Can Values Be Good for Science?

Lecture delivered to the Buenos Aires Sigma Xi Network, April 13, 2004*

Helen E. Longino, University of Minnesota

I am honored by the invitation of the Buenos Aires Sigma Xi Network to speak with you and I have been looking forward to the exchange of ideas which your invitation makes possible.

My topic is the role of values in the sciences. One may address this topic from many angles. These days, in the United States, we hear increasingly of the corrosive effects of commercial and industrial funding on the sciences, especially biomedicine. A new book by the sociologist Sheldon Krimsky (Science in the Private Interest) details how scientists funded by the private sector are required to withhold data and to accept constraints on publishing negative results about the effectiveness of new drugs. He tells of the punishment of university scientists who call attention to such corporate misdeeds, when the corporations are donors to the university. A few years ago, The Guardian newspaper reported on a practice whereby private companies paid scientists in university or non-corporate research positions to place their names as authors of studies on company products (primarily pharmaceutical) that the company had itself performed. "Rent-a reputation" we might call it. Biomedical journals had to institute new disclosure requirements to stop this practice. In addition to scandals such as these, scientists representing corporate interests (from biotech to oil and nuclear) sit on the review committees established by the Food and Drug Administration and the National Research

---

* Lecture based on a paper to be published in Values, Objectivity, and Science, edited by Peter Machamer and Gideon Wouters (Pittsburgh, PA: University of Pittsburgh Press, 2004)

Council to address such issues as food safety and environmental impacts of various energy production and consumption practices.

Such commingling of the commercial with the epistemic threatens to undermine public confidence in the sciences. In these kinds of cases, it seems that a dose of good old-fashioned values -- like honesty and integrity -- is what is required. Rather than relate horror stories that understandably evoke a demand for such values, I want to speak with you about a deeper issue – a failure of our philosophical ideas about knowledge that could have an even more serious effect on the credibility of the sciences.

One of the social benefits of science that doesn't consist in technological wizardry is its offering a model of disinterested seeking after knowledge and understanding of the natural world – a place where reason and observation hold sway, not propaganda, not wishful thinking, not public relations.

To the extent we think the sciences achieve this we place our faith in what scientists tell us about not just basic processes of say, star and galaxy formation or photosynthesis, but about the causal processes we are involved in everyday: the differences between bacteria and viruses, about the physiological workings of our bodies, about the relation between the consumption of fossil fuels, the loss of forest cover, and climate patterns, about the effects of exposure to various levels of ionizing radiation, or dioxin, or lead, or mercury, or – well – name your favorite culprit.

Those of us who have inherited the intellectual, social-political, and material legacy of Europe since the 16th century look to the sciences to give us answers to our questions about the natural world in order that we can act in an informed way in it. For

better or worse, this is what living in modernity means. Reliance on science is, however, too often pressed upon us through an image that is misleading: an image that says that there is some one thing called scientific method whose application to the natural world ultimately yields one unified comprehensive account of any given phenomenon and that guarantees that social values – political, moral, aesthetic – are barred from the arena of scientific activity. The values of neither the polluter nor the environmentalist, of neither the pharmaceutical vendor nor the health activist find their way into good science. Instead good science practiced in freedom from these values can serve as a neutral arbiter among these competitors.

This idea, that there is a method that guarantees that science is value-free, a method that constitutes a non-permeable boundary between science and society, is the one I want to challenge. It is based on a mistaken idea about the kind of knowledge that the sciences can provide as well as about the kinds of values that find their way into the sciences. If we expect too much from the sciences, then when they give us what they can, rather than what we mistakenly expect, we may become disillusioned and reject the sciences altogether.

My ambition is to offer an account of knowledge that is both more realistic than the one genuine method account, that doesn't remove science from its social context, and that nevertheless shows how scientific inquiry merits our trust. To develop this idea I will

Review why freedom from (social) values is thought to be a virtue for the sciences

Indicate how doubts about science's value-freedom and objectivity have arisen

Point to an assumption that has blocked fruitful discussion of these doubts

Outline my more realistic alternative

Bring this alternative view to bear on questions about values

I will argue for what might seem a paradoxical idea – that the way to achieve the goals which value-freedom and neutrality are supposed to achieve may be through more values and not fewer values.

So, first. There is a reason why philosophers worry about claims that seem to undermine the value-neutrality of science. It is worth pausing, therefore, to note why value-freedom has been thought to be an ideal of and for the sciences.

It is a virtue for science because we want our acceptance of theories to be the result of responsible cognitive practices, answerable to some notion of how things are, and not a matter of wishful thinking. In a culture in which so much rests on the sciences we fear that certain kinds of values will lead to acceptance of representations of the natural and social worlds in theories, hypotheses, and models that favor the interests of certain members of or groups in society over those of others. The ideal of value freedom is also bound up with the ideal of universality: what counts as a scientific truth or scientifically supported claim for any person or community should count as a scientific truth or scientifically supported claim for any other, no matter how different their cultural values.

The natural sciences were described by many philosophers of science as exemplifying the ideal of value freedom because they prescribed or were thought to

prescribe methods of hypothesis and theory testing that guaranteed that hypothesis and theory testing , and hence our acceptance of theories, hypotheses and models, relied on logic and observation alone, that is on universal capacities that could be exercised in a content-neutral way. Scientific inquiry pursued rigorously, could lead us to accept representations of natural (and perhaps social) phenomena and processes that were free both of the taint of metaphysics and also the taint of social biases such as the racism and sexism that infected much 19th century biology and anthropology.

The equation of value freedom with formal and methodological rigor cuts in several ways. Taking value freedom as an ideal led some of us, feminists, anti-racists, socialists, to question the value freedom of certain actual scientific research programs. Science should be value-free, but it is not. Greater vigilance about biases will correct this defect. (Cf. Hubbard 1979, Gould 1985) This focus led to an examination of research programs concerned with gender and sex and with racial difference, identification of sexist, androcentric, and racist and ethnocentric elements in them, and the development of alternative programs. But some research programs that had socially problematic conclusions did not seem challengeable on grounds that they were methodologically inferior to research not about sex, gender, or race. So the value-free ideal has another face: if impartially pursued, value blind, scientific inquiry produces results that do end up favoring certain groups in society, or that when applied have certain consequences, we must accept those results if they are the result of impartial methods impartially applied. One can see this consequence articulated in the response of advocates of research programs criticized for sexism. (Witelson 1985) If science tells us that women are biologically less well equipped than men to do math, well, that's unfortunate, but so be it.

This kind of attitude, the discovery that some problematic science did not differ from unproblematic science in the relevant ways, and the appeal to science to support social inequalities, are among the factors that stimulated feminist philosophers to investigate the grounds for claiming that science at its best or in its nature is value-free.[†] Some rejected the idea that science is or could be objective; others dug in their heels defending scientific objectivity and rationality.

These investigations opened up by the political moves were extended by investigators in social studies of science who studied the influence of interests and ideology in science. This eventually resulted in a situation called "the science wars" which pitted scholars engaged in social and cultural studies of science against scholars engaged in normative studies of scientific rationality. On the one side were those who claimed that science is all reason and on the other those who claimed that science is all power. Both sides exaggerated their claims and overlooked an alternative:
That the production of scientific knowledge is both governed by normative principles of rationality and embedded in and reflective of its social context.

Rationality, Sociality, Plurality

The science wars are at an impasse. I've argued that the stalemate between the two parties is produced by an acceptance by both sides to the debate of a dichotomous understanding of the cognitive and the social.

[†] Of course, one might take an alternative view and argue that what the sciences proclaim about human differences should have no bearing on social policy, that such policy ought to be determined by our political goals and values, not be transient empirical theories. I agree that there is a good argument to be made for this conclusion, but do not think this precludes an investigation into the grounds for the claims of scientific value-freedom.

Among the components of this dichotomy are two contrasting assumptions about the content of scientific knowledge: monism and non-monism. I understand monism as follows:

For any natural process there is one (and only one) correct account (model, theory) of the process. All correct accounts of natural processes can form part of a single consistent and comprehensive account of the natural world.

Non-monism is often treated as anti-realism of some kind, but there can be eliminativist, constructivist, and realist versions of non-monism. What would a realism that denies that there is or will be one correct and comprehensive account of the natural world be? It would be pluralist realism, which I understand as follows:

For any natural process, there can be more than one correct account (model, theory) of the process. This is especially likely in the case of complex processes. It is not necessary that all correct accounts of natural processes form part of a single consistent account of the natural world. Rather than one complete account, multiple approaches may yield partial and non-reconcilable accounts.

Philosophers of science who advocate pluralism disagree about the grounds for the view and about the precise nature of the pluralist claim. (For different articulations, see Dupre1993, Ereshevsky 1998, Mitchell 1995, Rosenberg 1994, Waters 1991.) Those advocating strong forms of pluralism are claiming that the complexity of natural processes eludes complete representation by any single theoretical or investigative approach available to human cognizers.[‡] Any given approach will be partial and completeness, if achieved at all, will be achieved not by a single integrated theory, but by a plurality of approaches that are partially overlapping, partially autonomous, and that

[‡] My formulation here deliberately equivocates between an ontological and an epistemological articulation.

resist unification. For example, organismic development can be investigated in different ways that each preclude alternative understandings. Insight into the genetic contributions to development is achieved by holding environmental conditions constant. But then one gets no understanding of environmental or other non-genetic factors in development. And vice-versa.

Many philosophical accounts of scientific knowledge are incompatible with such pluralism. They assume as a condition of adequacy of criteria of knowledge that there is one uniquely correct account of the phenomenon to be known. Conversely, a standard criticism of pluralism is that it makes knowledge impossible. But I contend that accounts of knowledge shouldn't presuppose either monism or pluralism. These are metaphysical views. Whether the world is such as to be describable by one model or many is neither a priori nor empirically decidable. So, one of the constraints on the analysis of knowledge ought to be that neither metaphysical position is presupposed. What would such an account look like?

Knowledge as Social

I propose the social account of knowledge as one way to satisfy the constraint. To see how it does so it is useful to start with the central problem to which that account is addressed: the underdetermination problem. The gap between what is presented to us, whether in the kitchen and garden or in the laboratory, and the processes that we suppose produce the world as we experience it, between our data and the theories, models, and hypotheses developed to explain the data, has been at the heart of philosophical reflection about scientific knowledge. As long as the content of theoretical statements is not

represented as generalizations of data or the content of observational statements is not identified with theoretical claims then there is a gap between hypotheses and data and the choice of hypothesis is not fully determined by the data. Dramatic example: claims about collisions and disintegrations of elementary particles and the data available to support such claims. That is, claims about the behavior of pions and muons and the other members of the "particle zoo" are not based on direct observation of the particles themselves, but on phenomena that can be observed – tracks in compressed gas, the sequence of cifres on data tapes. But generally, the correlation of one particular kind of event with another is evidence that one causes the other in light of an assumption that the one kind has or can have a causal influence on the other. For another example, correlation of exposure to or secretion of a particular hormone with a physiological or behavioral phenomenon is evidence that the hormone causes the physiological or behavioral phenomenon in light of an assumption that hormone secretions have a causal or regulative status in the processes in which they are found rather than being epiphenomenal to or effects of those processes. Nor do hypotheses specify the data that will confirm them. Data alone are consistent with different and conflicting hypotheses and require supplementation.

The supplement required to establish a connection between hypotheses and data reports is provided by (background) assumptions. These include substantive and methodological hypotheses that, from one point of view, form the framework within which inquiry is pursued and, from another, structure the domain about which inquiry is pursued. These hypotheses are most often not articulated, but presupposed by the

scientists relying on them. They facilitate the reasoning between what is known and what is hypothesized.

I take the general lesson of underdetermination to be that any empirical reasoning takes place against a background of assumptions that are neither self-evident nor logically true.[§] Such assumptions, or auxiliary hypotheses, are the vehicles by which social values can enter into scientific judgment. If there is no in principle way mechanically or formally to eliminate background assumptions there's no in principle way mechanically or formally to eliminate social values and interests from such judgment. Some sociologists of science, those on one side of the science wars, used versions of the underdetermination problem to argue that epistemological concerns with truth and good reasons are irrelevant to the understanding of scientific inquiry and judgment (Barnes and Bloor 1982; Pickering 1984; Shapin 1994; Collins and Pinch 1993; Knorr-Cetina 1983; Latour 1987, 1993). The point, however, should not be that observation and logic as classically understood are irrelevant, but that they are insufficient. The sociologists' empirical investigations show that they are explanatorily insufficient. The philosophers' underdetermination argument shows that they are epistemically insufficient.

My view is that rather than spelling doom for the epistemological concerns of the philosopher, the logical problem of underdetermination, taken together with the sociologists' studies of laboratory and research practices changes the ground on which philosophical concerns operate. This new ground or problem situation is constituted by 1) treating agents/subjects of knowledge as located in particular and complex

---

[§] This is to say, not that scientists face a gap over which they leap with careless abandon, but that the ways in which the gap between hypotheses and data is closed involves reliance on assumptions that are contestable.

interrelationships and 2) acknowledging that purely logical constraints cannot compel them to accept a particular theory. That network of relationships – with other individuals, social systems, natural objects, and natural processes – is not an obstacle to knowledge, but can be understood as a rich pool of resources – constraints and incentives – to help close the gap left by logic. The philosophical concern with justification is not irrelevant, but must be somewhat reconfigured to be made relevant to scientific inquiry. The reconfiguration I advocate involves treating justification not just as a matter of relations between sentences, statements, or the beliefs and perceptions of an individual, but as a matter of relations within and between communities of inquirers.

In my 1990 book, Science as Social Knowledge, I supported this move by looking at strategies the sciences themselves employ to guard against the intrusion of individual interests and personal or social values into the body of accepted results. (The conventions of peer review, reproducibility of experiments, etc.) To see these as part of scientific method we must expand the notion of justification. This expansion of justification sees it as consisting not just in the testing of hypotheses against data, but also in the subjection of hypotheses, data, reasoning, and background assumptions to criticism from a variety of perspectives. Establishing what the data are, what counts as acceptable reasoning, which assumptions are legitimate and which not becomes in this view a matter of social, discursive, interactions as much as a matter of interaction with the material world. Since assumptions are, by their nature, usually not explicit, but taken for granted ways of thinking, the function of critical interaction is to make them visible as well as to examine their metaphysical, empirical, and normative implications.

The point is not that sociality provides guarantees of the sort that formal connections were thought to provide in older conceptions of confirmation, but that cognitive practices have social dimensions – that human knowledge is made possible by our interdependence, not or not just by individual cognitive efforts simply added together. Acknowledging this social dimension has two consequences. In the first place, any normative rules or conditions for scientific inquiry must include conditions applying to social interactions in addition to conditions applying to observation and reasoning. A full account of justification or objectivity must spell out conditions that a community must meet for its discursive interactions to constitute effective criticism. [I have proposed that establishing or designating appropriate venues for criticism, uptake of criticism (i.e. response and change), public standards that regulate discursive interaction, and what I now call tempered equality of intellectual authority, are conditions that make effective or transformative criticism possible (Longino 2002a, pp. 128-135). The public standards include aims and goals of research, background assumptions, methodological stipulations, ethical guidelines, and so on. Such standards regulate critical interaction in the sense of serving to delimit what will count as legitimate criticism. They are, thus, invoked in different forms of critical discussion, but most importantly, they are themselves subject to critical scrutiny. Their status as regulative principles in some community depends on their continuing to serve the cognitive aims of that community. The conditions of effective or transformative criticism may not be the conditions ultimately settled on, but what I do contend is that something like them [conditions that establish the effectiveness of critical interaction] must be added to the set of methodological norms.]

Secondly, even though a community may operate with effective structures that block the spread of idiosyncratic assumptions, those assumptions that are shared by all members of a community will not only be shielded from criticism, but, because they persist in the face of effective structures, may even be reinforced. One obvious solution is to require interaction across communities, or at least to require openness to criticism both from within and from outside the community. Here, of course, availability is a strong constraint. Other communities that might be able to demonstrate the non self-evidence of shared assumptions or to provide new critical perspectives may be too distant – spatially or temporally – for contact. Background assumptions then are only provisionally legitimated; no matter how thorough their scrutiny given the critical resources available at any given time, it is possible that scrutiny at a later time will prompt reassessment and rejection. Such reassessment may be the consequence not only of interaction with new communities but also of changes in standards within a community. These observations suggest a distinction between a narrow sense of justification and a broad sense. Justification in a narrow sense would consist in survival of critical scrutiny relative to all perspectives available within the community, while justification in a broad or inclusive sense would consist in survival of critical scrutiny relative to all perspectives within and without the community.[**]

[**] Using this social account of justification one might then say: Some content A (a theory, model, hypothesis, observation report) is epistemically acceptable in community C at time *t* if A is supported by data *d* evident to C at *t* in light of reasoning and background assumptions which have survived critical scrutiny from as many perspectives as are available to C at *t*, and the discursive structures of C satisfy the conditions for effective criticism. In Longino (2002a, pp. 135-140), I use this notion of epistemic acceptability to provide accounts of epistemological concepts.

Background assumptions are, along with values and aims of inquiry, the public standards that regulate the discursive and material interactions of a community. The point here is that they are both provisional and subordinated to the overall goal of inquiry for a community. Truth *simpliciter* cannot be such a goal, since it's not sufficient to direct inquiry. Rather, communities seek particular kinds of truths. (They seek revealing representations, relevant explanations, accurate predictions, realizable technological recipes for use in construction of novel entities, etc. Researchers in biological communities seek truths about the development of individual organisms, about the history of lineages, about the physiological functioning of organisms, about the mechanics of parts of organisms, about the interactions of organisms, about molecular interactions, about ways to produce molecular configurations not found in nature (i.e. transgenic organisms), etc. Research in other areas is similarly organized around specific questions.) Which kinds of truths are sought in any particular research project is determined by the kinds of questions researchers are asking and the purposes for which they ask them, i.e. the uses to which the answers will be put. Truth is not opposed to social values, indeed it <u>is</u> a social value, but its regulatory function is directed/mediated by other social values and assumptions operative in the research context.

The assumptions [background assumptions made visible by the underdetermination argument] that partially constitute this context are of at least two kinds: substantive and methodological. Substantive assumptions concern the character of the world one is investigating. They may be compositional or processual. An example of the former is the assumption that the material world is constituted of particles that at the most fundamental level are indivisible. A processual assumption that had a following

recently is the assumption that all biological development is controlled by genes. Methodological assumptions have to do with the means we have of developing and acquiring knowledge. They can range from general philosophical views like the commitment to some form of empiricism to quite particular views about the kinds of data appropriate for kinds of question: field observation versus experiment, animal models versus human studies, in vivo versus in vitro studies, etc. They include assumptions concerning how much data of a certain type should be required, what kinds of data are relevant, and what mix of kinds of data from different kinds of study techniques should be required. They also include what philosophers sometimes call epistemic or cognitive values. The range of methodologies obviously depends in part on the availability of investigative resources, but these are not sufficient to limit methodologies to one.

Given such an array, one can understand investigative, or scientific, communities as constituted around selections of substantive and methodological assumptions suited to the particular questions of the research. To call them selections does not mean that they are deliberately picked out from a possible assortment, but that they represent a subset of possible alternatives. Since there are alternatives, reliance on any one subset must be defended in relation to the cognitive aims of the research. These aims are not just a matter of the individual motivations of the researchers but of the goals and interests of the communities that support and sustain the research.

The possibility of pluralism is a consequence of the possibility of alternative epistemological frameworks consisting of rules of data collection (including standards of relevance and precision, standards of statistical significance, specification of objects and units of measurement),rules for deciding borderline cases, inference principles, and

epistemic or cognitive values, in general, frameworks answerable to different kinds of (empirical) questions about the natural and social worlds. Other philosophers have advanced pluralism as a view about the world, i.e. as the consequence of a natural complexity so deep that no single theory or model can fully capture all the causal interactions involved in any given process. While this may be the case, the epistemological position I am advocating is merely open to pluralism in that it does not presuppose monism. It can be appropriate to speak of knowledge even when there are ways of knowing a phenomenon that cannot be simultaneously embraced within the same framework of understanding. Whether or not it is appropriate in any given case depends on features of the empirical situation and satisfaction of the social conditions of knowledge mentioned above. When these are satisfied, reliance on any particular set of assumptions must be defended in relation to the cognitive aims of the research. These are not just a matter of the individual motivations of the researchers but of the goals and interests of the communities that support and sustain the research. On the social view all of these must be publicly sustained through survival of critical scrutiny. Thus, social values come to play an ineliminable role in certain contexts of scientific judgment.

Values in Science, Again

I maintain that this is an account of scientific knowledge and inquiry (or the basics of one) that both integrates the rational and the social and that avoids begging the question for or against pluralism. As to the first, the philosopher is right to see the sciences as a locus of cognitive rationality; the sociologist or sociologically sensitive historian is right to see the sciences as a locus of social interactions (that are not

containable within the lab or research site). The mistake is to accept a conceptual framework within which these perspectives exclude each other. With respect to the second, we can talk about knowledge of a phenomenon X made possible by one set of methodological commitments and standards guided by a particular question and also about different knowledge of the same phenomenon made possible by a different set. As long as two (or more) incompatible models of X are working in the ways we want (are narrowly or even broadly justified) why not accept that they are latching on to real causal processes in the world, even if these cannot be reconciled into one account or model? Only a prior commitment to monism precludes this, but whether we end up, at that mythical end of inquiry with one true account for each domain or more than one is a matter of how the world is and is neither presupposed nor settled by epistemological reflection.

I would like now to draw some lessons concerning the relation of science and values. The possibility of pluralism that is part of this account has implications for the ideals of both universality and impartiality. Universality is still an ideal but very restricted way – the relation between aims, values, methods and results is open to scrutiny by all, it.is universally accessible. But results taken by themselves are binding only on those sharing cognitive aims and the values in relation to which a given cognitive aim makes sense. What about impartiality? One of the aims of many philosophers of science has been, as I mentioned at the beginning, to show how, in spite of the de facto presence of social (and personal) values and interests, scientific inquiry can nevertheless be cleansed of them. The very possibility of pluralism turns the value-free ideal upside down – values and interests must be addressed not by elimination or purification

strategies, but – and here is the paradox -- by more and different values. To see this, consider the following.

First, suppose pluralism is right, i.e. the world is not such as to be in the end describable by one theory or conjunction of theories. Then, even if a given theory has impeccable evidentiary support, is justified in the narrow sense, that it has problematic or noxious social consequences [i.e. its acceptance would advance or undermine the interests of one or more groups in society relative to others,] is reason not directly to reject it, but to develop an alternative approach that has equivalent empirical validity. (This is not an armchair pursuit; it takes time, effort, and resources). The social payoff is an escape route from natural inevitability arguments. The epistemic payoff is an increase in the range of phenomena that we can know or explain.

If monism is right, if the world <u>is</u> such as to in the end be describable by one theory or conjunction or uniquely domain specific theories, we won't have any reason to believe this unless those theories that belong in the set have been tested against all possible alternatives, so that a theory has noxious consequences is again good reason for one with different values to develop an alternative approach. This will increase the alternatives in play and increase the likelihood that eventually in our fumbling way we will exhaust all possible alternatives and settle on the conjunction of uniquely correct domain specific theories.

Feminist interventions in physical anthropology and primatology since the 1970s constitute a recent classic example of value-driven research that has improved quality of science in those areas. Feminists have brought new phenomena and data to the attention of their disciplines and have drawn new and different connections between phenomena

that were already known to their communities. The point is not that their interpretations replaced the old (although for many researchers, they did). Instead, their interventions stimulated a rethinking of the assumptions that had hitherto been invisible.

Another examples comes from the biological study organismic developments. Most contemporary research in genetics and micro-biology is gene-centered – it focuses on detailing the contribution of genes, particular DNA (or RNA) sequences to biological processes. Critics of this research worry that it reinforces unexamined social values in several ways: 1) its support of biological determinism (the idea that our phenotypic traits, including social behavior, are determined by inborn genetic structures, 2) its reinforcement, through its implicit commitment to linear, single factor, forms of causality, of an oversimplified conception of the natural world and of authoritarian social forms. Other critics worry that it leads us to ignore environmental causes of illness in favor of genetic ones.

A monist interpretation leads proponents of gene=centered approaches to think they are successful because they are based on a comprehensive theory that explains all the essentials of development and it leads critics of gene centered approaches to argue that they are wrong. On the pluralist view, one can see genetic and environmental approaches as correct as far as they go, but incomplete. The social concerns of the anti-genetic determinists lead them to pursue research into the other factors that are involved in development, from other elements inside the cell to larger environmental factors. Values keep the consensus from closing around a very partial picture.

In agricultural science, commitments to certain values sustain an alternative to the biotech intensive efforts of the large agribusiness corporations. Philosopher Hugh Lacey

has identified what he calls social-cultural nexa informing different research approaches: Agro-ecological strategies, characterized by an interest in preserving sustainability of environmental and cultural systems, and materialist strategies characterized by an interest in control of natural objects. The dominance of the latter, he argues, is not a function of its doing better, more successful, science, but of doing science that is compatible with a set of ascendant social values. The persistence of sustainability goals again keeps an alternative line of research open.

Conclusion

The ideal of value freedom was advanced because it was thought that value-free science could best ensure impartial (i.e. unbiased, socially neutral) science and universally valid science, i.e. results that would hold for anyone, anywhere. This has led individual investigators to suppose they must keep their own values out of the laboratory and that doing so would be sufficient to guarantee value-free, impartial, science. I've suggested that the conception of inquiry on which this thought is based is untenable. Science as practiced at the individual level is fragile and vulnerable both to the corruption described at the beginning and to the inevitable influence of background assumptions. The alternative, social, account of knowledge indicates that the objectives of the value-free ideal are better achieved if the constructive role of values is appreciated and the community structured to permit their critical examination. Furthermore, interdependence requires diversity – and here is an additional paradox – acknowledging interdependence and sociality in the production of knowledge, frees the individual scientist to explore new

avenues of analysis and interpretation – to be playful rather than grim in the pursuit of knowledge.

Finally, acknowledging the constructive as well as corroding role of values, acknowledging plurality, and strengthening institutional practices and structures to include open discussion of the social values at stake, will ensure that the sciences continue to merit public trust instead of becoming just another interest group.

Structuring the community to include multiple perspectives and values will do more to advance the aims in relation to which value-free science was an ideal – impartiality and universality – than appeals to narrow methodology ever could.