A science devoted solely to answering questions of immediate practical concern would be shortsighted—for we know that long-term practical successes sometimes come from pursuing quite theoretical and "useless" lines of research—but, quite independent of any future practical benefits, it would be a sadly diminished science. Conversely, putting all our efforts into satisfying disinterested curiosity, with no thought for satisfying urgent human needs, would be a form of self-indulgence verging on cruel neglect.

In thinking about the merits of the institutions of the sciences, we ought to consider ways in which the ideal might be achieved, but it is quite possible that the best we can do is to proceed toward the ideal in an indirect fashion.

I hope it is becoming clear how my suggestion that we revise Galton's image of science starts to uncover ethical concerns. For, once we see that significant truth is a matter of what matters to particular communities at particular stages, we have to consider what should matter. There are ethical questions not just about means, but about ends as well.

Well-ordered science

I said that the significant truths are those that would bring relief from ignorance about the questions that properly concern the community. But what counts as the "community"? And how do we specify its members' proper concern?

One simple thought is that the pertinent community is one of qualified scientists, and the questions that "properly" concern them are the ones that, perhaps after reflection, they find interesting. This proposal is vague: Who counts as a scientist? What guides scientists' "reflections"? What happens when they disagree? But my principal objection to it is that this proposal is elitist and antidemocratic, smacking far too much of the Galtonian idea of the secular priesthood. Why should this particular group decide what types of ignorance desperately need to be remedied?

An obvious answer: The scientists know what the genuine possibilities for inquiry are, what lines of investigation are most promising. This is correct, at least insofar as it points to a difference between the scientific community and outsiders. But it does not mean that scientists have a keen awareness of all the needs that are urgent to members of a broader population or that, in their responses to human needs, they represent the interests of that broader group. There is a place for expertise, but it needs to be tempered by representation of viewpoints that are equally expert about different things.

I suggest that we leave the Galtonian image entirely behind us. Instead of thinking of scientists as seers whose vision can never be questioned by those outside the scientific community, I propose that we conceive of scientists as artisans working for a public good. Science matters. And the ways in which it matters concern all of us, inside or outside the community of experts.

Here is a different answer to the question of how to understand significant truth: The inquiries we properly pursue are those that accord with the best hunches about how to promote the collective good. My talk of hunches here is deliberately designed to make explicit the point that we often cannot predict. But in science, as in other uncertain ventures that we undertake, we ought to do the best we can, given the highly fallible judgments we can make.

But what, you may wonder, is this collective good? How is that to be understood? I answer by offering an ideal of "well-ordered science." Science is well-ordered when the inquiries it pursues are those that accord with the agenda that would have been set by a group of discussants fully informed of the scientific opportunities, fully informed of one another's needs, and dedicated to doing the best they can to accommodate the needs of all (Kitcher 2001). It is understood that this group ought to include representatives of all viewpoints, not only within the scientific community, not only in the society that supports scientific research, but in the entire human population.

This is an ideal. Because it is so often misunderstood, I want to be clear on two points. First, I am not suggesting that democracy reign in the decision about what counts as "correct," which was why I was so concerned at the outset of this article to provide a modest defense of truth. Rather, my democratic ideal concerns the lines of inquiry that ought to be pursued. Second, I am not saying that the actual practice of the sciences should involve cumbersome discussions of the type that figure in the ideal. Ideals are one thing, methods for achieving them quite another. In thinking about the merits of the institutions of the sciences, we ought to consider ways in which the ideal might be achieved, but it is quite possible that the best we can do is to proceed toward the ideal in an indirect fashion.

I note further that the ideal discussion should be sensitive to the extent to which the course of science is unpredictable. To specify the ideal carries no implication that scientific research can be completely and accurately planned. But, as I have said, we can recognize our uncertainty and still set our course in a particular direction. (We do that all the time in planning our lives.) Moreover, we can sometimes find

ourselves deferring to the hunches of people with the characteristics of those whose aspirations have frequently proved fruitful in the past.

Let me sum up: We need to free ourselves from the Galtonian picture, recognizing—as Charles II and Thomas Shadwell did not—that scientific research is consequential for everyone. In an ideal world, scientific inquiry would be conducted as well-ordered science, seeking relief from ignorance in the best foreseeable way to solve human problems, not for this or that subgroup but for all of us. With this ideal in place, we can now return to my main topic, scientific responsibility.

Responsible science

If you take on the role of a parent, you have the obligation to do what is needed to fulfill that role, to realize the goals of parenting. The same goes for being a scientist. Collectively, the scientific community has the obligation to promote well-ordered science. Individually, scientists have the obligation to do what they can to move their community closer to the ideal of well-ordered science.

What does this mean? It can mean all sorts of things, things to which scientists are not well accustomed: perhaps asking what kinds of research one ought to pursue or reflecting on the likely consequences of one's current research; perhaps refusing certain kinds of opportunities for funding, because they would lead one away from the kinds of inquiry that a well-ordered science would pursue; perhaps campaigning for different research priorities or lobbying legislators for attention to problems that are being neglected.

The obligations arise in two different ways. When a reflective scientist asks, "What should I be doing?" the question may take for granted an existing set of constraints. Depending on the way the society to which the scientist belongs has set priorities of research, the issue may concern how best to work within the range of possible options. Yet, especially when the upshot of reflection is that the available choices are too narrow, the scientist may consider how the constrictive framework might be amended. So understood, the first-person pronoun becomes plural: "What kinds of science should we be doing?" and the scientist begins to ponder ways in which the ideal of well-ordered science might be realized more fully in the ambient society. Out of that may come a form of scientific activism.

It is easy to respond to these thoughts by declaring that this is not one's job or even that one was not trained for work like this. If one is part of an institution that is not fulfilling its function (or is not fulfilling it well), it is possible to resign oneself to going along with things as they are, on the grounds that the rules are set from on high. Perhaps scientists should simply stop with the first kind of question, asking how they can best work within the constraints as they are imposed by the pertinent authorities, without taking the plunge into activism. But resignation is not always morally defensible. Sometimes there is an obligation to try to change the institution one is part of, and—particularly if the cause seems a very important one—when the deviations from proper functioning are greater, the requirement to lobby for the changes is all the more urgent. So I do not want to deny that the obligations resulting from the ideal of well-ordered science may be heavy.

A different response would be to complain that this is all very abstract—and indeed it is. Often, an outsider's attempt to be more concrete will rightly be seen as dangerous meddling; lines of research will be dismissed as worthless and wasteful because their potential to deliver genuine benefits is unappreciated. Conversely, scientists who have thoroughly immersed themselves in a particular research program may come to overestimate its importance, with their view of the world resembling that of a Manhattanite in cartoonist Saul Steinberg's famous "View of the World from 9th Avenue"—Manhattan looms large while the rest of the world fades into the background. To work out genuine responsibilities is not a matter for external moralizers or for (possibly myopic) specialists. We need genuine dialogue, dialogue among scientists to delineate the real promise of lines of inquiry in very different domains, as well as between the scientific community and an informed group of people who represent widely divergent constituencies. At the end of that dialogue, it might even emerge that little change is required, that everything is going well (or as well as we could hope for).

But there are exceptional cases, instances in which the apparent discrepancy between our actual practice and the ideal of well-ordered science is very clear and where an outsider can draw the main contours of the problem. I think (but I am not completely confident) that ecological research into the consequences of modifications of tropical environments is one such example. I am much more confident about another example: the mismatch between our actual research practices and the global burden of disease.

Responsible biology: The case of global health

About 60 million people die each year, 40 million of them from disease. Some of these deaths are attributable to old age, but the vast majority are not. Many deaths therefore appear to be premature.

Some of these premature deaths could be prevented: In many cases, we know how to tackle the relevant diseases. However, we don't know how to export the methods to the contexts in which many sufferers die. In other cases, we have not yet found any method to combat the disease in question. In either instance, research would be beneficial, either to discover a method of treatment that could be exported or to find an approach where we currently have none.

Let us look at some figures on the annual burden of death. These are rough and conservative:

- Malaria: 1 million
- Tuberculosis: 2 million
- AIDS: 3 million
- Respiratory infections: 4 million
- Diarrhea and other water-borne disease: 2 million

In all these cases, the diseases occur disproportionately in poor countries. In some instances, the affluent nations have ways of preventing the diseases (clean water, public health infrastructure in general) that cannot easily be set up in the poorer nations. Public health improvements are a long-term goal, for there may be severe social and political obstacles to building the pertinent infrastructure; thus, in the short term, we may need vaccines, antibiotics, and other measures. (An ancillary benefit is that a commitment to investing in such measures might help create a climate of trust in which possibilities for public health opened up.)

Let us now look at funding. About $70 billion is spent annually on research on disease. Only about $3 billion of that sum comes from the nonaffluent world. I will define a disease's fair share as that portion of the total research funding ($70 billion) that corresponds to the fraction of the total disease burden (in terms of numbers of death) attributable to that disease. (Of course, this relatively crude measure of the disease burden fails to take nonlethal diseases into account, but it does provide results for the infectious diseases on which I will focus.) Using this simple approach, the fair share for malaria—which kills 1 million people a year, including a high proportion of children—would be $1.75 billion; in 1995, the actual figure spent on malarial research was about $85 million. Tuberculosis is responsible for 2 million deaths each year, and so its fair share would be $3.5 billion; at the high end of estimates, it received $33 million in 1990. (So, if malaria sufferers historically receive only a twentieth of the scientific attention their plight merits, victims of tuberculosis may only get a hundredth.) The single largest infectious cause of death is respiratory infection, which kills 4 million people a year, and the third largest cause is diarrheal 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 each. This situation is familiarly known as the "10/90 gap": the idea that only 10 percent of the world's resources are directed toward the diseases that afflict 90 percent of the population. (For discussion of the figures presented here, see Flory and Kitcher 2004).

What explains the 10/90 gap? The obvious fact is that affluent countries spend money on diseases that afflict their citizens; less obvious is that money is given to projects of relatively trivial concern ("lifestyle" drugs).

The existence of this gap imposes on us a multifaceted set of obligations. We ought to respond to the plight of people who are now afflicted—that is, all of us who are relatively affluent ought to send money to cover the costs of medicines without which poor people die. We ought to take steps to promote the development of an infrastructure of public health in developing countries, for without such an infrastructure, the citizens of poor nations will be in danger of recurrent infections. And we ought to change the scientific research agenda to accord with the fair share principle. Not only will this be the only way of responding to some diseases, it may also serve as a stopgap measure if standard public health approaches are simply not exportable to the conditions in which infectious diseases are rampant.

I will consider two obvious objections. First, you might suppose that the actual distribution of resources for research is justified by the promise of new techniques in biomedicine. Because it is reasonable to expect so much more from the kinds of research we actually pursue, we are justified in slighting the diseases from which the distant millions die. But this is not right. The successes from our current expenditures on the Human Genome Project and descendant projects are unpredictable (Kitcher 1996). For example, although the molecular basis of sickle cell anemia has been known for half a century, no substantial advances in treatment have been made, whereas treatment of cystic fibrosis has met with partial successes. Molecular medicine offers us a lot of tickets in a lot of lotteries, but we cannot tell in advance which ones will yield prizes or how big the returns will be.

In the first place, it is highly probable that we are rich enough to extend our research budget—or even to divert marginal funds from some currently well-supported lines of inquiry—in ways that would accord with distribution according to fair shares.

On the other hand, pathogen sequencing projects might lead to all kinds of new vaccines. There is no guarantee that we will be lucky in finding coat proteins or in introducing them into benign vectors that can stimulate the immune system, but this is surely worth exploring on a broad scale. At present, vaccines for malaria are a hope—a distant hope, to be sure—but tuberculosis vaccines are not even on the horizon. Systematic efforts to use current sequencing techniques as the start of a broad vaccine research program seem just as promising as the applications of the Human Genome Project, which did get funded. Hence you cannot justify the departure from the fair share principle by claiming that the course of inquiry we pursue is uniquely promising.

The second objection questions the moral basis of the allocation of research resources by fair shares. A natural response would be to declare that the resources we have did not drop from the skies to attend our decisions about how to use them. They were garnered by the efforts of citizens of affluent societies, and it is entirely proper to deploy them in ways that respond to those citizens' perceived needs. After all, the funds that flow to public scientific research come from taxes, and the scientists who carry out that research were (typically) educated within the affluent societies in which they continue to work. So, the argument goes, the diseases of the affluent world quite properly receive more than their fair share.

I do not believe in any such entitlement. In the first place, it is highly probable that we are rich enough to extend our research budget—or even to divert marginal funds from some

currently well-supported lines of inquiry—in ways that would accord with distribution according to fair shares. In the context of similar debates about responsibilities to address issues of global hunger, Peter Singer famously argued that when desperate people can be helped at small cost to those who are asked to give, there is a responsibility to supply the aid (Singer 1972; see also Unger 1996).

Second, what is of concern here is not so much a matter of maintaining something that we have but of considering ways to improve our situation. Plainly, there is an enormous difference in actual quality of life between the affluent countries and those regions of the world in which poverty, hunger, and disease are endemic. As we think about how science can be used to enhance human well-being, it is surely wrong—if not obscene—to think that the resources we commit entitle us to focus just on lines of research that will make the affluent better off—that is, to increase the gap in welfare between the rich and the poor.

Third, it may be comforting to believe that we deserve the benefits that flow from scientific research because we have worked so hard to make such research possible, but to do so is to overlook all the accidents of history—not to mention the inequities and injustices—that have contributed to the current, highly skewed distribution of resources around our planet. Our ability to carry out investigations that can bring practical benefits, as well as important growth in disinterested understanding of nature, is in significant part the result of luck. Consciousness of our good fortune should inspire at least glimmers of compassion for those who lack the resources to invest in scientific inquiries that might bear on their own urgent needs, and so lead us to acquiesce in division according to fair shares.

Each of these points could be developed at far greater length. But I hope these brief remarks will suffice to show how I would resist the claim that affluent societies are entitled to do the kinds of science that matter most to them (or at least to their most affluent and powerful members).

Conclusion

In the distribution of research resources, we find a clear deviation from well-ordered science, and that deviation obligates scientists and nonscientists alike to modify directions of research and to campaign for redistribution of research funds. I offer this as one example of what responsible biology demands. But more generally, I urge biologists to ponder the ends to which their research projects contribute and to ask whether their current programs represent their most effective possible contributions to addressing the urgent problems that confront us all.

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