Chapter 5

WELL-ORDERED SCIENCE

17. SCIENTIFIC SIGNIFICANCE

Public systems of knowledge expect investigators to contribute new statements to the public depository. They expect the contributions to be worth having. It is evident that many things we might come to know—*easily* come to know—would be poor targets for investigation. My speculations about Paleolithic systems of public knowledge supposed that detailed reports of the vistas from places in the surrounding environment would not be welcome news. Similar points apply today. There are enormous numbers of ways of describing the parts of the world we visit and vast numbers of true descriptions that could be supplied—about temperature, color, spatial relations, the number of objects of specific types—which only the monomaniacal would find interesting. Nor is truth always our concern. Sometimes an approximation, a statement recognized as false but "true enough" will serve our purposes (Elgin 2004).[1]

If contemporary Science, and the public system of knowledge in which it is embedded, is to serve the purposes of citizens of a democratic society, what kinds of investigation should be pursued? Although it is easy to conceive the public depository as a collection of statements, many kinds of investigation aim at nonlinguistic products: researchers seek new molecules, new organisms, new drugs, new instruments, new techniques. Think of a *problem for investigation* as arising when some entity of a specified type is sought. Problems worth pursuing can be labeled as *significant*. Those problems are *adequately solved* when an item is produced that is close enough to the type sought to serve the purposes that confer significance on the problem. If the problem is to answer a question, an adequate solution is a statement "true enough" to enable those who have it to achieve whatever ends made the question significant. If the problem is to produce a new vaccine, an adequate solution is one

providing acceptable protection against the pertinent disease. If the problem is to develop a new technique, an adequate solution is one allowing people to proceed sufficiently successfully in the contexts of intended use.

The first task of this chapter is to explain a notion of scientific significance that will fit with democratic values. It should be evident that the notion of significance is value-laden, and the explanation I shall develop will flow directly from the approach to values offered in chapter 2. Scientific significance accrues to those problems that would be singled out under a condition of *well-ordered science*: science is well ordered when its specification of the problems to be pursued would be endorsed by an ideal conversation, embodying all human points of view, under conditions of mutual engagement.[2] This understanding of scientific significance will require development and defense. Before proceeding to the explanations, however, it is worth being fully explicit about why popular treatments of significance, usually under the rubric of the goals (or ends) of Science, are deficient.

Scientists and philosophers often declare that the aim of the sciences is to provide us with a complete true story of our world. Plainly that cannot be right. There is some large infinity of languages people might adopt for talking about nature: think of the myriad ways in which the boundaries of objects can be drawn and in which objects can be grouped together. For each of these languages, there is a large infinity of true statements about the cosmos. Given these elementary facts, it is not obvious that the notion of the "whole truth" is coherent, and, even if it is, it is surely beyond human formulation or comprehension. Moreover, well-established parts of physics inform us that some parts of the universe are completely inaccessible to us: regions outside our light cone are a prime example. These losses are not serious, for virtually all of the "whole truth" lacks any interest for anybody (think, for example, of the large infinity of truths about the areas of triangles whose vertices are three arbitrarily chosen objects). Supposing that Science aims at the complete true story of the world is as misguided as the suggestion that geography seeks to draw a universal map, one revealing every feature of the globe.[3]

Behind the casual proposal that Science aims at the "whole truth" is a more plausible idea. Thinking of the sciences as primarily in the business of providing theoretical understanding (a legacy of ideas about the value of knowledge that have been influential since Plato and Aristotle, ideas that clearly moved the "gentlemen" who formed the scientific societies of the seventeenth century), scholars envisage an objective agenda, set for us by nature,

to which human beings, as cognitive agents, should respond.[4] Unfortunately, nature's agenda setting is a metaphor, one that dissolves under scrutiny. The only plausible way to give it substance is to substitute a different metaphor, suggesting that Science seeks a full inventory of the "laws of nature."

Scientific inquiry does sometimes look for generalizations—and with good reason. Knowing something general can be valuable, for you may be able to use the generalization to answer many significant specific questions: Newton's second law can be applied to different dynamical systems, the generalizations embodied in the genetic code can be used to make predictions about lots of amino acid sequences, and so forth. Yet this banal point leaves plenty of room for variant ideas about the aims of the sciences. Are generalizations the *only* significant statements? Surely not. We sometimes take questions about specific things—earthquake zones, particular disease vectors—to be prime targets for scientific research. What kinds of generalizations count as *laws*? This is a perennially difficult philosophical question, and none of the (variously problematic) attempts to answer it explains why the laws of nature might be specially worth knowing.

Here again, thinking about Science and its goals is tainted by the residues of conceptions people have long discarded. The predilection for talking about "laws" of nature (which sits oddly with the haphazard ways in which particular scientific contributions are labeled as "laws," "rules," "principles," and "theories") was entirely explicable at a time when investigators thought in explicitly theological terms, seeing deep generalizations about natural phenomena as expressing the decrees of a law *giver*. Copernicus, Kepler, Descartes, Boyle, and Newton imagined their research would reconstruct part of the divine rulebook used by the Creator in setting up the show and that the reconstruction would enable people to "think God's thoughts after him" (Burtt 1932). *They* could have answered any challenge to explain why finding the laws is a worthwhile goal, but, when the theology drops away, we are left with the idea that our universe operates *as if* it has certain fundamental rules. Why should those rules be paradigms of significance?

The best hope for identifying goals for Science that will provide an "objective agenda" and thus steer clear of worrisome value-judgments is to try to build on the banal point that generalization is typically useful. Suppose Euclid's brilliant strategy for geometry works for Science as a whole. There is some manageable collection of fundamental laws, from which all generalizations about nature flow: the fundamental laws are the first principles of a

"theory of everything." Whatever particular explanations or predictions are needed can be generated from this theory by conjoining the principles of the grand axiomatic system to statements about specific conditions. Science supplies an all-purpose instrument, available to anyone to understand or foresee whatever things interest him. Judgments about what matters, what is significant, can thus be left to the idiosyncratic interests of the users of a universally applicable tool.

Although this picture of the unity of Science has attracted many adherents, it cannot be sustained. For the presupposition of a series of reductions, available "in principle," breaks down once the likeliest candidates are examined closely. Classical genetics and molecular biology are well-developed sciences, and it is plain that the latter has been immensely valuable in refining views about hereditary phenomena. Despite the insights provided by chemical understanding of biologically important molecules, it is false to suppose that *every* significant generalization can be derived and explained within molecular biology. Consider, for example, the principle that genes on different chromosomes assort independently at meiosis. This cannot be derived from principles of molecular biology, since there is no way of singling out, within the language of molecular biology, all and only those entities that count as genes (or as chromosomes—or what processes count as meiotic divisions). Further, even if this obstacle were overcome, the *explanation* of independent assortment focuses on the general structure of the process of meiotic division: at meiosis, homologous chromosomes are paired, and, after exchange of genetic material between homologues, one member of each pair is passed on to a gamete. Because of the way the pairing and separation works, genes on different chromosomes are transmitted independently (Kitcher 1984; 1999).

The *single* axiomatic system, adequate for everything, is unavailable. Perhaps Science could settle for less—for a bundle of sciences, each coming with its fundamental laws. Less tidy than the imagined (and imaginary) theory of everything, a manageable bundle would still provide a universal instrument and would thus expel value-judgments from Science and leave them to the variant tastes of individuals. Even among the natural sciences, there is little hope this retreat will succeed: in many areas of biology and the earth and atmospheric sciences basic laws are hard to come by. If the realm of Science is extended into psychology, economics, and sociology, the prospects are dimmer still. Moreover, there is no basis for thinking that as

new areas of inquiry are developed, they will yield further small clusters of generalizations. On the basis of the sciences so far achieved, different areas of inquiry are likely to be disparate, some allowing a small set of powerful generalizations, others requiring ramified ways of treating a large range of variation among cases (Cartwright 1999).

The resolute efforts to ban value-judgments in considering the ends of the sciences have obscured the fact that, like earlier societies that have constructed systems of public knowledge, we expect inquiry to help us with *particular types of problems*. As with our predecessors, some of these problems are practical: in those domains of inquiry that bear on medicine, agriculture, and responding to the challenges of the environment, a search for some cognitive benefit—detached understanding, say—is not primary. The aim is to grow healthy crops under adverse conditions, to find ways of curing or treating a serious disease, to know in advance the path of the hurricane or the site of the earthquake. Generalizations, laws of nature are welcome to the extent we can discover them, and all the better if they enable us to deal with a wide spectrum of cases—but if we can reach our practical ends without them, that is good enough.

On the face of it, there are also sciences that do seek understanding for its own sake. Even if an understanding of the origins of our species (or of life, or of the solar system) offered no practical payoff whatsoever, many people would still view it as valuable for its own sake. How the various hominid species evolved, and how *Homo sapiens* came to be the last one standing, is something they want to know, something about which they are curious. We should no more ignore the fact that some great scientific achievements answer human curiosity than we should slight the impact scientific knowledge has on human lives. There are three simple, but misguided, suggestions about the aims of Science and thus about the proper pursuit of scientific research.

A. The aim of Science is to discover those fundamental principles that would enable us to understand nature.
B. The aim of Science is to solve practical problems.
C. The aim of Science is to solve practical problems, but, since history shows that the achievement of understanding is a means to this end, seeking fundamental principles (generalizations, laws) is an appropriate derivative goal.

There once was a system of public knowledge in which A made excellent sense, namely, the Christian conception reviewed briefly in the last chapter (§15). Devout investigators who accepted knowledge of God, of his attributes and purposes, could think of their own inquiries as disclosing the ideas realized in the Creation. To understand nature, to reveal its most fundamental modes of organization, was to decipher a second text bestowed upon us by a wise and benevolent deity. Besides the revelation of the scriptures, humanity could study the Book of Nature, enhancing our appreciation of the glory of God. The pious Robert Boyle endowed the lecture series named for him with precisely that end in view.

As I have argued, without the theological backdrop—and the associated conception of public knowledge—A has no plausibility. Even the slightest sympathy with pragmatism (in either the philosophical or the everyday sense) will recognize circumstances in which the esoteric interests of scientific specialists ought to give way to the urgent needs of people who live in poverty and squalor. By suggesting that the areas in which pure understanding is sought for its own sake stem from widely shared forms of human curiosity, I have not vulgarly thrown pure theory into the balance with applied sciences: *it was already there*. Systems of public knowledge, including the ones that treat Science as a central part, cannot avoid value-judgments about what is significant and what is not, and some of those judgments turn on weighing the competing claims of pure understanding and practical problem solving.

Precisely because I see a competition here, I cannot opt for the simplistic pragmatism that would repudiate A in favor of B or C. Focusing just on the practical (as B recommends) would often be misguided, inefficient, or unproductive, as champions of C will point out. Often the best route to potential gains down the road is to investigate quite recondite questions: Thomas Hunt Morgan's wise decision to postpone consideration of human medical genetics and concentrate on fruit flies prepared the way for the (ongoing) revolution in which molecular understandings are transforming medical practice. Nevertheless, C inherits a major error from B by failing to recognize the ways the ethical project has expanded the scope of human desires, equipping us with richer notions of what it is to live well, ideals that include, even if they should not be limited by, the attainment of understanding and the satisfaction of curiosity for its own sake.

Not only must our system of public knowledge make value-judgments

but the questions to be confronted are *hard.* They involve weighing two types of goods it is very difficult to reduce to a common measure: value accrues to answering a large question that arouses our curiosity; it is also valuable to advance human welfare. There is no escape from the balancing business, and it is not easy to see how to begin the balancing. That, of course, motivates the attempted solutions I have been criticizing, the efforts at uncovering an "objective," "neutral" agenda for Science, in which an all-purpose instrument is devised and given to individual people to use as they think fit. If that could be done, we could avoid the challenge of weighing variant values and idiosyncratic tastes.

Once we see that the challenge has to be faced, and once we recognize its form, we should appreciate the many ways in which balancing is required. If you take seriously the idea that Science is for the *human* good (not American good, not the good of intellectuals, not the good of affluent, well-educated people), you will see how it is necessary to balance the interests of very disparate groups of people. There will be issues about the schedules on which problems are to be tackled, about whether strategies offering long-term success are to be preferred or whether some issues are so urgent we cannot wait. Judgments of significance involve a multidimensional balancing act.

How, then, should we do it?

18. WELL-ORDERED SCIENCE: EXPLANATION

Many theorists who have reflected on values, or on the values realized in gaining knowledge, would answer the question directly. Some would embrace one of the positions rejected in the previous section, announcing the overriding importance of knowledge of the deity, or of theoretical contemplation, or of increasing the sum of pleasure-minus-pain across the class of sentient beings. Others would strive to give an authoritative answer, based on a scheme for placing different types of consequences on a single scale and assessing their relative weights. *Any such direct resolution is at odds with the approach to values outlined in chapter 2, an approach to be applied to the issue at hand.* According to that approach, the answer is not for any single person—not even an insightful religious teacher or a clever philosopher—to determine. Individuals can make *proposals*, but the only authority in this arena derives from a conversation. Tentative proposals about the character of

the conversation are valuable to the extent they facilitate discussion. That is the way of the ethical project.[5]

Section 7 proposed that we renew the ethical project by eliminating particular accretions it has taken on during its tens-of-thousands-year history, emulating the focus of the early stages, but scaling up to recognize that the "band" in which we live is the human species. The suggested overarching conception of the general good is a state in which all people are offered serious, and equal, opportunities for worthwhile lives, where worthwhile lives are understood in terms of free choice of projects, some of which involve interactions with others. Decisions about norms and values should accord with those that would be reached in a panhuman conversation under conditions of mutual engagement.

Chapter 3 suggested that these proposals elaborate a deep democratic ideal, one that views democracy as important because of its promotion of varieties of freedom, distributed equally across humankind. One obvious way to approach some of the issues of balancing that underlie judgments of significance is to appeal to democratic principles. You start from the picture of a population with different aspirations and interests, and suppose scientific significance involves integrating these diverse elements, producing some kind of collective good. If you are tempted by the thin view of democracy (which chapter 3 attempted to transcend), seeing democracy as residing in the possibility of free elections and voting, you will suppose the apt standard for scientific significance is majority vote: each member of the population thinks about the investigations she would like to see go forward, and everybody then casts a vote.[6] Many people, especially scientists, worry that this would be an extremely bad procedure for arriving at judgments of significance. They point out, quite reasonably, that ascriptions of significance achieved in this way would favor short-term practical inquiries over research of long-term significance, that the emergent research agenda would be myopic and probably unfruitful. From the first discussions of the public role in decisions about what kinds of science should be done, scientists have taken steps to avoid confinement by public control. Vannevar Bush's masterstroke was to argue for a framework of decision making that ensured the reins could never be pulled tight—the attributions of significance were to be the province of the experts.

From the perspectives of chapters 2 and 3, both polar positions—the appeal to the thin ("voting") conception of democracy and the expert reaction

to it—are completely misguided. Built in to the ideal of discussion under mutual engagement are cognitive and affective constraints: instead of myopic voters choosing in ignorance of the possibilities, and of the consequences for others, completely absorbed in their own self-directed wishes, the ideal conversationalists are to have a wide understanding of the various lines of research, what they might accomplish, how various findings would affect others, how those others adjust their starting preferences, and the conversationalists are dedicated to promoting the wishes other participants eventually form. As we shall see shortly, there is no reason to suppose that judgments of significance achieved in this way should cause scientific shudders.

The trouble with putting judgments of significance to majority vote is not the *democracy* but the *vulgarity* of the view of democracy it embodies. The reaction—to place decisions about significance in the hands of experts—might well be superior to the tyranny of ignorance that vulgar democracy would likely produce, but it arrogates to the expert community a judgment about values it is unqualified to make. It is another distortion of the ethical project, another mode of undermining the authority of a conversation among affected parties and replacing it with the *illegitimate* authority of a group. Anyone tempted to acquiesce in that authority should seriously consider the virtues of Plato's *kallipolis*, in which decisions are similarly left to the judgment of the wise.

These strong charges rest on recognizing how the conception of an ideal discussion under mutual engagement offers a better standard for scientific significance. Familiar features of everyday decision making provide motivation. Most adult members of large societies face the general problem of balancing one sort of activity against another, apportioning time to a variety of worthwhile projects. Although we sometimes give weight to one way of spending time on the grounds that it will enhance other enterprises in which we take an interest, there are many instances in which we identify two very different sources of value and are unwilling to slight either completely. In reflecting on our apportioning decisions, we recognize that we sometimes make them badly. Over the course of our lives, we develop strategies for avoiding the kinds of mistakes to which we are most susceptible.

Whatever skills we develop are put to work in joint decision making with family and friends. We would think it absurd to make plans by immediately drawing up a list of options, taking a vote, and proceeding in whatever way achieved the majority. Better to talk first. An outcome that represents the col-

lective will should be based on genuine appreciation of the possibilities, on recognition of the felt needs of others, on understanding how the options would bear on those needs, on tracking the ways in which all of us modify our views in learning about what others want, and on a determination to avoid an outcome that someone would find unacceptable. Except when something has to be done very quickly, it is worth taking time to explore what others know and what others want. If voting ever occurs, it is as a matter of last resort, when we reluctantly agree that consensus is impossible.

These important characteristics of responsible decision making, both in balancing our own lives and in joint activities with those about whom we care, are reflected more precisely in the conditions of mutual engagement (§7), and those conditions yield my ideal of well-ordered science. A society practicing scientific inquiry is well ordered just in case it assigns priorities to lines of investigation through discussions whose conclusions are those that would be reached through deliberation under mutual engagement and which expose the grounds such deliberation would present. The society is likely to contain many different views about how the course of inquiry should now proceed; some, maybe most, of these perspectives may be sadly handicapped by ignorance of the state of the various sciences. Given the cognitive requirements on mutual engagement, that must be corrected. So we should suppose that, in an ideal deliberation, representatives of the various points of view come together and, at the first phase of the discussion, gain a clear sense of what has so far been accomplished and of what possibilities it opens up for new investigation. Those who have been addressing the technical questions of particular fields explain why they regard certain findings, particular products of research, and various currently unanswered questions to be significant. Sometimes they suggest that a question has intrinsic interest, that answering it would satisfy human curiosity; on other occasions, they relate how the answer has practical potential; on yet others, they mention both kinds of factors. At the end of this explanatory period, all the participants in the deliberation have been *tutored*; they have a picture of how the various fields of inquiry are currently constituted, in the sense of seeing how significance is taken to accrue to projects researchers have undertaken in the past and a range of options now available.

At this stage, the deliberators assess those options by voicing their own preferences. Initially, their preferences will embody their individual points of view, already amended from their previous untutored state through a clear

appreciation of what might advance their personal goals. The views they set forth thus reflect their newly achieved awareness of the current state of the sciences. As each listens to the attitudes of others, preferences are further modified, since each wishes to accommodate the others, insofar as this is possible and, especially, to avoid outcomes that leave some of their fellows completely unsatisfied. Where there are difficulties and disagreements, they use the processes of mirroring, primitive and extended, to consider their potential actions from a wide range of perspectives.

As they look toward the future, their assessment of consequences, for themselves and for others, will sometimes require judgments about the likely outcomes of pursuing various investigations. Here they will need the testimony of expert witnesses. The pertinent experts are selected by following chains of deference: all participants initially defer to the community of scientists; within this community, there is deference to fields, subfields, and ultimately to individuals. Sometimes, of course, there will be serious controversy, and the chains will bifurcate. When there are rival "experts" making incompatible forecasts, the entire package is presented to the deliberators, together with the grounds on which the various estimates are made, as well as the track records of those who make them.

Conversation may end in one of three states. The best outcome is for the deliberators to reach a plan all perceive as best. Considering the conduct of inquiry within the entire spectrum of their society's projects, they judge a particular level of support for continuing research to be good, and they agree on a way of dividing the support among various lines of investigation. Second best is for each person to specify a set of plans he considers acceptable, and for the intersection of these sets to be nonempty. If there is a unique plan in the intersection, it is chosen; if more than one plan is acceptable to everyone, the choice is made through majority vote. The third option occurs when there is no plan acceptable to all and when the choice is made by majority vote. That is a last resort for expressing the collective will.

Three points should be obvious. First, the procedure outlined applies to the problem of assessing scientific significance, reflecting the general approach to value-judgments developed in §7. Second, that procedure idealizes mundane occasions of what would be viewed as good decision making. Third, any *actual* conversation of this type is impossible. This last fact may incline you to think it absurd to approach scientific significance as I have done. Understanding an *ideal*, however, can sometimes help us to improve

our practice, and this is the hope of my proposal. The next section will attempt to disclose some reasons for hope.

So far, the ideal is not fully specific, since it refers, vaguely, to the range of points of view present in a society without saying how large or small this society may be. Chapter 2 favors a *broad* conception, one that would require scientific significance to be assessed by considering all the alternative perspectives present in the human population, including those of people yet unborn. Of course, those future perspectives cannot be known with any precision, but they can be estimated by further ventures in mutual engagement, by sympathetic understanding of their attitudes toward particular world-conditions we might bequeath to our descendants: it is hardly speculative to suppose they would be indifferent to a world in which violent disruptions of agriculture and water supply were commonplace. Plainly, one could draw boundaries more narrowly. One obvious way to do so is to propose that societies are identified with nation-states: the scientific practice of a particular nation is well ordered just in case its judgments about significance reflect those reached in an ideal deliberation embodying *all and only the perspectives present in that nation.* There are many others: you could confine the deliberators to some group of scientists, or to the community of tycoons, or to people who score above a particular value on some test purported to measure intelligence, or to "gentlemen, free and unconfin'd."

The possibilities just listed are intended to be unattractive. Not only is it an obvious retreat from any ideal of democracy to leave the judgments to the few, but it flies in the face of the significance of Science as an institution. Seventeenth-century gentlemen could pursue what they liked, for they had no basis for understanding how enterprises descending from theirs would transform the world, not just the world of their comfortable environs but the world inhabited by *everybody.* Future perspectives deserve representation because we know how consequential present decisions are for the people who will come after us. The choices we make will have important effects on the problems to be confronted tomorrow. Human needs arise in an environment, and the environment, since the seventeenth century, has been increasingly shaped by the particular course inquiry has taken. How could value-judgments ignore the standpoints of future people—how could ideal deliberations leave them out? By the same token, how could the scope of the conversationalists be restricted to a privileged subset of those who live now?

The most plausible rival to the broad conception is the first of the narrow

conceptions given in the last paragraph: confine the perspectives represented to those of nation-states. Consideration of other ways of narrowing poses a challenge to all the rivals. What is it that makes narrowing in a specific way acceptable and further confinement illegitimate? Why not the gentlemen, or the members of Mensa, or the tycoons, or the scientists? The best answer focuses on the status of nations as economic units. Nations produce the resources needed for supporting inquiry, and that gives citizens of a particular nation a privileged voice in determining scientific significance. If Americans are contributing more to supporting Science, their needs should be given greater weight.

Seductive though it may initially seem, this line of argument is obviously dangerous. If division by productivity is appropriate, why not carry it through more finely and on a broader range of issues? Strictly speaking, the productivity of a nation emerges from the efforts of its individual members. Why not give those who generate more a proportionally greater say? Why not apply the principle across other decisions, apportioning votes on electoral offices according to the contributions made to the national resources: to each according to his productivity . . . ? Although questions of this sort embarrass the narrow conception, the more basic reasons for its unacceptability lie in the general approach to value found in chapter 2.

Imagine that ideal discussion according to the broad conception would license a line of inquiry that would be rejected on the narrow conception. Suppose further that the pertinent research program has severe consequences for some particular population of nonaffluent people. How could the decision not to pursue it be explained to them? It would have to be acknowledged that their perspective was not included in an ideal deliberation, and that the basis for leaving them out was economic: the evaluation of significance proceeded by consulting those who contributed the resources to be used. If all nations had resources available for supporting research meeting their particular needs, the project would not falter, since the group to whom it is important could include it on their own agenda. Because of their poverty, however, they are in no position to pursue scientific research. Hence, proceeding on the basis of the narrow conception has severe consequences for them.

Behind the contemporary worldwide distribution of resources stands a long and tangled history. It would be very hard to defend the judgment that all are now rewarded according to their deserts: the route to the present has involved all sorts of murky acts, as well as plenty of luck. *There is thus no*

basis for any group of people with a heterogeneous distribution of resources to accept the view that decisions profoundly affecting human welfare should be made on the basis of restricting views to those who happen to have done well. As a result, even a generic commitment to the approach to values in terms of discussion under conditions of mutual engagement, one that does not yet specify the size of the population of discussants, cannot endorse the grounds on which the narrow conception is based. The framework of chapter 2 thus requires the broad conception for the ideal of well-ordered science.

19. WELL-ORDERED SCIENCE: DEFENSE

Many people, especially scientists, react to a plea for democracy with alarm, insisting on the autonomy of scientific practice. Part of the fear stems from suspicions that democratization, even in the guise of well-ordered science, will submit research to the tyranny of ignorance. It is worth repeating that well-ordered science is deliberately designed to overcome this problem, that it imposes stringent cognitive conditions, and that it assigns an important role to the authority of experts. Moreover, scientific autonomy, like that of other agents, covers many spheres of activity, and it is important to understand just which of these might be threatened. Very likely, the image of the autonomous scientist is a residue of the original commitment to private activity, embodied in the "gentlemen's clubs" of the early modern period, no longer apt when Science has become central to the public knowledge system—despite Vannevar Bush's ingenious attempt to combine public support with the maintenance of a class of Platonic guardians.

In this section, I plan to respond to some common objections to well-ordered science, many of which descend from insistence on scientific autonomy. The most basic form points out that the scientific community has a clearer vision of the collective good to be realized through inquiry. As things stand, that is probably correct—and if one had to pick any single group to decide what lines of investigation to pursue, scientists would be the most appropriate choice. Yet the asymmetry between scientists and the lay public should not be overblown. One of the most fundamental thoughts behind democracy is that individual people have a better understanding of aspects of their own predicament than do outsiders, however wise and well-intentioned. Some years ago, a team of investigators visited a group of

African pastoralists and discussed with them the possibility of developing vaccines for their children. Their interlocutors asked for some time to ponder the issue and, when they returned, made the unexpected suggestion that a vaccine for their goats would be even more welcome.[7]

Furthermore, as anyone who has ever heard different groups of scientists debating the promise of their own special fields will know, even if the scientific view is more farsighted than that of outsiders, it is typically myopic. Each specialist tends to view the scientific universe in the style of Saul Steinberg's famous Manhattan cartoon (in which the Upper West Side has far greater prominence than Middle America, the Pacific Coast, or Asia). To construct any balanced view of research possibilities would require something like the ideal conversation envisaged, at least among representatives of various scientific fields, and, when the insights of individuals into their own needs are appreciated, it becomes evident that outsiders ought to be included. Well-ordered science emphasizes the importance of tutoring, precisely because, to pursue their interests, the outsiders will need the various kinds of special knowledge the scientific community can supply. Rather than trying to drown out responsible judgments with a chorus of ignorant voices, its conditions fuse the different kinds of knowledge distributed through the human population.[8]

Champions of autonomy will proceed to more sophisticated objections. "We already know," they declare, "that directed scientific research goes badly; that it has been wonderfully fruitful in the past for brilliant scientists to explore their hunches, that unanticipated benefits come from inquiries into apparently impractical questions, and that the course of science is unpredictable." Arguments like these are often made from the armchair—or *ex cathedra*. The autonomist has a few bits of anecdotal evidence, having read a book on Lysenkoism and a biography of Einstein. In fact, little is known in any systematic way about the responsiveness of scientific research to social directives. The basis for any hypothesis about the bad effects of something like well-ordered science is extraordinarily thin. The autonomist's pronouncement rests on the sorts of judgments routinely denounced in basic courses in methodology in any scientific field: sketchy histories are invoked without any attention to sampling or to proper comparisons. So far, the social study of scientific knowledge cannot deliver a statistical basis from which anyone can project the likely effects of attempts to plan different kinds of research. More fundamentally, however, insofar as genuine knowledge about social direction of inquiry, success of brilliant individuals, or fruits of

research into pure topics, is, or becomes, available, that knowledge could and should be employed to further the democratic process. *It should be part of what the ideal discussants know.* So, in their deliberations, they can take into account the track record of different attempts to direct inquiry.

Many contemporary molecular biologists would frown on centrally directed ventures that attack prominent medical problems—analogs of the "War on Cancer"—insisting that the route to success is often indirect. Their reservations are not antipathetic to the ideal of well-ordered science, however. To acknowledge a particular problem as practically significant, as when cancer is seen as requiring major scientific effort, is not to favor any specific strategy for addressing that problem; for example, a blind assault that dismisses all attention to "basic issues" in pertinent sciences. Strategy should be informed by what is known about the past successes and failures of various ways of conducting investigations aimed at similar ends—and that is exactly what well-ordered science demands.

The last part of the autonomist's protest deserves a slightly different response. What exactly follows from the fact that we cannot foresee the course of science? Is it supposed that no decision we can now make about issues that matter is preferable to any other? Are past attempts to allocate priorities among lines of scientific research arbitrary and capricious? If so, the autonomist's own confidence in the wisdom of scientific judgments would be undermined. You might just as well toss coins or read tea leaves. The practices the autonomist wants to preserve testify to our understanding that, while we cannot make fine-grained predictions about what research will bring, we are not completely clueless. We know for example that needs are more likely to be met if more effort is expended in one direction rather than another: stepping up research into mechanisms of gene transcription is not likely to enable us to slow global warming—it might, but the probability is not high. The scientific situation is, again, akin to ordinary circumstances of decision making. Families plan for the education of children and the retirement of parents in ignorance of crucial information about what the future will bring. They know that unforeseen contingencies might disrupt the most well-considered plans. Responsible people do not conclude that they might just as well spend to their credit limit (or beyond). In light of the best judgments they can make, acknowledged as rough, they seek the most likely paths to achieve their ends. Working in concert, the scientific community and the broader public ought to be able to achieve something similar.

It may still appear, however, that the ideal of well-ordered science is too pragmatic, too restrictive. Should there not be some place for people whose lives are centered on projects of disinterested inquiry, whose plan for their mortal span consists in answering questions about which others do not care? They do no harm. Often they work for no great rewards. Why should the Republic of Letters not allow them a modest place?[9]

Even under well-ordered science there might be room for impractical dreamers—for there might be benefits to all from allowing them to follow their own fancies. During the Early Modern Period the status of mathematicians changed, as it became evident that new extensions of mathematical language might prove useful resources for inquiry generally. In effect, mathematicians were given a license to address esoteric questions they found interesting, and the decision to grant that license has paid off handsomely. So, too, it might be more generally. In the end, however, making a place for the satisfaction of refined curiosity ought to be defensible in the ideal democratic conversation well-ordered science envisages. The informed deliberators ought to be able to recognize the value of pursuing these inquiries, even if the benefits are indirect. They ought to be convinced not only that no harm is done but also that the talents of these investigators are properly used, contributing to the broader human good. To think otherwise is to yearn for the existence of seventeenth-century gentlemen "free and unconfin'd," even though the social world has changed and Science has become central to public knowledge. That change brings responsibilities that those drawn to "projects of pure disinterested inquiry" ought to recognize.

Turn now to a different worry about well-ordered science, not that it constitutes a clumsy form of interference with a valuable institution (one better left to the wise people who contribute to it) but that it does too little to change the status quo. Is the ideal toothless? Can it be developed precisely enough to recommend modifications in current research agendas?

Consider contemporary biomedical research. Most of it is carried out in affluent societies, and almost all of it concentrates on diseases afflicting people in those societies. (At least, that is what the community of researchers tells those who ask what they are doing; a closer look would surely reveal many investigators working on "pure" problems in "basic biology," questions whose significance they could usually defend as likely to yield medical advances in the more or less distant future.) Contrast the distribution of disease research with the statistical data on worldwide dis-

ease and disability. Diseases that cause a vast amount of human suffering, particularly among children, receive only a tiny part of the investigative effort. In some instances, that is because the pertinent disease has already been "solved": a method of prevention, cure, or treatment is available and can protect children in the affluent world. The fact that the method cannot be imported into the circumstances in which poor children live—and fall ill, and die—does not affect the status of the "solution"; it is not, on that account, recognized as partial.

Well-ordered science recommends a plausible principle: the *fair-share principle*. Waiving considerations of tractability, each disease should be investigated according to its contribution to the total suffering caused by disease. A simple measure, applicable only to fatal diseases, would measure the contributions by the numbers of resultant deaths. More subtle appraisals discount the years of a person's life by the disabilities to which she is subject. However the contributions are assessed, if the principle is applied directly to the statistics on disease incidence, it is evident that actual research into diseases is skewed toward conditions affecting affluent people. Many diseases that kill or incapacitate poor people receive support on the order of one-hundredth of their fair share (Flory and Kitcher 2004; Reiss and Kitcher 2009).

Mechanical application of the fair-share principle would be foolish, since considerations about profitable inquiry should attend to considerations of research promise. Hence, the formulation given introduced the proviso that issues of relative tractability were waived. Consequently, the actual distribution of research effort might be defended by proposing that the affluent diseases actually investigated—possibly even overstudied—are especially likely to yield important insights. Any defense along these lines would have to cope with the fact that contemporary biomedicine supplies promising tools for tackling diseases that bring misery to millions. Genomic sequencing of pathogens offers clues for designing effective vaccines capable of transportation to the environments in which they are needed. There are no sure-fire strategies (particularly in the case of rapidly mutating infectious agents), but knowledge of the genome can indicate potential genes, encoding proteins likely to appear on the surface of the disease vector; if such proteins can be inserted into benign micro-organisms, it is possible for them to produce antibodies to the pathogen. In contrast to many diseases currently attracting large support (because they afflict rich people), a large number of understudied diseases of the poor lend themselves to a more systematic pro-

gram of research. If anything, these diseases are *more* tractable than those actually investigated.

Well-ordered science requires refiguring of the medical research agenda in light of considerations of global health. Even without articulating the ideal further than I have done, it is possible to recognize places at which our actual practice would be revised. That is because the very basic needs of many people are not met, and because there are lines of inquiry promising to relieve this situation. Surely there are many instances in which it would be hard to predict much about the outcome of a conversation under conditions of mutual engagement, yet problems bearing on the health of children in regions of high mortality and disability are not among them. However they are tutored, deliberators who represent those children and their parents will be expected to continue to feel, and to express, the pains those children and their families experience. The details of the ideal conversation need not concern us when one feature of it is so evident.

Contrast this example with a different question, one that has figured in earlier sections. How is the balance between "pure," or "basic," research and investigations directed toward immediate problem solving to be struck? Recall a conclusion of §17: there are two potential bases for justifying attention to "pure" questions: pursuing them is likely to produce tools for solving a wide range of practically significant problems down the road (Vannevar Bush's "seed-corn" argument); answering them would satisfy widespread human curiosity. How these lines of justification play out in ideal deliberation depends on crucial details about the state of the sciences and about the needs of contemporary and future people. Some problems requiring investigation may be so urgent that counsel to wait for the fruits of "basic" research would ring hollow. In other areas, ideal deliberators might judge either that direct attempts, undertaken without more "basic" understanding, would be futile, or that stopgap measures could be deployed while the research community sought a more systematic solution.

Without extensive further information about the research opportunities now available—the sort of information that would be provided in the tutoring well-ordered science envisages—it is impossible to be certain that "pure" questions, conceived as stepping-stones to future practical benefits, would inevitably figure in the agenda of well-ordered science. Perhaps aggregate human needs are so urgent that we should deploy the knowledge already gained to craft directed programs of inquiry to satisfy those needs as

speedily as possible. Precisely because the knowledge available is, in some areas, so powerful, so susceptible of further development, and precisely because it has often grown out of programs of "basic" research, it appears highly unlikely that the ideal deliberation would abandon so profitable a historical strategy. Should we not imitate those many scientists of the past who posed and answered questions without any obvious pragmatic payoff—the physicists, chemists, and biologists whose basic research underlies countless present technologies? Unless you suppose the situation is truly critical, that our species faces practical problems that command direct attention, well-ordered science is likely to maintain a role for "basic" research.

Is that enough? Section 17 assigned a place to the satisfaction of human curiosity, independent of any practical benefit. Achieving satisfaction of that sort is valuable *in principle*, but it does not follow that ideal deliberators would be moved by it. Here, the outcome of the ideal deliberation is even less certain. Without a far more detailed survey of aggregate human needs, of the possibilities of addressing them directly, and of the theoretical projects justifiable on the basis of their promise for future strategies of intervention, nobody can predict how the ideal conversation would come to conclusion. Can you rule out the scenario in which research directed at immediate relief, together with lines of "pure" research that promise future fruits in application as well as answers to questions that arouse curiosity, are so abundant and so compelling that they leave no place for the luxury of knowing something "merely" for its own sake? To allow, as I have done, that disinterested "pure" understanding has a value, that it should be placed in one balance of the scale, does not guarantee its being sufficiently weighty to offset whatever occupies the opposite pan. Genuine doubt is appropriate here.

Scientists, especially those fascinated by the aspects of nature they study, will probably find this conclusion troubling—even grounds for doubting the ideal of well-ordered science. They should not. For their own perspective is registered in the ideal deliberation, their own fascination with (say) the hominid family tree is conveyed to their fellow discussants, who feel its force as they do. On what basis could they object if, after serious sympathetic engagement with all human perspectives, the practical needs of others seemed more urgent? Could they themselves engage with the other perspectives, think themselves into situations in which more elemental things than the satisfaction of curiosity are lacking, and still insist that their own "pure" questions merit attention? Especially if, as I have conceded, the search for

"basic" understanding will continue in areas in which it is coupled to future practical promise.

As we shall see in chapter 7, the value of satisfying curiosity is one—like freedom (§11)—deserving attention to its distribution. Defending the value of "pure" knowledge for its own sake is far easier if the benefits of refined understanding are widely available. Placing private satisfactions of this sort ahead of attention to urgent human problems of an elementary sort involves a failure of altruism—and remedying altruism failures is the original function of the ethical project (§6).

20. MERELY AN IDEAL?

Well-ordered science is an *ideal.* It may seem a utopian fantasy, the sort of thing that may figure in philosophical discussions but that has little place in a realistic account of the sciences (Lewontin 2002). There is an important distinction between specifying an ideal, something at which our practices should aim, and identifying procedures for attaining or approximating the ideal. To proceed to the latter task requires a large amount of empirical information, information no one yet has. Nonetheless, meaningful ideals are those for which we can envisage a path that might lead us toward them, and a philosopher who proposes an ideal should be able to point to the initial steps we might take (as Dewey insisted; it is also important to appreciate that, as we move toward an ideal, our conception of it may be refined).

Actual deliberations about the ends of the sciences are often, probably always, infected by special interests, ideological presuppositions, and inequalities of power. These facts do not diminish the importance of the ideal. They suggest difficulties to be overcome in realizing the ideal, ways in which well-entrenched features of political life might need amendment. To scoff at philosophical ideals on grounds that they require a lot of changes would be a serious mistake, for, without some understanding of where you want to go, efforts to improve on the status quo will be leaps in the dark.

My attempts to identify some steps forward will begin from diagnoses of respects in which the current framing of investigation departs in striking ways from well-ordered science. I offer four hypotheses that develop points made earlier in this chapter.

1. Present competition among scientists and fields of science is con-

strained by historical contingencies that no longer reflect human needs. Even if you thought scientists were the only people whose judgments should count in setting the research agenda, you ought to worry about the ways in which priorities are set. As already remarked, individual scientific visions are parochial. They are often pitted against one another in an arena in which there is no serious possibility of surveying the merits of competing possibilities, and in which institutional structures partition potential research proposals into areas defined in the past. History frames the current distribution of public research, often in baroque fashion. For example, the current ramshackle organization of the National Institutes of Health reflects the accidents of the past.

2. The flaws of vulgar democracy are inherited by existing systems of public input. Vulgar democracy is problematic because the preferences expressed are untutored. Contemporary public procedures for shaping the research agenda proceed from two sources: either government (typically responses to large perceived problems but often slanted toward constituencies deemed important by the politicians involved) or special groups of concerned citizens, sometimes well-informed about the issues they raise (local pollution, say, or a particular disease) but ignorant about the full range of scientific possibilities and the diverse needs of their fellow citizens, let alone those of more distant people. Priorities are set as a result of haphazard shouting of more or less powerful voices, each expressing, at best, some partial truth. Possibly public input of this sort improves the results that would be achieved if the scientific community were left to its own devices—that is an empirical issue about which we have little evidence—but there is no reason to think it takes us far toward well-ordered science. Insofar as we introduce democracy into thinking about Science, we incorporate the adversarial rather than the deliberative elements.

3. Privatization of scientific research will probably make matters worse. Government pressures and the clamor of interest groups sometimes have the advantage of representing people with urgent needs. Private investment in scientific research, ever more apparent in biomedical investigations and in the information sciences—the two fastest-growing fields of inquiry of our age—is, in both the long run and the short, tied to considerations of financial profit. One immediate result, of concern to many biological researchers, is the neglect of "basic" questions in favor of areas in which profits can be expected. The decisions issuing from two large groups, the scientific com-

munity and the general public, are likely to be dominated by a clash of parochial visions. Nevertheless, each of these groups has some connection with the ideal deliberators of well-ordered science: the scientists appreciate the significance of achievements in their own specialized areas, members of the public recognize their own urgent needs. Entrepreneurs are at a further remove. To the extent their decisions respond to genuine needs, those needs will be raw and untutored, typically self-directed, *and they will be the needs of those who pay*. Markets sometimes work the wonders frequently attributed to them, but there are systematic reasons for thinking that, in shaping scientific research, an unregulated market will produce a travesty of well-ordered science.

4. Current scientific research neglects the interests of a vast number of people, except insofar as their interests coincide with those of people in the affluent world. The example of the distribution of biomedical research and the deviation from the fair-share principle provides a striking illustration of a potentially general phenomenon. The world's poor are only accidentally represented in decisions about the lines of inquiry to be pursued. Without a more detailed understanding of their needs and aspirations, it is impossible to know just how much difference this makes, that is how frequently the neglect manifested in the biomedical case obtains.

Although each of the hypotheses is plausible in light of obvious features of our current situation, more detailed information about the attributed effects would be welcome. Assuming the diagnoses are roughly correct, it is not hard to envisage steps toward well-ordered science. I offer some pervasive problems and proposals.

Myopia. Even when informed and well-intentioned scientists try to think broadly about research options, their discussions suffer from the absence of a synthetic vision. Instead of pitting one partial perspective against another, it would be preferable to create a space in which the entire range of our inquiries could be soberly appraised. We would do well to have an institution for the construction and constant revision of an *atlas of scientific significance*. That atlas would provide maps of the various fields of inquiry, showing how significance accrues to the work that has already been done and how it might be extended in significant ways. It would connect the technical work of specialists with broad issues about which people are curious and with practical consequences for human lives. The resultant maps—*significance graphs* (Kitcher 2001)—would enable everyone, scientists and the

public alike, to appreciate the full range of current opportunities, to understand all the ways in which some inquiries might, given our best present judgments, bear fruit. The atlas would allow a more reflective view to replace the competing myopic visions offered by (understandably enthusiastic) specialists.

Ignorance of science. The atlas is part, though by no means the whole, of what is required if public input into science policy is to come closer to well-ordered science. Central to democracy is the thought that people can take political action to express their *interests*, not merely the variously misguided preferences they might have. Even before we envisage deliberators who are sensitive to the interests of others, it is important that their self-directed wishes be enlightened. Many people around the world oppose measures intended to develop alternative forms of energy and strongly want to continue their familiar practices of fuel consumption. Most of these people have a far deeper and more central wish that the world in which their children and grandchildren live should be habitable, not subject to violent disruptions that would create massive difficulties in obtaining shelter, food, water, and protection against disease. According to the contemporary consensus in climate science, these people's desires are in a state of considerable internal tension: policies framed in accordance with the short-term wishes (energy consumption as usual) threaten the more central wish that descendants will thrive. The case of climate policy is one of the starkest instances of unidentified oppression, but widespread ignorance of important parts of public knowledge contributes to many gaps between the preferences citizens express and their most central interests. If public input into scientific research is to overcome the perils of vulgar democracy, steps must be taken to increase levels of scientific literacy.

How might this be achieved? The problem is many-sided, and we shall be considering aspects of it in later chapters. For the moment, focusing on the possibility of steps toward well-ordered science, two ways of improving communication between Science and the public deserve consideration. The first would proceed from the sciences out toward the public. In recent years, there has been a shift in attitude within the scientific community, a sense that spokespeople for major scientific fields are valuable, not to be dismissed as vulgar "popularizers" or reputation-seeking "has-beens." Writers like Carl Sagan, Stephen Jay Gould, E. O. Wilson, Richard Dawkins, and Brian Greene have done valuable service by explaining major ideas lucidly and

elegantly. Their writings and their television appearances have greatly expanded public understanding of science—and it was a signal of their achievement that the United Kingdom instituted professorships in the public understanding of science and appointed Dawkins to the first chair (at Oxford). This trend could be extended far more widely, and scientists who are especially good at communication could be encouraged to view this as a central part of their mission.

Conversely, it would be possible to create groups of citizen representatives, drawn from diverse segments of different societies, who would undergo some practicable approximation to the tutoring envisaged in well-ordered science. These people would be "led behind the scenes," brought into thriving areas of scientific research, and given explanations of the state of knowledge, of the lines of envisaged future progress, and of the accompanying difficulties, as the specialists see these things. The atlas of scientific significance would be explained to them. After discussions with one another, they would then be available to the broader public, to report on their—non-expert but informed—understanding of the state of inquiry, and to discuss possibilities with a much wider group of lay citizens. In light of these discussions, they could then return to conversation with specialists, acting as intermediaries in facilitating information flow and dialogue.[10]

Ignorance of others. The ideal deliberators envisaged by well-ordered science not only understand the state of scientific knowledge but also recognize one another's needs. Although no readily constructible institution could provide all the nuanced understanding available in the ideal conversation, it is surely possible to remedy some of our ignorance. The atlas of scientific significance could be supplemented with an *index of human needs.* That index would be built up by systematically exploring human problems as they are perceived by the people who encounter them. Ideally, the investigations would proceed by striving to isolate deep desires, real interests that might sometimes be masked by distorting ignorance, so that here, too, there would be efforts at tutoring to clear away common misapprehensions and problems of unidentifiable oppression. Imperfect though such efforts would be, even rough approximations would enable research to be guided in ways that no longer leave out large segments of our species.

Failure of sympathy. Ideal conversationalists not only know the wishes of their fellows, they also adjust their own preferences to accommodate others. Overcoming ignorance about the plight of people whom inquiry often

neglects should be the prelude to sympathetic identification with them. Here, as in the case of ignorance about science, the issue is many-sided. One part of a remedy would take seriously the idea that part of education consists in the encouragement and expansion of altruistic tendencies.

Another would lie in commitment to exposing cases in which scientific research is distorted through subordinating benefits for many to economic profits for the few. Scholarly research sometimes reveals how inquiry is directed toward ends quite different from any public good: pharmaceutical companies do not produce a drug that could cure thousands of poor children because there is no profit in it; well-known scientists with ideological commitments or ties to particular industries block public awareness of important information (Oreskes and Conway 2010). Commentators on the sciences need to pursue inquiries of this sort more widely, and their findings, when well documented, should be well publicized (that is part of the responsibility of journalism). As the public information system becomes fractured between public and private forms of support, it is important to keep track of the places in which an "invisible hand" really does operate, producing outcomes that yield widespread benefit and those in which the market harms the many for the enrichment of a very few.

All the proposals I have made need further refinement and development. They are attempts to respond to the challenge posed at the beginning of this section, to show that, even though well-ordered science imposes strong—unrealistic—conditions, we can nevertheless envisage steps to take us closer to it. In specifying the path more exactly, it is possible to learn through small-scale social experimentation. Researchers can investigate—and have investigated—the merits of various ways of improving communication among different groups or facilitating outside oversight of decision making (Fishkin 2009; Jefferson Project). The institutions whose functions I have sketched would best be fashioned in light of such research and through trial of various possibilities. In proposing that we explore in this way, I reiterate a theme of earlier discussions: our system of public knowledge is the product of a tortuous history, and there is little reason to think it has delivered to us a set of institutions insusceptible of any improvement.

21. CONSTRAINTS ON PURSUIT

To close my discussion of questions about investigation, it is worth looking more briefly at the phase of inquiry that follows the setting of the agenda. Various important questions have been isolated (and we hope the decisions correspond approximately to those that would have occurred under well-ordered science). How are they to be pursued?

Typically, we expect investigators to be well-informed about the best methods for achieving their goals, and to follow those methods. We shall later consider complications that might arise when there are various possibilities for proceeding (chapter 8). For the moment, however, attention will be restricted to two main issues: Who are these investigators? Are they subject to constraints that might not be commonly appreciated?

During recent decades, it has become a commonplace that certain ways of doing research are not to be tolerated: commentators look back in horror on the notorious Tuskegee experiments (in which African Americans known to be infected with syphilis were deliberately left untreated) and on the "science" undertaken by Nazi doctors in the death camps. Ethical limits are imposed, even when the cost of the restrictions is that questions we hope to address become more difficult or even unanswerable. Sometimes, urgent issues about the causes of a disease could be settled by selectively exposing people to pathogens; we could answer questions about nature and nurture relatively directly by separating carefully selected children from their families and rearing some of them in bizarre environments. All scientific communities now acknowledge ethical constraints forbidding such experiments. Communities also frown on scientific piracy, attempts to acquire without consent the data obtained by others, although they also recognize that scientists have obligations to share their findings. Do these restrictions exhaust the proper constraints on the pursuit of knowledge?

The examples just given have more subtle relatives. Much contemporary research employs sentient animals, sometimes submitting them to unusual pain, sometimes bringing into existence creatures whose lives will be short and unpleasant. A total ban on experiments that inflict suffering on animals would inhibit many lines of inquiry with great potential for alleviating human agony and misery. A completely tolerant attitude toward animal suffering would allow investigations that pursue trivial goals. Where are the lines to be drawn?

The perspective on value-judgments of chapter 2 provides a basis for decision. Again, it is a matter of balancing valuable goals against one another. As in the framework of well-ordered science, a proper verdict would be one achieved by ideal deliberators, well tutored and mutually engaged, who considered the effects both of the proposed experiments and of not undertaking them. Since the affected parties are people who suffer from disease, on the one hand, and sentient nonhuman animals, on the other, it is crucial that both these groups be represented in the conversation.

How can that be? Even if a tiny few of the animals who would be affected are credited with some linguistic skills, those are far too rudimentary for them to engage in the kind of conversation envisaged. Indeed, there appears to be a very general objection to my approach to values; to wit, that it arbitrarily excludes our many sentient relatives. Despite its inclusiveness with respect to the human population, is it guilty of an illegitimate human chauvinism, something some might take to be as noxious as ethical stances that have confined their attention to a small subset of the human population?

Ideal conversation already has to represent *people* who cannot speak for themselves. Members of future generations are not available to comment, nor are the very young, nor are adults suffering various types of disabilities. Their perspectives are to be included through representation by involving people who know them intimately and who are devoted to their interests. So, too, for nonhuman animals. If we were to try to simulate an ideal conversation about the propriety of using an animal subject in a particular fashion, it would be important to involve people who could supply details about the animal's physiology, its responses to various kinds of pain and deprivation, its kinship with human sufferers, and so forth. Equally, it would be crucial to include those intimately familiar with the sufferings of human disease victims, people who might obtain relief if the animal experiments were permitted.

The situation might well be completely symmetrical. Some forms of human disease strike suddenly and, when they do, preclude any possibility of the patient's testifying on her own behalf. Under such circumstances, both primarily affected groups have to be represented by others, and the responsibility of the representatives is to provide an adequate account of the consequences, one that will enable everyone to reach a decision about whether to allow the proposed program or to debar it. No doubt these choices will sometimes be hard—although actual cases are often more tractable than the stark scenarios beloved of abstract philosophy, in that there are ways of min-

imizing animal suffering or pursuing human benefits along alternative lines. There is, I submit, no better way to make them than to replicate, to the extent we can, a conversation that proceeds through mutual engagement with all the potentially affected parties.

It is similar for other difficult examples. Sometimes people who are passionately dedicated to particular causes, or who know that their lives will soon end, volunteer as subjects in experiments in which involuntary participation would be banned. If those people truly feel that participation is a constituent of their life project, central to who they are and what they aspire to, preventing their noble sacrifices would be an interference with their freedom. The obvious suspicion is that some form of coercion has been at work, that, at bottom, these people are no more expressing an autonomous choice than were the Tuskegee subjects or the "patients" of the Nazi doctors. To address worries of that sort, we can adapt the ideal of well-ordered science and the more general approach to value-judgments in terms of ideal conversation it embodies. Volunteers would discuss their plan of action with people of different perspectives, including some who were dedicated to their welfare and some who were suspicious about social coercion, aiming to replicate insofar as they could conditions of engagement with the would-be experimental subjects. Permission would depend on their final agreement.

Or consider more complicated cases of scientific piracy. We frown on stealing data when those with important information act swiftly to release it to their colleagues. There are, however, envisageable cases (maybe actual examples) in which the data are urgently needed and a pathologically doubtful investigator feels the need for further trials. Does a genuine ethical constraint debar someone deeply concerned with people threatened by the delay from attempting to tease the findings out of subordinates in the lab or even to find ways of looking at a crucial photograph? Is the dithering doubter living up to the proper responsibilities of a scientist? To answer such questions, we can only appeal to judgments about the case, formed through the best approximation to ideal deliberation we can contrive.

Contemporary practice, especially in the biomedical sciences, already embodies a healthy approximation to the ideal and even the procedures I am recommending. Although researchers sometimes complain about them, institutional review boards provide good ways of elaborating and applying constraints on research. If their discussions are currently problematic, that is not because of the recourse to conversation but a result of the channels through

which the discussion flows. On the approach to values recommended in chapter 2, decisions should not be made by wielding abstract principles (of the sort medical specialists absorb from simple philosophical textbooks and struggle to apply) but through deep immersion in the case from a variety of human perspectives. As the cognitive conditions on mutual engagement demand, the conversation must not be stopped by announcing religious precepts—for those have dubious authority—but neither is there any secular source that can transcend the authority of the conversation. Promising practices of research review could be improved by finding ways to bring deliberation closer to conditions of mutual engagement (perhaps by increasing the diversity of perspectives), and, in principle, those practices could be applied elsewhere, as in the imagined conflict between the dithering doubter and the researcher who desperately needs the results currently withheld.

The example of possibly permissible piracy introduces a point about scientific responsibility, for we might judge that the doubter fails to live up to the demands of responsible investigation. Coordinated activities require people to do their bit, to discharge the tasks assigned them so that a common goal can be realized. Thinking of Science in this way, as a collective attempt to expand and refine public knowledge, imposes apparent burdens on researchers—they are no longer "free and unconfin'd." They can be held accountable not only for what they do but for what they fail to contribute. We can approach my other question about the pursuit of knowledge—Who are the investigators?—in light of this perspective.

Consider an obvious extension of well-ordered science. At the end of the ideal deliberation through which they have drawn up the agenda, the discussants turn to a different question. How are they to assign the members of the community of researchers to the projects they have selected for pursuit? We can imagine them to have extensive knowledge of track records and talents. Combining this with the priorities they have set, they act as field marshals, assigning the troops to their tasks so as to maximize expected success.

Confronted with this totalitarian vision, many people—perhaps all scientists—will surely protest. Here is an intolerable invasion of autonomy! No scientist should be told what research project to undertake! These protests are entirely justified. The imagined extension of well-ordered science is compatible neither with democratic ideals nor with the approach to values I have proposed.

To see why this is so, it is useful to distinguish two different questions:

(A) Are scientists ethically required to undertake the kind of work that would best advance the community goal (the promotion of public knowledge in the form emerging, in the context, from the ideal deliberation)? (B) Should there be a procedure within Science for making scientists do what is ethically required of them (assigning them to the tasks that would best advance the community goal, or punishing them if they refuse to undertake those tasks)? Notice first that, even if you were to believe in an affirmative answer to (A), you might give a negative answer to (B). There are many kinds of human conduct that depart from or violate ethical obligations that we do not bring within the scope of coercion or punishment—and for which we think that coercion or punishment would be a breach of ideals of freedom. Democracies rightly leave leeway in the ethical choices of citizens.

It is wrong to suppose, however, that the answer to (A) is an automatic yes. The supposition descends from a misunderstanding of the attitudes of the ideal deliberators. They are imagined as having finished setting the agenda and proceeding to the optimal distribution of tools for its implementation. *Were they to proceed in that way, they would be guilty of a crass failure of mutual engagement.* For the scientists they envisage "assigning" are not *tools* but *people* whose perspectives and projects ought to be represented in the deliberation. Under many circumstances, the fact that scientist X is already passionately committed to thinking about question Q makes X less good as a candidate for inquiring about Q*, even though, without that passionate commitment, X would be the optimal person to investigate Q*. Furthermore, even when X's passion for Q would not invalidate his status as the best investigator of Q*, that passion should be taken into account and respected—for the ideal deliberators recognize its role within X's life projects, and they are concerned, when other things are equal (or approximately equal) to promote success in life projects. The perspective on values adopted in chapter 2 thus allows for cases, probably the overwhelming majority, in which scientists have no ethical obligation to pursue questions other than those they freely and reflectively choose.

There will, however, be occasions on which the ideal conversation is more demanding—*and these correspond to obligations we already recognize*. Suppose that Q* is enormously important, that the lives and projects of many people turn on investigating it successfully. Assume further that X is significantly more likely to carry out the investigation successfully than anybody else, and that X, like everyone else, knows that. Although Q fascinates

X, an answer to it is not particularly urgent. Under these circumstances, ideal deliberators would conclude that X has an ethical obligation to pursue Q*—and if X were to engage in conversation under conditions of mutual engagement, X would appreciate the obligation. Although they are not common, circumstances of this sort are quite familiar. A state of emergency calls suddenly for particular lines of research, and scientists drop what they have been doing and play the roles others ask of them. They go, for example, to Bletchley or Los Alamos.

Fear of well-ordered science as leading to the research gulag is unfounded. It is possible, however, that ideal conversation, aimed at balancing the claims of private projects and the public good, might impose more general obligations on people—not just researchers but all citizens who contribute to some collective enterprise—than those of which we are currently aware. We should come to view those obligations as a broadening of the special instances we already recognize, when, say, some large danger calls for us to modify our activities, as a call to greater unselfishness.

The distribution of researchers reflects individual preferences, but it is entirely legitimate for the community to offer incentives to guide investigative effort toward important projects, currently neglected. (Chapter 8 will take up this issue more systematically.) I want to close with a brief look at a related question. It is tempting to think of the pursuit of knowledge as a closed enterprise: promising young people are thoroughly trained, and, eventually, they become part of a community whose members address the questions viewed as significant. Outsiders are not expected to make any contribution. Possibly their efforts are even discouraged.

There are some areas of inquiry in which efforts to bypass the standard training regimes waste time and resources. To address the technical questions adequately requires specialized knowledge, to operate the equipment properly demands experience. Anyone who has ever edited a professional journal is familiar with the submissions that confidently claim to overturn received knowledge: the "refutations of Einstein," for example. It is not always so, however. G. H. Hardy deserves enormous credit for his willingness to read far enough in the curious letter sent to him from India to recognize genius, even if that genius was expressed in unfamiliar, even amateurish, terms.

Even in a predemocratic society, scientific research was open to the contributions of outsiders. The gentlemen of the Royal Society listened to the

reports of sea captains who had visited distant regions of the globe (even if their accounts of mermaids in the Sargasso Sea were not altogether reliable). Democratic societies might well explore ways of making greater use of people who are not professional scientists: naturalists with an eye for local flora, dedicated amateur astronomers. As I finish this chapter, news sources have announced some interesting results achieved by an unusual team of investigators. Computer game aficionados have made some advances on the recalcitrant problem of protein folding, not because they have deep chemical knowledge but through their experience in transforming images on the screen. Thanks to the design of an ingenious game—Foldit—a different set of skills can be marshaled for scientific inquiry. It provides an interesting precedent for further ways of widening the set of contributors to public knowledge and thus not only advancing a specialized field but also making Science more democratic.