How to Be a Moral Realist

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1
INTRODUCTION

1.1
Moral realism

Scientific realism is the doctrine that scientific theories should be understood as putative descriptions of real phenomena, that ordinary scientific methods constitute a reliable procedure for obtaining and improving (approximate) knowledge of the real phenomena which scientific theories describe, and that the reality described by scientific theories is largely independent of our theorizing. Scientific theories describe reality and reality is "prior to thought" (see Boyd 1982).

By "moral realism" I intend the analogous doctrine about moral judgments, moral statements, and moral theories. According to moral realism:

1 Moral statements are the sorts of statements which are (or which express propositions which are) true or false (or approximately true, largely false, etc.);
2 The truth or falsity (approximate truth…) of moral statements is largely independent of our moral opinions, theories, etc.;
3 Ordinary canons of moral reasoning—together with ordinary canons of scientific and everyday factual reasoning—constitute, under many circumstances at least, a reliable method for obtaining and improving (approximate) moral knowledge.

It follows from moral realism that such moral terms as "good", "fair", "just", "obligatory" usually correspond to real properties or relations and that our ordinary standards for moral reasoning and moral disputation—together with reliable standards for scientific and everyday reasoning—constitute a fairly

reliable way of finding out which events, persons, policies, social arrangements, etc. have these properties and enter into these relations. It is *not* a consequence of moral realism that our ordinary procedures are "best possible" for this purpose—just as it is not a consequence of scientific realism that our existing scientific methods are best possible. In the scientific case, improvements in knowledge can be expected to produce improvements in method (Boyd 1980, 1982, 1983, 1985a, 1985b, 1985c), and there is no reason to exclude this possibility in the moral case.

Scientific realism contrasts with instrumentalism and its variants and with views like that of Kuhn (1970) according to which the reality which scientists study is largely constituted by the theories they adopt. Moral realism contrasts with non-cognitivist metaethical theories like emotivism and with views according to which moral principles are largely a reflection of social constructs or conventions.

What I want to do in this essay is to explore the ways in which recent developments in realist philosophy of science, together with related "naturalistic" developments in epistemology and philosophy of language, can be employed in the articulation and defense of moral realism. It will not be my aim here to establish that moral realism is true. Indeed, if moral realism is to be defended along the lines I will indicate here then a thoroughgoing defense of moral realism would be beyond the scope of a single essay. Fortunately a number of extremely important defenses of moral realism have recently been published (see, e.g., Brink 1984, forthcoming; Gilbert 1981b, 1982, 1984b, 1986b, forthcoming; Miller 1984b; Railton 1986; Sturgeon 1984a, 1984b). What I hope to demonstrate in the present essay is that moral realism can be shown to be a more attractive and plausible philosophical position if recent developments in realist philosophy of science are brought to bear in its defense. I intend the general defense of moral realism offered here as a proposal regarding the metaphysical, epistemological, and semantic framework within which arguments for moral realism are best formulated and best understood.

In addition, I am concerned to make an indirect contribution to an important recent debate among Marxist philosophers and Marx scholars concerning the Marxist analysis of moral discourse (see, e.g., Gilbert 1981a, 1981b, 1982, 1984b, 1986a, 1986b; Miller 1979, 1981, 1982, 1983, 1984a, 1984b; Wood 1972, 1979). Two questions are central in this debate: the question of what metaethical views Marx and other Marxist figures actually held or practiced and the question of what metaethical views are appropriate to a Marxist analysis of history and in particular to a Marxist analysis of the role of class ideology in the determination of the content of moral conceptions. I have nothing to contribute to the efforts to answer the first question, which lies outside my competence. About the second, I am convinced that Marxists should be moral realists and that the admirably motivated decision by many antirevisionist Marxists to adopt a nonrealist relativist stance in metaethics represents a sectarian (if nonculpable) error. I intend the defense of moral realism presented here to be fully compatible

with the recognition of the operation in the history of moral inquiry of just the sort of ideological forces which Marxist historians (among others) have emphasized. A thoroughgoing defense of this compatibility claim is not attempted in the present essay; I develop it in a forthcoming essay.

1.2
Scientific knowledge and moral skepticism

One of the characteristic motivations for anti-realistic metaethical positions — either for non-cognitivist views or for views according to which moral knowledge has a strong constructive or conventional component—lies in a presumed epistemological contrast between ethics, on the one hand, and the sciences, on the other. Scientific methods and theories appear to have properties —objectivity, value-neutrality, empirical testability, for example– which are either absent altogether or, at any rate, much less significant in the case of moral beliefs and the procedures by which we form and criticize them. These differences make the methods of science (and of everyday empirical knowledge) seem apt for the *discovery* of facts while the 'methods' of moral reasoning seem, at best, to be appropriate for the rationalization, articulation, and application of preexisting social conventions or individual preferences.

Many philosophers would like to explore the possibility that scientific beliefs and moral beliefs are not so differently situated as this presumed epistemological contrast suggests. We may think of this task as the search for a conception of "unified knowledge" which will bring scientific and moral knowledge together within the same analytical framework in much the same way as the positivists' conception of "unified science" sought to provide an integrated treatment of knowledge within the various special sciences. There are, roughly, two plausible general strategies for unifying scientific and moral knowledge and minimizing the apparent epistemological contrast between scientific and moral inquiry:

1 Show that our scientific beliefs and methods actually possess many of the features (e.g., dependence on nonobjective "values" or upon social conventions) which form the core of our current picture of moral beliefs and methods of moral reasoning.
2 Show that moral beliefs and methods are much more like our current conception of scientific beliefs and methods (more "objective", "external", "empirical", "intersubjective", for example) than we now think.

The first of these options has already been explored by philosophers who subscribe to a "constructivist" or neo-Kantian conception of scientific theorizing (see, e.g., Hanson 1958; Kuhn 1970). The aim of the present essay will be to articulate and defend the second alternative. In recent papers (Boyd 1979, 1982, 1983, 1985a, 1985b, 1985c) I have argued that scientific realism is correct, but that its adequate defense requires the systematic adoption of a distinctly

naturalistic and realistic conception of knowledge, of natural kinds, and of reference. What I hope to show here is that once such a distinctly naturalistic and realistic conception is adopted, it is possible to offer a corresponding defense of moral realism which has considerable force and plausibility.

My argumentative strategy will be to offer a list of several challenges to moral realism which will, I hope, be representative of those considerations which make it plausible that there is the sort of epistemological contrast between science and ethics which we have been discussing. Next, I will present a summary of some recent work in realistic philosophy of science and related "naturalistic" theories in epistemology and the philosophy of language. Finally, I will indicate how the results of this recent realistic and naturalistic work can be applied to rebut the arguments against moral realism and to sketch the broad outlines of an alternative realistic conception of moral knowledge and of moral language.

2
SOME CHALLENGES TO MORAL REALISM

2.1
Moral intuitions and empirical observations

In the sciences, we decide between theories on the basis of observations, which have an important degree of objectivity. It appears that in moral reasoning, moral intuitions play the same role which observations do in science: we test general moral principles and moral theories by seeing how their consequences conform (or fail to conform) to our moral intuitions about particular cases. It appears that it is the foundational role of observations in science which makes scientific objectivity possible. How could moral intuitions possibly play the same sort of foundational role in ethics, especially given the known diversity of moral judgments between people? Even if moral intuitions do provide a "foundation" for moral inquiry, wouldn't the fact that moral "knowledge" is grounded in intuitions rather than in observation be exactly the sort of fundamental epistemological contrast which the received view postulates, especially since peoples' moral intuitions typically reflect the particular moral theories or traditions which they already accept, or their culture, or their upbringing? Doesn't the role of moral intuitions in moral reasoning call out for a "constructivist" metaethics? If moral intuitions don't play a foundational role in ethics and if morality is supposed to be epistemologically like science, *then what plays, in moral reasoning, the role played by observation in science*?

2.2
The role of "reflective equilibrium" in moral reasoning

We have already seen that moral intuitions play a role in moral reasoning which appears to threaten any attempt to assimilate moral reasoning to the model of objective empirical scientific methodology. Worse yet, as Rawls (1971) has reminded us, what we do with our moral intuitions, our general moral principles, and our moral theories, in order to achieve a coherent moral position, is to engage in "trading-off between these various categories of moral belief in order to achieve a harmonious "equilibrium". Moral reasoning *begins* with moral *presuppositions,* general as well as particular, and proceeds by negotiating between conflicting *presuppositions*. It is easy to see how this could be a procedure for rationalization of individual or social norms or, to put it in more elevated terms, a procedure for the "construction" of moral or ethical systems. But if ethical beliefs and ethical reasoning are supposed to be like scientific beliefs and methods, then this procedure would have to be a procedure for *discovering* moral facts! How could any procedure so presupposition-dependent be a *discovery* procedure rather than a *construction procedure*? (See Dworkin 1973.)

2.3
Moral progress and cultural variability

If moral judgments are a species of factual judgment, then one would expect to see moral progress, analogous to progress in science. Moreover, one of the characteristics of factual inquiry in science is its relative independence from cultural distortions: scientists with quite different cultural backgrounds can typically agree in assessing scientific evidence. If moral reasoning is reasoning about objective moral *facts,* then what explains our lack of progress in ethics and the persistence of cultural variability in moral beliefs?

2.4
Hard cases

If goodness, fairness, etc. are real and objective properties, then what should one say about the sorts of hard cases in ethics which we can't seem *ever* to resolve? Our experience in science seems to be that hard scientific questions are only *temporarily* rather than permanently unanswerable. Permanent disagreement seems to be very rare indeed. Hard ethical questions seem often to be permanent rather than temporary.

In such hard ethical cases, is there a fact of the matter inaccessible to moral inquiry? If so, then doesn't the existence of such facts constitute a significant epistemological difference between science and ethics? If not, if there are not facts of the matter, then isn't moral realism simply refuted by such indeterminacy?

2.5
Naturalism and naturalistic definitions

If goodness, for example, is a real property, then wouldn't it be a *natural* property? If not, then isn't moral realism committed to some unscientific and superstitious belief in the existence of non-natural properties? If goodness would be a natural property, then isn't moral realism committed to the extremely implausible claim that moral terms like "good" possess naturalistic definitions?

2.6
Morality, motivation, and rationality

Ordinary factual judgments often provide us with reasons for action; they serve as constraints on rational choice. But they do so only because of our antecedent interests or desires. If moral judgments are merely factual judgments, as moral realism requires, then the relation of moral judgments to motivation and rationality must be the same. It would be possible in principle for someone, or some thinking thing, to be entirely rational while finding moral judgments motivationally neutral and irrelevant to choices of action.

If this consequence follows from moral realism, how can the moral realist account for the particularly close connection between moral judgments and judgments about what to do? What about the truism that moral judgments have commendatory force as a matter of their meaning or the plausible claim that the moral preferability of a course of action always provides a reason (even if not an overriding one) for choosing it?

2.7
The semantics of moral terms

Moral realism is an anti-subjectivist position. There is, for example, supposed to be a single objective property which we're all talking about when we use the term "good" in moral contexts. But people's moral concepts differ profoundly. How can it be maintained that our radically different concepts of "good" are really concepts of one and the same property? Why not a different property for each significantly different conception of the good? Don't the radical differences in our conceptions of the good suggest either a non-cognitivist or a constructivist conception of the semantics of ethical terms?

2.8
Verificationism and anti-realism in ethics

Anti-realism in ethics, like the rejection of theoretical realism in science, is a standard positivist position. In the case of science, there is a straightforward verificationist objection to realism about alleged "theoretical entities": they are

unobservables; statements about them lie beyond the scope of empirical investigation and are thus unverifiable in principle. (See Boyd 1982 for a discussion of various formulations of this key verificationist argument.)

It is interesting to note that the challenges to moral realism rehearsed in 2.1–2.7 do not take the form of so direct an appeal to verificationism. Only in the case of the concern about non-natural moral properties (2.5) might the issue of verifiability be directly relevant, and then only if the objection to non-natural properties is that they would be unobservable. Instead, the arguments in 2.1–2.7 constitute an *indirect* argument against moral realism: they point to features of moral beliefs or of moral reasoning for which, it is suggested, the best explanation would be one which entailed the rejection of moral realism. Moreover, what is true of the challenges to moral realism rehearsed above is typical: by and large positivists, and philosophers influenced by positivism, did not argue directly for the unverifiability of moral statements; they did not make an appeal to the unobservability of alleged moral properties or deny that moral theories had observational consequences. Instead, they seemed to take a non-cognitivist view of ethics to be established by an "inductive inference to the best explanation" of the sort of facts cited in 2.1–2.7.

In this regard, then, the standard arguments against moral realism are more closely analogous to Kuhnian objections to scientific realism than they are to the standard verificationist arguments against the possibility of knowledge of "theoretical entities." Sections 2.1, 2.2, 2.3, and 2.7 rehearse arguments which are importantly similar to Kuhn's arguments from the paradigm dependence of scientific concepts and methods to a constructivist and anti-realistic conception of science. I have argued elsewhere (Boyd 1979, 1982, 1983, 1985a) that a systematic rebuttal to the verificationist epistemology and philosophy of language which form the foundations of logical positivism can in fact be extended to a defense of scientific realism against the more constructivist and neo-Kantian considerations represented by Kuhn's work. If the arguments of the present essay are successful, then this conclusion can be generalized: a realist and anti-empiricist account in the philosophy of science can be extended to a defense of moral *realism* as well, even though the challenges to moral realism are apparently only indirectly verificationist.

3
REALIST PHILOSOPHY OF SCIENCE

3.1
The primacy of reality

By "scientific realism" philosophers mean the doctrine that the methods of science are capable of providing (partial or approximate) knowledge of unobservable ("theoretical") entities, such as atoms or electromagnetic fields, in

addition to knowledge about the behavior of observable phenomena (and of course, that the properties of these and other entities studied by scientists are largely theory-independent).

Over the past three decades or so, philosophers of science within the empiricist tradition have been increasingly sympathetic toward scientific realism and increasingly inclined to alter their views of science in a realist direction. The reasons for this realist tendency lie largely in the recognition of the extraordinary role which theoretical considerations play in actual (and patently successful) scientific practice. To take the most striking example, scientists routinely modify or extend operational "measurement" or "detection" procedures for "theoretical" magnitudes or entities on the basis of new theoretical developments. This sort of methodology is perfectly explicable on the realist assumption that the operational procedures in question really are procedures for the measurement or detection of unobservable entities and that the relevant theoretical developments reflect increasingly accurate knowledge of such "theoretical" entities. Accounts of the reusability of operational procedures which are compatible with a non-realist position appear inadequate to explain the way in which theory-dependent revisions of "measurement" and "detection" procedures make a positive methodological contribution to the progress of science.

This pattern is quite typical: The methodological contribution made by theoretical considerations in scientific methodology is inexplicable on a non-realist conception but easily explicable on the realist assumption that such considerations are a reflection of the growth of *theoretical* knowledge. (For a discussion of this point see Boyd 1982, 1983, 1985a, 1985b.) Systematic development of this realist theme has produced developments in epistemology, metaphysics, and the philosophy of language which go far beyond the mere rejection of verificationism and which point the way toward a distinctly realist conception of the central issues in the philosophy of science. These developments include the articulation of causal or naturalistic theories of reference (Kripke 1971, 1972; Putnam 1975a; Boyd 1979, 1982), of measurement (Byerly and Lazara 1973), of "natural kinds" and scientific categories (Quine 1969a; Putnam 1975a; Boyd 1979, 1982, 1983, 1985b), of scientific epistemology generally (Boyd 1972, 1979, 1982, 1983, 1985a, 1985b, 1985c), and of causation (Mackie 1974; Shoemaker 1980; Boyd 1982, 1985b).

Closely related to these developments has been the articulation of causal or naturalistic theories of knowledge (see, e.g., Armstrong 1973; Goldman 1967, 1976; Quine 1969b). Such theories represent generalizations of causal theories of perception and reflect a quite distinctly realist stance with respect to the issue of our knowledge of the external world. What all these developments—both within the philosophy of science and in epistemology generally—have in common is that they portray as a posteriori and contingent various matters (such as the operational "definitions" of theoretical terms, the "definitions" of natural kinds, or the reliability of the senses) which philosophers in the modern tradition have typically sought to portray as a priori. In an important sense, these developments

represent the fuller working out of the philosophical implications of the realist doctrine that reality is prior to thought. (For a further development of this theme see Boyd 1982, 1983, 1985a, 1985b.) It is just this a posteriority and contingency in philosophical matters, I shall argue, which will make possible a plausible defense of moral realism against the challenges outlined in part 2.

In the remaining sections of part 3 I will describe some of the relevant features of these naturalistic and realistic developments. These "results" in recent realistic philosophy are not, of course, uncontroversial, and it is beyond the scope of this essay to defend them. But however much controversy they may occasion, unlike moral realism, they do not occasion incredulity: they represent a plausible and defensible philosophical position. The aim of this essay is to indicate that, if we understand the relevance of these recent developments to issues in moral philosophy, then moral realism should, though controversial, be equally credible.

3.2
Objective knowledge from theory-dependent methods

I suggested in the preceding section that the explanation for the movement toward realism in the philosophy of science during the past two or three decades lies in the recognition of the extraordinarily theory-dependent character of scientific methodology and in the inability of any but a realist conception of science to explain why so theory-dependent a methodology should be reliable. The theoretical reusability of measurement and detection procedures, I claimed, played a crucial role in establishing the plausibility of a realist philosophy of science.

If we look more closely at this example, we can recognize two features of scientific methodology which are, in fact, quite general. In the first place, the realist's account of the theoretical reusability of measurement and detection procedures rests upon a conception of scientific research as *cumulative by successive approximations to the truth*.

Second, this cumulative development is possible because *there is a dialectical relationship between current theory and the methodology for its improvement*. The approximate truth of current theories explains why our existing measurement procedures are (approximately) reliable. That reliability, in turn, helps to explain why our experimental or observational investigations are successful in uncovering new theoretical knowledge, which, in turn, may produce improvements in experimental techniques, etc.

These features of scientific methodology are *entirely* general. Not only measurement and detection procedures but all aspects of scientific methodology- principles of experimental design, choices of research problems, standards for the assessment of experimental evidence, principles governing theory choice, and rules for the use of theoretical language—are highly dependent upon current theoretical commitments (Boyd 1972, 1973, 1979, 1980, 1982, 1983, 1985a, 1985b; Kuhn 1970; van Fraassen 1980). No aspect of scientific method involves

the "presupposition-free" testing of individual laws or theories. Moreover, the theory dependence of scientific methodology *contributes* to its reliability rather than detracting from it.

The only scientifically plausible explanation for the reliability of a scientific methodology which is so theory-dependent is a thoroughgoingly realistic explanation: Scientific methodology, dictated by currently accepted theories, is reliable at producing further knowledge precisely *because, and to the extent that, currently accepted theories are relevantly approximately true*. For example, it is because our current theories are approximately true that the canons of experimental design which they dictate are appropriate for the rigorous testing of new (and potentially more accurate) theories. What the scientific method provides is a paradigm-dependent paradigm-modification strategy: a strategy for modifying or amending our existing theories in the light of further research, which is such that its methodological principles at any given time will themselves depend upon the theoretical picture provided by the currently accepted theories. If the body of accepted theories is itself relevantly sufficiently approximately true, then this methodology operates to produce a subsequent dialectical improvement both in our knowledge of the world and in our methodology itself. Both our new theories and the methodology by which we develop and test them depend upon previously acquired theoretical knowledge. It is not possible to explain even the instrumental reliability of actual scientific practice without invoking this explanation and without adopting a realistic conception of scientific knowledge (Boyd 1972, 1973, 1979, 1982, 1983, 1985a, 1985b, 1985c).

The way in which scientific methodology is theory-dependent dictates that we have a strong methodological preference for new theories which are plausible in the light of our existing theoretical commitments; this means that we prefer new theories which relevantly resemble our existing theories (where the determination of the relevant respects of resemblance is itself a theoretical issue). The reliability of such a methodology is explained by the approximate truth of existing theories, and one consequence of this explanation is that *judgments of theoretical plausibility are evidential*. The fact that a proposed theory is itself plausible in the light of previously confirmed theories is evidence for its (approximate) truth (Boyd 1972, 1973, 1979, 1982, 1983, 1985a, 1985b, 1985c). A purely conventionalistic account of the methodological role of considerations of theoretical plausibility cannot be adequate because it cannot explain the contribution which such considerations make to the instrumental reliability of scientific methodology (Boyd 1979, 1982, 1983).

The upshot is this: The theory-dependent conservatism of scientific methodology is *essential* to the rigorous and reliable testing and development of new scientific theories; on balance, theoretical "presuppositions" play neither a destructive nor a conventionalistic role in scientific methodology. They are essential to its reliability. If by the "objectivity" of scientific methodology we mean its capacity to lead to the discovery of *theory-independent reality,* then

scientific methodology is objective precisely because it *is theory-dependent* (Boyd 1979, 1982, 1983, 1985a, 1985b, 1985c).

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3.3

Naturalism and radical contingency in epistemology

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Modern epistemology has been largely dominated by positions which can be characterized as "foundationalist": all knowledge is seen as ultimately grounded in certain foundational beliefs which have an epistemically privileged position— they are a priori or self-warranting, incorrigible, or something of the sort. Other true beliefs are instances of knowledge only if they can be justified by appeals to foundational knowledge. Whatever the nature of the foundational beliefs, or whatever their epistemic privilege is supposed to consist in, it is an a priori question which beliefs fall in the privileged class. Similarly, the basic inferential principles which are legitimate for justifying non-foundational knowledge claims, given foundational premises, are such that they can be identified a priori and it can be shown a priori that they are rational principles of inference. We may fruitfully think of foundationalism as consisting of two parts, *premise foundationalism,* which holds that all knowledge is justifiable from an a priori specifiable core of foundational beliefs, and *inference foundationalism,* which holds that the principles of justifiable inference are ultimately reducible to inferential principles which can be shown a priori to be rational.

Recent work in "naturalistic epistemology" or "causal theories of knowing" (see, e.g., Armstrong 1973; Goldman 1967, 1976; Quine 1969b) strongly suggest that the foundationalist conception of knowledge is fundamentally mistaken. For the crucial case of perceptual knowledge, there seem to be (in typical cases at least) neither premises (foundational or otherwise) nor inferences; instead, perceptual knowledge obtains when perceptual beliefs are produced by epistemically reliable mechanisms. For a variety of other cases, even where premises and inferences occur, it seems to be the reliable production of belief that distinguishes cases of knowledge from other cases of true belief. A variety of naturalistic considerations suggests that there are no beliefs which are epistemically privileged in the way foundationalism seems to require.

I have argued (see Boyd 1982, 1983, 1985a, 1985b, 1985c) that the defense of scientific realism requires an even more thoroughgoing naturalism in epistemology and, consequently, an even more thoroughgoing rejection of foundationalism. In the first place, the fact that scientific knowledge grows cumulatively by successive approximation and the fact that the evaluation of theories is an ongoing social phenomenon require that we take the crucial causal notion in epistemology to be reliable *regulation* of belief rather than reliable belief *production*. The relevant conception of belief regulation must reflect the approximate social and dialectical character of the growth of scientific knowledge. It will thus be true that the causal mechanisms relevant to knowledge will include mechanisms, social and technical as well as psychological, for the

criticism, testing, acceptance, modification, and transmission of scientific theories and doctrines. For that reason, an understanding of the role of social factors in science may be relevant not only for the sociology and history of science but for the epistemology of science as well. The epistemology of science is in this respect dependent upon empirical knowledge.

There is an even more dramatic respect in which the epistemology of science rests upon empirical foundations. All the significant methodological principles of scientific inquiry (except, perhaps, the rules of deductive logic, but see Boyd 1985c) are profoundly theory-dependent. They are a reliable guide to the truth *only* because, and to the extent that, the body of background theories which determines their application is relevantly approximately true. The rules of rational scientific inference are not reducible to some more basic rules whose reliability as a guide to the truth is independent of the truth of background theories. Since it is a contingent empirical matter which background theories are approximately true, the rationality of scientific principles of inference ultimately rests on a contingent matter of empirical fact, just as the epistemic role of the senses rests upon the contingent empirical fact that the senses are reliable detectors of external phenomena. Thus inference foundationalism is radically false; there are no a priori justifiable rules of nondeductive inference. The epistemology of empirical science is an empirical science. (Boyd 1982, 1983, 1985a, 1985b, 1985c.)

One consequence of this radical contingency of scientific methods is that the emergence of scientific rationality as we know it depended upon the logically, epistemically, and historically contingent emergence of a relevantly approximately true theoretical tradition. It is not possible to understand the initial emergence of such a tradition as the consequence of some more abstractly conceived scientific or rational methodology which itself is theory-independent. There is no such methodology. We must think of the establishment of the corpuscular theory of matter in the seventeenth century as the beginning of rational methodology in chemistry, not as a consequence of it (for a further discussion see Boyd 1982).

3.4
Scientific intuitions and trained judgment

Both noninferential perceptual judgments and elaborately argued explicit inferential judgments in theoretical science have a purely contingent a posteriori foundation. Once this is recognized, it is easy to see that there are methodologically important features of scientific practice which are intermediate between noninferential perception and explicit inference. One example is provided by what science textbook authors often refer to as "physical intuition", "scientific maturity", or the like. One of the intended consequences of professional training in a scientific discipline (and other disciplines as well) is that the student acquire a "feel" for the issues and the actual physical materials which the science

studies. As Kuhn (1970) points out, part of the role of experimental work in the training of professional scientists is to provide such a feel for the paradigms or "worked examples" of good scientific practice. There is very good reason to believe that having good physical (or biological or psychological) intuitions is important to epistemically reliable scientific practice. It is also quite clear both that the acquisition of good scientific intuitions depends on learning explicit theory, as well as on other sorts of training and practice, *and* that scientists are almost never able to make fully explicit the considerations which play a role in their intuitive judgments. The legitimate role of such "tacit" factors in science has often been taken (especially by philosophically inclined scientists) to be an especially puzzling feature of scientific methodology.

From the perspective of the naturalistic epistemology of science, there need be no puzzle. It is, of course, a question of the very greatest psychological interest just how intuitive judgments in science work and how they are related to explicit theory, on the one hand, and to experimental practice, on the other. But it seems overwhelmingly likely that scientific intuitions should be thought of as trained judgments which resemble perceptual judgments in not involving (or at least not being fully accounted for by) explicit inferences, but which resemble explicit inferences in science in depending for their reliability upon the relevant approximate truth of the explicit theories which help to determine them. This dependence upon the approximate truth of the relevant background theories will obtain even in those cases (which may be typical) in which the tacit judgments reflect a deeper understanding than that currently captured in explicit theory. It is an important and exciting fact that some scientific knowledge can be represented tacitly before it can be represented explicitly, but this fact poses no difficulty for a naturalistic treatment of scientific knowledge. Tacit or intuitive judgments in science are reliable because they are grounded in a theoretical tradition (itself partly tacit) which is, as a matter of contingent empirical fact, relevantly approximately true.

3.5
Non-Humean conceptions of causation and reduction

The Humean conception of causal relations according to which they are analyzable in terms of regularity, correlation, or deductive subsumability under laws is defensible only from a verificationist position. If verificationist criticisms of talk about unobservables are rejected—as they should be— then there is nothing more problematical about talk of causal powers than there is about talk of electrons or electromagnetic fields. There is no reason to believe that causal terms have definitions (analytic or natural) in noncausal terms. Instead, "cause" and its cognates refer to natural phenomena whose analysis is a matter for physicists, chemists, psychologists, historians, etc., rather than a matter of conceptual analysis. In particular, it is perfectly legitimate—as a naturalistic conception

of epistemology requires—to employ unreduced causal notions in philosophical analysis. (Boyd, 1982, 1985b; Shoemaker 1980).

One crucial example of the philosophical application of such notions lies in the analysis of "reductionism". If a materialist perspective is sound, then *in some sense* all natural phenomena are "reducible" to basic physical phenomena. The (prephilosophically) natural way of expressing the relevant sort of reduction is to say that all substances are composed of purely physical substances, all forces are composed of physical forces, all causal powers or potentialities are realized in physical substances and their causal powers, etc. This sort of analysis freely employs unreduced causal notions. If it is "rationally reconstructed" according to the Humean analysis of such notions, we get the classic analysis of reduction in terms of the syntactic reducibility of the theories in the special sciences to the laws of physics, which in turn dictates the conclusion that all natural properties must be definable in the vocabulary of physics. Such an analysis is entirely without justification from the realistic and naturalistic perspective we are considering. Unreduced causal notions are philosophically acceptable, and the Humean reduction of them mistaken. The prephilosophically natural analysis of reduction is also the philosophically appropriate one. In particular, purely physical objects, states, properties, etc. need not have definitions in "the vocabulary of physics" or in any other reductive vocabulary (see Boyd 1982).

3.6
Natural definitions

Locke speculates at several places in Book IV of the *Essay* (see, e.g., IV, iii, 25) that when kinds of substances are defined by "nominal essences", as he thinks they must be, it will be impossible to have a general science of, say, chemistry. The reason is this: nominal essences define kinds of substance in terms of sensible properties, but the factors which govern the behavior (even the observable behavior) of substances are insensible corpuscular real essences. Since there is no reason to suppose that our nominal essences will correspond to categories which reflect uniformities in microstructure, there is no reason to believe that kinds defined by nominal essences provide a basis for obtaining general knowledge of substances. Only if we could sort substances according to their hidden real essences would systematic general knowledge of substances be possible.

Locke was right. Only when kinds are defined by natural rather than conventional definitions is it possible to obtain sound scientific explanations (Putnam 1975a; Boyd 1985b) or sound solutions to the problem of "projectibility" in inductive inference in science (Quine 1969a; Boyd 1979, 1982, 1983, 1985a, 1985b, 1985c). Indeed this is true not only for the definitions of natural kinds but also for the definitions of the properties, relations, magnitudes, etc. to which we must refer in sound scientific reasoning. In particular, a wide variety of terms do not possess analytic or stipulative definitions and are instead

defined in terms of properties, relations, etc. which render them appropriate to particular sorts of scientific or practical reasoning. In the case of such terms, proposed definitions are always in principle revisable in the light of new evidence or new theoretical developments. Similarly, the fact that two people or two linguistic communities apply different definitions in using a term is not, by itself, sufficient to show that they are using the term to refer to different kinds, properties, etc.

3.7
Reference and epistemic access

If the traditional empiricist account of definition by nominal essences (or "operational definitions" or "criterial attributes") is to be abandoned in favor of a naturalistic account of definitions (at least for some terms) then a naturalistic conception of reference is required for those cases in which the traditional empiricist semantics has been abandoned. Such a naturalist account is provided by recent casual theories of reference (see, e.g., Feigl 1956; Kripke 1972; Putnam 1975a). The reference of a term is established by causal connections of the right sort between the use of the term and (instances of) its referent.

The connection between causal theories of reference and naturalistic theories of knowledge and of definitions is quite intimate: reference is itself an epistemic notion and the sorts of causal connections which are relevant to reference are just those which are involved in the reliable regulation of belief (Boyd 1979, 1982). *Roughly,* and for nondegenerate cases, a term *t* refers to a kind (property, relation, etc.) *k* just in case there exist causal mechanisms whose tendency is to bring it about, over time, that what is predicated of the term *t* will be approximately true of *k* (excuse the blurring of the use-mention distinction). Such mechanisms will typically include the existence of procedures which are approximately accurate for recognizing members or instances of *k* (at least for easy cases) and which relevantly govern the use of *t,* the social transmission of certain relevantly approximately true beliefs regarding *k,* formulated as claims about *t* (again excuse the slight to the use-mention distinction), a pattern of deference to experts on *k* with respect to the use of *t,* etc. (for a fuller discussion see Boyd 1979, 1982). When relations of this sort obtain, we may think of the properties of *k* as regulating the use of *t* (via such causal relations), and we may think of what is said using *t* as providing us with socially coordinated *epistemic access* to *k*; *t* refers to *k* (in nondegenerate cases) just in case the socially coordinated use of *t* provides significant epistemic access to *k,* and not to other kinds (properties, etc.) (Boyd 1979, 1982).

3.8
Homeostatic property-cluster definitions

The sort of natural definition[1] in terms of corpuscular real essences anticipated by Locke is reflected in the natural definitions of chemical kinds by molecular formulas; "water H_2O" is by now the standard example (Putnam 1975a). Natural definitions of this sort specify necessary and sufficient conditions for membership in the kind in question. Recent non-naturalistic semantic theories in the ordinary language tradition have examined the possibility of definitions which do not provide necessary and sufficient conditions in this way. According to various property-cluster or criterial attribute theories, some terms have definitions which are provided by a collection of properties such that the possession of an adequate number of these properties is sufficient for falling within the extension of the term. It is supposed to be a conceptual (and thus an a priori) matter what properties belong in the cluster and which combinations of them are sufficient for falling under the term. Insofar as different properties in the cluster are differently "weighted" in such judgments, the weighting is determined by our concept of the kind or property being defined. It is characteristically insisted, however, that our concepts of such kinds are "open textured" so that there is some indeterminacy in extension *legitimately* associated with property-cluster or criterial attribute definitions. The "imprecision" or "vagueness" of such definitions is seen as a perfectly appropriate feature of ordinary linguistic usage, in contrast to the artificial precision suggested by rigidly formalistic positivist conceptions of proper language use.

I shall argue (briefly) that—despite the philistine antiscientism often associated with "ordinary language" philosophy—the property-cluster conception of definitions provides an extremely deep insight into the possible form of *natural* definitions. I shall argue that there are a number of scientifically important kinds, properties, etc. whose natural definitions are very much like the property-cluster definitions postulated by ordinary-language philosophers (for the record, I doubt that there are any terms whose definitions actually fit the ordinary-language model, because I doubt that there are any significant "conceptual truths" at all). There are natural kinds, properties, etc. whose natural definitions involve a kind of property cluster *together with* an associated indeterminacy in extension. Both the property-cluster form of such definitions and the associated indeterminacy are dictated by the scientific task of employing categories which correspond to inductively and explanatorily relevant causal structures. In particular, the indeterminacy in extension of such natural definitions could not be remedied without rendering the definitions unnatural in the sense of being scientifically misleading. What I believe is that the following sort of situation is commonplace in the special sciences which study complex structurally or functionally characterized phenomena:

1 There is a family F of properties which are "contingently clustered" in nature in the sense that they co-occur in an important number of cases.

2 Their co-occurrence is not, at least typically, a statistical artifact, but rather the result of what may be metaphorically (sometimes literally) described as a sort of *homeostasis*. Either the presence of some of the properties in F tends (under appropriate conditions) to favor the presence of the others, or there are underlying mechanisms or processes which tend to maintain the presence of the properties in $F,$ or both.

3 The homeostatic clustering of the properties in F is causally important: there are (theoretically or practically) important effects which are produced by a conjoint occurrence of (many of) the properties in F together with (some or all of) the underlying mechanisms in question.

4 There is a kind term t which is applied to things in which the homeostatic clustering of most of the properties in F occurs.

5 This t has no analytic definition; rather all or part of the homeostatic cluster F together with some or all of the mechanisms which underlie it provides the natural definition of t. The question of just which properties and mechanisms belong in the definition of t is an a posteriori question—often a difficult theoretical one.

6 Imperfect homeostasis is nomologically possible or actual: some thing may display some but not all of the properties in $F;$ some but not all of the relevant underlying homeostatic mechanisms may be present.

7 In such cases, the relative importance of the various properties in F and of the various mechanisms in determining whether the thing falls under t—if it can be determined at all—is a theoretical rather than a conceptual issue.

8 In cases in which such a determination is possible, the outcome will typically depend upon quite particular facts about the actual operation of the relevant homeostatic mechanisms, about the relevant background conditions, and about the causal efficacy of the partial cluster of properties from E For this reason the outcome, if any, will typically be different in different possible worlds, even when the partial property cluster is the same and even when it is unproblematical that the kind referred to by t in the actual world exists.

9 Moreover, there will be many cases of extensional vagueness which are such that they are not resolvable, even given all the relevant facts and all the true theories. There will be things which display some but not all of the properties in F (and/or in which some but not all of the relevant homeostatic mechanisms operate) such that no rational considerations dictate whether or not they are to be classed under $t,$ assuming that a dichotomous choice is to be made.

10 The causal importance of the homeostatic property cluster F together with the relevant underlying homeostatic mechanisms is such that the kind or property denoted by t is a natural kind in the sense discussed earlier.

11 No refinement of usage which replaces *t* by a significantly less extensionally vague term will preserve the naturalness of the kind referred to. Any such refinement would either require that we treat as important distinctions which are irrelevant to causal explanation or to induction, or that we ignore similarities which are important in just these ways.

The reader is invited to assure herself that 1–11 hold, for example, for the terms "healthy" and "is healthier than." Whether these are taken to be full-blown cases of natural property (relation) terms is not crucial here. They do illustrate almost perfectly the notion of a homeostatic property cluster and the correlative notion of a homeostatic cluster term. It is especially important to see *both* that a posteriori theoretical considerations in medicine can sometimes decide problematical cases of healthiness or of relative healthiness, often in initially counterintuitive ways *and* that nevertheless only highly artificial modifications of the notions of health and relative health could eliminate most or all of the extensional vagueness which they possess. One way to see the latter point is to consider what we would do if, for some statistical study of various medical practices, we were obliged to eliminate most of the vagueness in the notion of relative healthiness even where medical theory was silent. What we would strive to do would be to resolve the vagueness in such a way as not to bias the results of the study—not to favor one finding about the efficacy of medical practices over another. The role of natural kinds is, by contrast, precisely *to bias* (in the pejoratively neutral sense of the term) inductive generalization (Quine 1969a; Boyd 1979, 1981, 1983, 1985a, 1985b). Our concern not to bias the findings reflects our recognition that the resolution of vagueness in question would be unnatural in the sense relevant to this inquiry.

The paradigm cases of natural kinds—biological species—are examples of homeostatic cluster kinds in this sense. The appropriateness of any particular biological species for induction and explanation in biology depends upon the *imperfectly* shared and homeostatically related morphological, physiological, and behavioral features which characterize its members. The definitional role of mechanisms of homeostasis is reflected in the role of interbreeding in the modern species concept; for sexually reproducing species, the exchange of genetic material between populations is thought by some evolutionary biologists to be essential to the homeostatic unity of the other properties characteristic of the species and it is thus reflected in the species definition which they propose (see Mayr 1970). The *necessary* indeterminacy in extension of species terms is a consequence of evolutionary theory, as Darwin observed: speciation depends on the existence of populations which are intermediate between the parent species and the emerging one. Any "refinement" of classification which artificially eliminated the resulting indeterminacy in classification would obscure the central fact about heritable variations in phenotype upon which biological evolution depends. More determinate species categories would be scientifically inappropriate and misleading.

It follows that a consistently developed scientific realism *predicts* indeterminacy for those natural kind or property terms which refer to complex phenomena; such indeterminacy is a necessary consequence of "cutting the world at its (largely theory-independent) joints." Thus consistently developed scientific realism *predicts* that there will be some failures of bivalence for statements which refer to complex homeostatic phenomena (contrast, e.g., Putnam 1983 on "metaphysical realism" and vagueness). Precision in describing indeterminate or "borderline" cases of homeostatic cluster kinds (properties, etc.) consists not in the introduction of artificial precision in the definitions of such kinds but rather in a detailed description of the ways in which the indeterminate cases are like and unlike typical members of the kind (see Boyd 1982 on borderline cases of knowledge, which are themselves homeostatic cluster phenomena).

4
HOW TO BE A MORAL REALIST

4.1
Moral semantics, intuitions, reflective equilibrium, and hard cases

Some philosophical opportunities are too good to pass up. For many of the more abstract challenges to moral realism, recent realistic and naturalistic work in the philosophy of science is suggestive of possible responses in its defense. Thus for example, it has occurred to many philosophers (see, e.g., Putnam 1975b) that naturalistic theories of reference and of definitions might be extended to the analysis of moral language. *If* this could be done successfully *and if* the results were favorable to a realist conception of morals, then it would be possible to reply to several anti-realist arguments. For example, against the objection that wide divergence of moral concepts or opinions between traditions or cultures indicates that, at best, a constructivist analysis of morals is possible, the moral realist might reply that differences in conception or in working definitions need not indicate the absence of shared causally fixed referents for moral terms.

Similarly, consider the objection that a moral realist must hold that goodness is a natural property, and thus commit the "naturalistic fallacy" of maintaining that moral terms possess analytic definitions in, say, physical terms. The moral realist may choose to agree that goodness is probably a physical property but deny that it has any analytic definition whatsoever. If the realist's critique of the syntactic analysis of reductionism in science is also accepted, then the moral realist can deny that it follows from the premise that goodness is a physical property or that goodness has any physical definition, analytic or otherwise.

If the moral realist takes advantage of naturalistic and realistic conceptions in epistemology as well as in semantic theory, other rebuttals to antirealist challenges are suggested. The extent of the potential for rebuttals of this sort can best be recognized if we consider the objection that the role of reflective equilibrium in moral reasoning dictates a constructivist rather than a realist conception of morals. The moral realist might reply that the dialectical interplay of observations, theory, and methodology which, according to the realist, constitutes the *discovery* procedure for scientific inquiry *just* is the method of reflective equilibrium, so that the prevalence of that method in moral reasoning cannot *by itself* dictate a non-realist conception of morals.

If the response just envisioned to the concern over reflective equilibrium is successful, then the defender of moral realism will have established that —in moral reasoning as in scientific reasoning—the role of culturally transmitted presuppositions in reasoning does not necessitate a constructivist (or non-cognitivist) rather than a realist analysis of the subject matter. *If* that is established, then the moral realist might defend the epistemic role of culturally determined intuitions in ethics by treating ethical intuitions on the model of theory-determined intuitions in science, which the scientific realist takes to be examples of epistemically reliable trained judgments.

Finally, if the moral realist is inclined to accept the anti-realist's claim that the existence of hard cases in ethics provides a reason to doubt that there is a moral fact of the matter which determines the answer in such cases (more on this later), then the scientific realist's conclusion that bivalence fails for some statements involving homeostatic cluster kind terms *might* permit the moral realist to reason that similar failures of bivalence for some ethical statements need not be fatal to moral realism.

In fact, I propose to employ just these rebuttals to the various challenges to moral realism I have been discussing. They represent the application of a coherent naturalistic conception of semantics and of knowledge against the challenges raised by the critic of moral realism. But they do not stand any chance of rebutting moral anti-realism unless they are incorporated into a broader conception of morals and of moral knowledge which meets certain very strong constraints. These constraints are the subject of the next section.

4.2
Constraints on a realist conception of moral knowledge

Suppose that a defense of moral realism is to be undertaken along the lines just indicated. What constraints does that particular defensive strategy place on a moral realist's conception of morals and of moral knowledge? Several important constraints are suggested by a careful examination of the realist doctrines in the philosophy of science whose extension to moral philosophy is contemplated.

In the first place, the scientific realist is able to argue that "reflective equilibrium" in science and a reliance on theory-dependent scientific intuitions

are epistemically reliable *only* on the assumption that the theoretical tradition which governs these methodological practices contains theories which are relevantly approximately true. Indeed, the most striking feature of the consistently realistic epistemology of science is the insistence that the epistemic reliability of scientific methodology is contingent upon the establishment of such a theoretical tradition. Moreover, the possibility of offering a realist rather than a constructivist interpretation of reflective equilibrium and of intuition in science rests upon the realist's claim that observations and theory-mediated measurement and detection of "unobservables" in science represent epistemically relevant causal interactions between scientists and a theory-independent reality. Were the realist unable to treat observation and measurement as providing "epistemic access" to reality in this way, a constructivist treatment of scientific knowledge would be almost unavoidable.

Similarly, the scientific realist is able to employ a naturalistic conception of definitions and of reference only because (1) it is arguable that the nature of the subject matter of science dictates that kinds, properties, etc. be defined by nonconventional definitions, and (2) it is arguable that actual scientific practices result in the establishment of "epistemic access" to the various "theoretical entities" which, the realist maintains, are (part of) the subject matter of scientific inquiry.

Finally, the realist can insist that realism not only can tolerate but implies certain failures of bivalence only because it can be argued that homeostatic cluster kinds (properties, etc.) must have indeterminacy in extension in order for reference to them to be scientifically fruitful. These considerations suggest that the following constraints must be satisfied by an account of moral knowledge if it is to be the basis for the proposed defense of moral realism:

1 It must be possible to explain how our moral reasoning *started out* with a stock of relevantly approximately true moral beliefs so that reflective equilibrium in moral reasoning can be treated in a fashion analogous to the scientific realist's treatment of reflective equilibrium in scientific reasoning. Note that this constraint does not require that it be possible to argue that we started out with close approximations to the truth (seventeenth-century corpuscular theory was quite far from the truth). What is required is that the respects of approximation be such that it is possible to see how continued approximations would be forthcoming as a result of subsequent moral and nonmoral reasoning.

2 There must be an answer to the question "What plays, in moral reasoning, the role played by observation in science?" which can form the basis for a realist rather than a constructivist conception of the foundations of reflective equilibrium in moral reasoning.

3 It must be possible to explain why moral properties, say goodness, would require natural rather than conventional definitions.

4 It must be possible to show that our ordinary use of moral terms provides us with epistemic access to moral properties. Moral goodness must, to some extent, regulate the use of the word "good" in moral reasoning. Here again examination of the corresponding constraint in the philosophy of science indicates that the regulation need not be nearly perfect, but it must be possible to show that sufficient epistemic access is provided to form the basis for the growth of moral knowledge.

5 It must be possible to portray occasional indeterminacy in the extension of moral terms as rationally dictated by the nature of the subject matter in a way analogous to the scientific realist's treatment of such indeterminacy in the case of homeostatic cluster terms.

In the work of scientific realists, the case that the analogous constraints are satisfied has depended upon examination of the substantive findings of various of the sciences (such as, e.g., the atomic theory of matter or the Darwinian conception of speciation). It is very unlikely that an argument could be mounted in favor of the view that moral knowledge meets the constraints we are considering which does not rely in a similar way on substantive doctrines about the foundations of morals. What I propose to do instead is to *describe* one account of the nature of morals which almost ideally satisfies the constraints in question and to indicate how a defense of moral realism would proceed on the basis of this account.

It will not be my aim here to defend this account of morals against morally plausible rivals. In fact, I am inclined to think—*partly* because of the way in which it allows the constraints we are considering to be satisfied —that *if* there is a truth of the matter about morals (that is, if moral realism is true), then the account I will be offering is close to the truth. But my aim in this paper is merely to establish that moral realism is plausible and defensible. The substantive moral position I will consider is a plausible version of nonutilitarian consequentialism, one which—I believe—captures many of the features which make consequentialism *one* of the standard and plausible positions in moral philosophy. If moral realism is defensible on the basis of a plausible version of consequentialism, then it is a philosophically defensible position which must be taken seriously in metaethics; and that's all I'm trying to establish here.

It is, moreover, pretty clear that a variety of plausible alternative conceptions of the foundations of morals satisfy the constraints we are discussing. If I am successful here in mounting a plausible defense of moral realism, given the substantive conception I will propose, then it is quite likely that the very powerful semantic and epistemic resources of recent realist philosophy of science could be effectively employed to defend moral realism on the basis of many of the alternative conceptions. I leave it to the defenders of alternative conceptions to explore these possibilities. The defense of moral realism offered here is to be thought of as (the outline of) a "worked example" of the application of the general strategy proposed in 4.1.

One more thing should be said about the substantive conception of morals offered here. Like any naturalistic account, it rests upon potentially controversial empirical claims about human psychology and about social theory. It is a commonplace, I think, that moral realism is an optimistic position (or, perhaps, that it is typically an optimist's position). One nice feature of the substantive analysis of morals upon which my defense of moral realism will be based is that it quite obviously rests upon optimistic claims about human potential. Perhaps in that respect it is well suited to serve as a representative example of the variety of substantive moral views which would satisfy the constraints in question. (For a further discussion of the methodological implications of the moral realist's reliance on particular substantive moral theories see section 5.3.)

4.3
Homeostatic consequentialism

In broad outline, the conception of morals upon which the sample defense of moral realism will rest goes like this:

1 There are a number of important human goods, things which satisfy important human needs. Some of these needs are physical or medical. Others are psychological or social; these (probably) include the need for love and friendship, the need to engage in cooperative efforts, the need to exercise control over one's own life, the need for intellectual and artistic appreciation and expression, the need for physical recreation, etc. The question of just which important human needs there are is a potentially difficult and complex empirical question.

2 Under a wide variety of (actual and possible) circumstances these human goods (or rather instances of the satisfaction of them) are homeostatically clustered. In part they are clustered because these goods themselves are— when present in balance or moderation— mutually supporting. There are in addition psychological and social mechanisms which when, and to the extent to which, they are present contribute to the homeostasis. They probably include cultivated attitudes of mutual respect, political democracy, egalitarian social relations, various rituals, customs, and rules of courtesy, ready access to education and information, etc. It is a complex and difficult question in psychology and social theory just what these mechanisms are and how they work.

3 Moral goodness is defined by this cluster of goods and the homeostatic mechanisms which unify them. Actions, policies, character traits, etc. are morally good to the extent to which they tend to foster the realization of these goods or to develop and sustain the homeostatic mechanisms upon which their unity depends.

4 In actual practice, a concern for moral goodness can be a guide to action for the morally concerned because the homeostatic unity of moral goodness

tends to mitigate possible conflicts between various individual goods. In part, the possible conflicts are mitigated just because various of the important human goods are mutually reinforcing. Moreover, since the existence of effective homeostatic unity among important human goods is part of the moral good, morally concerned choice is constrained by the imperative to balance potentially competing goods in such a way that homeostasis is maintained or strengthened. Finally, the improvement of the psychological and social mechanisms of homeostasis themselves is a moral good whose successful pursuit tends to further mitigate conflicts of the sort in question. In this regard, moral practice resembles good engineering practice in product design. In designing, say, automobiles there are a number of different desiderata (economy, performance, handling, comfort, durability…) which are potentially conflicting but which enjoy a kind of homeostatic unity if developed in moderation. One feature of good automotive design is that it promotes these desiderata within the limits of homeostasis. The other feature of good automotive design (or, perhaps, of good automotive engineering) is that it produces technological advances which permit that homeostatic unity to be preserved at higher levels of the various individual desiderata. So it is with good moral practice as well.[2]

I should say something about how the claim that the nature of the constituents of moral goodness is an empirical matter should be understood. I mean the analogy between moral inquiry and scientific inquiry to be taken *very* seriously. It is a commonplace in the history of science that major advances often depend on appropriate social conditions, technological advances, and prior scientific discoveries. Thus, for example, much of eighteenth-century physics and chemistry was possible only because there had developed (a) the social conditions in which work in the physical sciences was economically supported, (b) a technology sufficiently advanced to make the relevant instrumentation possible, and (c) the theoretical social potential. Much of this knowledge is genuinely *experimental* knowledge and the relevant experiments are ("naturally" occurring) political and social experiments whose occurrence and whose interpretation depends both on "external" factors and upon the current state of our moral understanding. Thus, for example, we would not have been able to explore the dimensions of our needs for artistic expression and appreciation had not social and technological developments made possible cultures in which, for some classes at least, there was the leisure to produce and consume art. We would not have understood the role of political democracy in the homeostasis of the good had the conditions not arisen in which the first limited democracies developed. Only after the moral insights gained from the first democratic experiments were in hand, were we equipped to see the depth of the moral peculiarity of slavery. Only since the establishment of the first socialist societies are we even beginning to obtain the data necessary to assess the role of egalitarian social practices in fostering the good.

It is also true of moral knowledge, as it is in case of knowledge in other "special sciences", that the improvement of knowledge may depend upon theoretical advances in related disciplines. It is hard, for example, to see how deeper understanding in history or economic theory could fail to add to our understanding of human potential and of the mechanisms underlying the homeostatic unity of the good.

Let us now consider the application of the particular theory of the good presented here as a part of the strategy for the defense of moral realism indicated in the preceding section. I shall be primarily concerned to defend the realist position that moral goodness is a real property of actions, policies, states of affairs, etc. and that our moral judgments are, often enough, reflections of truths about the good. A complete realist treatment of the semantics of moral terms would of course require examining notions like obligation and justice as well. I will not attempt this examination here, in part because the aim of this essay is merely to indicate briefly how a plausible defense of moral realism might be carried out rather than to carry out the defense in detail. Moreover, on a consequentialist conception of morals such notions as obligation and justice are derivative ones, and it is doubtful if the details of the derivations are relevant to the defense of moral realism in the way that the defense of a realist conception of the good is.

In the remaining sections of the essay I shall offer a defense of homeostatic consequentialist moral realism against the representative antirealist challenges discussed in part 2. The claim that the term "good" in its moral uses refers to the homeostatic cluster property just described (or even the claim that there is such a property) represents a complex and controversial philosophical and empirical hypothesis. For each of the responses to anti-realist challenges which I will present; there are a variety of possible anti-realist rebuttals, both empirical and philosophical. It is beyond the scope of this essay to explore these rebuttals and possible moral realist responses to them in any detail. Instead, I shall merely indicate how plausible realist rebuttals to the relevant challenges can be defended. Once again, the aim of the present paper is not to establish moral realism but merely to establish its plausibility and to offer a general framework within which further defenses of moral realism might be understood.

4.4
Observations, intuitions, and reflective equilibrium

Of the challenges to moral realism we are considering, two are straightforwardly epistemological. They suggest that the role of moral intuitions and of reflective equilibrium in moral reasoning dictate (at best) a constructivist interpretation of morals. As we saw in section 4.2, it would be possible for the moral realist to respond by assimilating the role of moral intuitions and reflective equilibrium to the role of scientific intuitions and theory-dependent methodological factors in the realist account of scientific knowledge, but this response is viable only if it is

possible to portray many of our background moral beliefs and judgments as relevantly approximately true and only if there is a satisfactory answer to the question: "What plays, in moral reasoning, the role played in science by observation?" Let us turn first to the latter question.

I propose the answer: "Observation".

According to the homeostatic consequentialist conception of morals (indeed, according to any naturalistic conception) goodness is an ordinary natural property, and it would be odd indeed if observations didn't play the same role in the study of this property that they play in the study of all the others. According to the homeostatic consequentialist conception, goodness is a property quite similar to the other properties studied by psychologists, historians, and social scientists, and observations will play the same role in moral inquiry that they play in the other kinds of empirical inquiry about people.

It is worth remarking that in the case of any of the human sciences *some* of what must count as observation is observation of oneself, and *some* is the sort of self-observation involved in introspection. Moreover, *some* of our observations of other people will involve trained judgment and the operation of sympathy. No reasonable naturalistic account of the foundations of psychological or social knowledge *or* of our technical knowledge in psychology or the social sciences will fail to treat such sources of belief— when they are generally reliable—as cases of observation in the relevant sense.

It is true, of course, that both the content and the evidential assessment of observations of this sort will be influenced by theoretical considerations, but this does not distinguish observations in the human sciences from those in other branches of empirical inquiry. The theory dependence of observations and their interpretation is simply one aspect of the pervasive theory dependence of methodology in science which the scientific realist cheerfully acknowledges (since it plays a crucial role in arguments for scientific realism). It is possible to defend a realist interpretation of the human sciences because it is possible to argue that actual features in the world constrain the findings in those sciences sufficiently that the relevant background theories will be approximately true enough for theory-dependent observations to play a reliable epistemic role.

In the case of moral reasoning, observations and their interpretation will be subject to just the same sort of theory-dependent influences. This theory dependence is one aspect of the general phenomenon of theory dependence of methodology in moral reasoning which we, following Rawls, have been describing as reflective equilibrium. We will be able to follow the example of scientific realists and to treat the observations which play a role in moral reasoning as sufficiently reliable for the defense of moral realism just in case we are able to portray the theories upon which they and their interpretation depend as relevantly approximately true—that is, just in case we are able to carry out the other part of the moral realist's response to epistemic challenges and to argue that our background moral beliefs are sufficiently near the truth to form the

foundations for a reliable empirical investigation of moral matters. Let us turn now to that issue.

What we need to know is whether it is reasonable to suppose that, for quite some time, we have had background moral beliefs sufficiently near the truth that they could form the basis for subsequent improvement of moral knowledge in the light of further experience and further historical developments. Assuming, as we shall, a homeostatic consequentialist conception of morals, this amounts to the question whether our background beliefs about human goods and the psychological and social mechanisms which unite them had been good enough to guide the gradual process of expansion of moral knowledge envisioned in that conception. Have our beliefs about our own needs and capacities been good enough— since, say the emergence of moral and political philosophy in ancient Greece—that we have been able to respond to new evidence and to the results of new social developments by expanding and improving our understanding of those needs and capacities even when doing so required rejecting some of our earlier views in favor of new ones? It is hard to escape the conclusion that this is simply the question "Has the rational empirical study of human kind proven to be possible?" Pretty plainly the answer is that such study has proven to be possible, though difficult. In particular we have improved our understanding of our own needs and our individual and social capacities by just the sort of historically complex process envisioned in the homeostatic consequentialist conception. I conclude therefore that there is no reason to think that reflective equilibrium—which is just the standard methodology of any empirical inquiry, social or otherwise—raises any epistemological problems for the defense of moral realism.

Similarly, we may now treat moral intuitions exactly on a par with scientific intuitions, as a species of trained judgment. Such intuitions are *not* assigned a foundational role in moral inquiry; in particular they do not substitute for observations. Moral intuitions are simply one cognitive manifestation of our moral understanding, just as physical intuitions, say, are a cognitive manifestation of 'physicists' understanding of their subject matter. Moral intuitions, like physical intuitions, play a limited but legitimate role in empirical inquiry *precisely because* they are linked to theory *and* to observations in a generally reliable process of reflective equilibrium.

It may be useful by way of explaining the epistemic points made here to consider very briefly how the moral realist might respond to one of the many possible anti-realist rebuttals to what has just been said. Consider the following objection: The realist treatment of reflective equilibrium requires that our background moral beliefs have been for some time relevantly approximately true. As a matter of fact, the overwhelming majority of people have probably always believed in some sort of theistic foundation of morals: moral laws are God's laws; the psychological capacities which underlie moral practice are a reflection of God's design; etc. According to the homeostatic consequentialism which we are supposed to accept for the sake of argument, moral facts are mere

natural facts. Therefore, according to homeostatic consequentialism, most people have always had profoundly mistaken moral beliefs. How then can it be claimed that our background beliefs have been relevantly approximately true?

I reply that—assuming that people have typically held theistic beliefs of the sort in question—it does follow from homeostatic consequentialism that they have been *in that respect* very wrong indeed. But being wrong in that respect does not preclude their moral judgments having been relatively reliable reflections of facts about the homeostatic cluster of fundamental human goods, according to the model of the development of moral knowledge discussed earlier. Until Darwin, essentially all biologists attributed the organization and the adaptive features of the physiology, anatomy, and behavior of plants and animals to God's direct planning. That attribution did not prevent biologists from accumulating the truly astonishing body of knowledge about anatomy, physiology, and animal behavior upon which Darwin's discovery of evolution by natural selection depended; nor did it prevent their recognizing the profound biological insights of Darwin's theory. Similarly, seventeenth-century corpuscular chemistry did provide the basis for the development of modern chemistry in a way that earlier quasi-animistic "renaissance naturalism" in chemistry could not. Early corpuscular theory was right that the chemical properties of substances are determined by the fundamental properties of stable "corpuscles"; it was wrong about almost everything else, but what it got right was enough to point chemistry in a fruitful direction. I understand the analogy between the development of scientific knowledge and the development of moral knowledge to be very nearly exact.

There may indeed be one important respect in which the analogy between the development of scientific knowledge and the development of moral knowledge is inexact, but oddly, this respect of disanalogy makes the case for moral realism stronger. One of the striking consequences of a full-blown naturalistic and realistic conception of knowledge is that our knowledge, even our most basic knowledge, rests upon logically contingent "foundations". Our perceptual knowledge, for example, rests upon the logically contingent a posteriori fact that our senses are reliable detectors of certain sorts of external objects. In the case of perceptual knowledge, however, there is a sense in which it is nonaccidental, noncontmgent, that our senses are reliable detectors. The approximate reliability of our senses (with respect to some applications) is explained by evolutionary theory in a quite fundamental way (Quine 1969a). By contrast, the reliability of our methodology in chemistry is much more dramatically contingent. As a matter of fact, early thinkers tried to explain features of the natural world by analogy to sorts of order they already partly understood: mathematical, psychological, and mechanical. The atomic theory of matter represents one such attempt to assimilate chemical order to the better-understood mechanical order. In several important senses it was highly contingent that the microstructure of matter turned out to be particulate and mechanical enough that the atomic (or "corpuscular") *guess* could provide the foundation for epistemically reliable

research in chemistry. The accuracy of our guess in this regard is not, for example, explained by either evolutionary necessity or by deep facts about our psychology. In an important sense, the seventeenth-century belief in the corpuscular theory of matter was not reliably produced. It was not produced by an antecedent generally reliable methodology: reasoning by analogy is *not* generally reliable except in contexts where a rich and approximately accurate body of theory *already* exists to guide us in finding the right respects of analogy (see Boyd 1982).

By contrast, the emergence of relevantly approximately true beliefs about the homeostatic cluster of fundamental human goods—although logically contingent —was much less strikingly "accidental". From the point of view either of evolutionary theory or of basic human psychology it is hardly accidental that we are able to recognize many of our own and others' fundamental needs. Moreover, it is probably not accidental from an evolutionary point of view that we were able to recognize some features of the homeostasis of these needs. Our initial relevantly approximately accurate beliefs about the good may well have been produced by generally reliable psychological and perceptual mechanisms and thus may have been clear instances of knowledge in a way in which our initial corpuscular beliefs were not (for a discussion of the latter point see Boyd 1982). It is *easier,* not *harder,* to explain how moral knowledge is possible than it is to explain how scientific knowledge is possible. Locke was right that we are fitted by nature for moral knowledge (in both the seventeenth—and the twentieth-century senses of the term) in a way that we are not so fitted for scientific knowledge of other sorts.

4.5
Moral semantics

We have earlier considered two objections to the moral realist's account of the semantics of moral terms. According to the first, the observed diversity of moral concepts—between cultures as well as between individuals and groups within a culture—suggests that it will not be possible to assign a single objective subject matter to their moral disputes. The divergence of concepts suggests divergence of reference of a sort which constructivist relativism is best suited to explain. According to the second objection, moral realism is committed to the absurd position that moral terms possess definitions in the vocabulary of the natural sciences. We have seen that a moral realist rebuttal to these challenges is possible which assimilates moral terms to naturalistically and nonreductively definable terms in the sciences. Such a response can be successful only if (1) there are good reasons to think that moral terms must possess natural rather than stipulative definitions and (2) there are good reasons to think that ordinary uses of moral terms provide us with epistemic access to moral properties, so that, for example, moral goodness to some extent regulates our use of the word "good" in moral contexts.

The homeostatic consequentialist conception of morals provides a justification for the first of these claims. If the good is denned by a homeostatic phenomenon the details of which we still do not entirely know, then it is a paradigm case of a property whose "essence" is given by a natural rather than a stipulative definition.

Is it plausible that the homeostatic cluster of fundamental human goods has, to a significant extent, regulated the use of the term "good" so that there is a general tendency, of the sort indicated by the homeostatic consequentialist conception of the growth of moral knowledge, for what we say about the good to be true of that cluster? If what I have already said about the possibility of defending a realist conception of reflective equilibrium in moral reasoning is right, the answer must be "yes." Such a tendency is guaranteed by basic evolutionary and psychological facts, and it is just such a tendency which we can observe in the ways in which our conception of the good has changed in the light of new evidence concerning human needs and potential. Indeed, the way we ("preanalytically") recognize moral uses of the term "good" and the way we identify moral terms in other languages are precisely by recourse to the idea that moral terms are those involved in discussions of human goods and harms. We tacitly assume *something like* the proposed natural definition of "good" in the practice of translation of moral discourse. I think it will help to clarify this realist response if we consider two possible objections to it. The first objection reflects the same concern about the relation between moral and theological reasoning that we examined in the preceding section. It goes like this: How is it possible for the moral realist who adopts homeostatic consequentialism to hold that there is a general tendency for our beliefs about the good to get truer? After all, the error of thinking of the good as being defined by God's will persists unabated and is—according to the homeostatic consequentialist's conception—a very important falsehood.

I reply, first, that the sort of tendency to the truth required by the epistemic access account of reference is not such that it must preclude serious errors. Newtonians were talking about mass, energy, momentum, etc. all along, even though they were massively wrong about the structure of space-time. We might be irretrievably wrong about some other issue in physics and still use the terms of physical theory to refer to real entities, magnitudes, etc. All that is required is a significant epistemically relevant causal connection between the use of a term and its referent.

Moreover, as I suggested earlier, it is characteristic of what we recognize as moral discourse (whether in English or in some other language) that considerations of human well-being play a significant role in determining what is said to be "good". The moral realist need not deny that other considerations— perhaps profoundly false ones—also influence what we say is good. After all, the historian of biology need not deny that the term "species" has relatively constant reference throughout the nineteenth century, even though, prior to Darwin, religious considerations injected profound errors into biologists' conception of

species. Remember that we do not ordinarily treat a theological theory as a theory *of* moral goodness at all unless it says something about what we independently recognize as human well-being. The role of religious considerations in moral reasoning provides a challenge for moral realists, but exactly the same challenge faces a realist interpretation of biological or psychological theorizing before the twentieth century, and it can surely be met.

The second objection I want to consider represents a criticism of moral realism often attributed to Marx (see, e.g., Wood 1972; for the record I believe that Marx's position on this matter was confused and that he vacillated between an explicit commitment to the relativist position, which Wood discusses, and a tacit commitment to a position whose reconstruction would look something like the position defended here). The objection goes like this: The moral realist—in the guise of the homeostatic consequentialist, say—holds that what regulate the use of moral terms are facts about human well-being. But this is simply not so. Consider, for example, sixteenth-century discussions of rights. One widely acknowledged "right" was the divine right of kings. Something surely regulated the use of the language of rights in the sixteenth century, but it clearly wasn't human well-being construed in the way the moral realist intends. Instead, it was the well-being of kings and of the aristocratic class of which they were a part.

I agree with the analysis of the origin of the doctrine of the divine right of kings; indeed, I believe that such class determination of moral beliefs is a commonplace phenomenon. But I do not believe that this analysis undermines the claim that moral terms refer to aspects of human well-being. Consider, for example, the psychology of thinking and intelligence. It is extremely well documented (see, e.g., Gould 1981; Kamin 1974) that the content of much of the literature in this area is determined by class interests rather than by the facts. Nevertheless, the psychological terms occurring in the most egregiously prejudiced papers refer to real features of human psychology; this is so because, in other contexts, their use is relevantly regulated by such features. Indeed—and this is the important point—if there were not such an epistemic (and thus referential) connection to real psychological phenomena, the ideological rationalization of class structures represented by the class-distorted literature would be ineffective. It's only when people come to believe, for example, that Blacks lack a trait, *familiar in other contexts as "intelligence",* that racist theories can serve to rationalize the socioeconomic role to which Blacks are largely confined.

Similarly, I argue, in order for the doctrine of the divine right of kings to serve a class function, it had to be the case that moral language was often enough connected to issues regarding the satisfaction of real human needs. Otherwise, an appeal to such a supposed right would be ideologically ineffective. Only when rights-talk has *some* real connection to the satisfaction of the needs of non-aristocrats could this instance of rights-talk be useful to kings and their allies.

Once again, when the analogy between moral inquiry and scientific inquiry is fully exploited, it becomes possible to defend the doctrines upon which moral realism rests.

4.6
Hard cases and divergent views

Two of the challenges to moral realism we are considering are grounded in the recognition that some moral issues seem very hard to resolve. On the one hand, there seem to be moral dilemmas which resist resolution even for people who share a common moral culture. Especially with respect to the sort of possible cases often considered by moral philosophers, there often seems to be no rational way of deciding between morally quite distinct courses of action. Our difficulty in resolving moral issues appears even greater when we consider the divergence in moral views that exists between people from different backgrounds or cultures. The anti-realist proposes to explain the difficulties involved by denying that there is a common objective subject matter which determines answers to moral questions.

We have seen that—to the extent that she chooses to take the difficulties in resolving moral issues as evidence for the existence of moral statements for which bivalence fails—the moral realist can try to assimilate such failures to the failures of bivalence which realist philosophy *predicts* in the case, for example, of some statements involving homeostatic cluster terms. Such a response will work only to the extent that moral terms can be shown to possess natural definitions relevantly like homeostatic cluster definitions. Of course, according to homeostatic consequentialism, moral terms (or "good" at any rate) just are homeostatic cluster terms, so this constraint is satisfied. What I want to emphasize is that a moral realist *need not* invoke failures of bivalence in every case in which difficulties arise in resolving moral disputes.

Recall that on the conception we are considering moral inquiry is about a complex and difficult subject matter, proceeds often by the analysis of complex and "messy" naturally occurring social experiments, and is subject to a very high level of social distortion by the influence of class interests and other cultural factors. In this regard moral inquiry resembles inquiry in any of the complex and politically controversial social sciences. In such cases, even when there is no reason to expect failures of bivalence, one would predict that the resolution of some issues will prove difficult or, in some particular social setting, impossible. Thus the moral realist can point to the fact that moral inquiry is a species of social inquiry to explain much of the observed divergence in moral views and the apparent intractability of many moral issues.

Similarly, the complexity and controversiality of moral issues can be invoked to explain the especially sharp divergence of moral views often taken to obtain between different cultures. For the homeostatic consequentialist version of moral realism to be true it must be the case that in each culture in which moral inquiry

takes place the homeostatically clustered human goods epistemically regulate moral discourse to an appreciable extent. On the realistic and naturalistic conception of the growth of knowledge, this will in turn require that the moral tradition of the culture in question embody some significant approximations to the truth about moral matters. It is, however, by no means required that two such cultural traditions have started with initial views which approximated the truth to the same extent or along the same dimensions, nor is it required that they have been subjected to the same sorts of social distortion, nor that they have embodied the same sorts of naturally occurring social experimentation. It would thus be entirely unsurprising if two such traditions of moral inquiry should have, about some important moral questions, reached conclusions so divergent that no resolution of their disagreement will be possible within the theoretical and methodological framework which the two traditions *currently* have in common, even though these issues may possess objective answers eventually discoverable from within either tradition or from within a broader tradition which incorporates insights from both.

In this regard it is useful to remember the plausibility with which it can be argued that, if there were agreement on all the nonmoral issues (including theological ones), then there would be no moral disagreements. I'm not sure that this is exactly right. For one thing, the sort of moral agreement which philosophers typically have in mind when they say this sort of thing probably does not include agreement that some question has an indeterminate answer, which is something predicted by homeostatic consequentialism. Nevertheless, careful philosophical examination will reveal, I believe, that agreement on nonmoral issues would eliminate *almost all* disagreement about the sorts of moral issues which arise in ordinary moral practice. Moral realism of the homeostatic consequentialist variety provides a quite plausible explanation for this phenomenon.

It is nevertheless true that, for some few real-world cases and for *lots* of the contrived cases so prevalent in the philosophical literature, there does appear to be serious difficulty in finding rational resolutions—assuming as we typically do that an appeal to indeterminacy of the extension of "good" doesn't count as a resolution. In such cases the strategy available to the moral realist *is* to insist that failures of bivalence do occur just as a homeostatic consequentialist moral realist predicts.

Philosophers often suggest that the major normative ethical theories will yield the same evaluations in almost all actual cases. Often it is suggested that this fact supports the claim that there is some sort of objectivity in ethics, but it is very difficult to see just why this should be so. Homeostatic consequentialist moral realism provides the basis for a satisfactory treatment of this question. Major theories in normative ethics have almost always sought to provide definitions for moral terms with almost completely definite extensions. This is, of course, in fact a mistake; moral terms possess homeostatic cluster definitions instead. The appearance of sharp divergence between major normative theories, with respect to the variety of possible cases considered by philosophers, arises from the fact

that they offer different putative resolutions to issues which lack any resolution *at all* of the sort anticipated in those theories. The general agreement of major normative theories on almost all actual cases is explained both by the fact that the actual features of the good regulate the use of the term "good" in philosophical discourse *and* by the homeostatic character of the good: when different normative theories put different weight on different components of the good, the fact that such components are—in actual cases—linked by reliable homeostatic mechanisms tends to mitigate, in real-world cases, the effects of the differences in the weights assigned. Homeostatic consequentialism represents the common grain of truth in other normative theories. (For further discussion of the resulting case for moral realism see section 5.4.)

4.7
Morality, motivation, and rationality

There remains but one of the challenges to moral realism which we are here considering. It has often been objected against moral realism that there is some sort of logical connection between moral judgments and reasons for action which a moral realist cannot account for. It might be held, for example, that the recognition that one course of action is morally preferable to another *necessarily* provides a reason (even if not a decisive one) to prefer the morally better course of action. Mere facts (especially mere *natural* facts) cannot have this sort of logical connection to rational choice or reasons for action. Therefore, so the objection goes, there cannot be moral facts; moral realism (or at least naturalistic moral realism) is impossible.

It is of course true that the naturalistic moral realist must deny that moral judgments necessarily provide reasons for action; surely, for example, there could be nonhuman cognizing systems which could understand the natural facts about moral goodness but be entirely indifferent to them in choosing how to act. Moral judgments might provide for them no reasons for action whatsoever. Moreover, it is hard to see how the naturalistic moral realist can escape the conclusion that it would be *logically possible* for there to be a human being for whom moral judgments provided no reasons for action. The moral realist must therefore deny that the connection between morality and reasons for action is so strong as the objection we are considering maintains. The appearance of an especially intimate connection must be explained in some other way.

The standard naturalist response is to explain the apparent intimacy of the connection by arguing that the natural property moral goodness is one such that for psychologically normal humans, the fact that one of two choices is morally preferable will in fact provide some reason for preferring it. The homeostatic consequentialist conception of the good is especially well suited to this response since it defines the good in terms of the homeostatic unity of fundamental human needs. It seems to me that this explanation of the close connection between moral judgments and reasons for action is basically right, but it ignores—it

seems to me—one important source of the anti-realist's intuition that the connection between moral judgments and rational choice must be a necessary one. What I have in mind is the very strong intuition which many philosophers share that the person for whom moral judgments are motivationally indifferent would not only be psychologically atypical but would have some sort of *cognitive* deficit with respect to moral reasoning as well. The anti-realist diagnoses this deficit as a failure to recognize a definitional or otherwise necessary connection between moral goodness and reasons for action.

I think that there is a deep insight in the view that people for whom questions of moral goodness are irrelevant to how they would choose to act suffer a cognitive deficit. I propose that the deficit is not—as the anti-realist would have it—a failure to recognize a necessary connection between moral judgments and reasons for action. Instead, I suggest, if we adopt a naturalistic conception of moral knowledge we can diagnose in such people a deficit in the capacity to make moral judgments somewhat akin to a perceptual deficit. What I have in mind is the application of a causal theory of moral knowledge to the examination of a feature of moral reasoning which has been well understood in the empiricist tradition since Hume, that is, the role of sympathy in moral understanding.

It is extremely plausible that for normal human beings the capacity to access human goods and harms—the capacity to *recognize* the extent to which others are well or poorly off with respect to the homeostatic cluster of moral goods and the capacity to *anticipate correctly* the probable effect on others' well-being of various counterfactual circumstances—depends upon their capacity for sympathy, their capacity to imagine themselves in the situation of others or even to find themselves involuntarily doing so in cases in which others are especially well or badly off. The idea that sympathy plays this sort of cognitive role is a truism of nineteenth-century faculty psychology, and it is very probably right.

It is also very probably right, as Hume insists, that the operation of sympathy is *motivationally* important: as a matter of contingent psychological fact, when we put ourselves in the place of others in imagination, the effects of our doing so include our taking pleasure in others' pleasures and our feeling distress at their misfortune, and we are thus motivated to care for the well-being of others. The psychological mechanisms by which all this takes place may be more complicated than Hume imagined, but the fact remains that one and the same psychological mechanism—sympathy—plays *both* a cognitive *and* a motivational role in normal human beings. We are now in a position to see why the morally unconcerned person, the person for whom moral facts are motivationally irrelevant, probably suffers a *cognitive* deficit with respect to moral reasoning. Such a person would have to be deficient in sympathy, because the motivational role of sympathy is precisely to make moral facts motivationally relevant. In consequence, she or he would be deficient with respect to a cognitive capacity (sympathy) which is ordinarily important for the correct assessment of moral facts. The motivational deficiency would, as a matter of contingent fact about human psychology, be a cognitive deficiency as well.

Of course it does not follow that there could not be cognizing systems which are quite capable of assessing moral facts without recourse to anything like sympathy; they might, for example, rely on the application of a powerful tacit or explicit theory of human psychology instead. Indeed it does not follow that there are not actual people—some sociopaths and con artists, for example—who rely on such theories instead of sympathy. But it is true, just as the critic of moral realism insists, that there is generally a cognitive deficit associated with moral indifference. The full resources of naturalistic epistemology permit the moral realist to acknowledge and explain this important insight of moral anti-realists.

4.8
Conclusion

I have argued that if the full resources of naturalistic and realistic conceptions of scientific knowledge and scientific language are deployed and if the right sort of positive theory of the good is advanced, then it is possible to make a plausible case for moral realism in response to typical anti-realist challenges. Two methodological remarks about the arguments I have offered may be useful. In the first place, the rebuttals I have offered to challenges to moral realism really do depend strongly upon the naturalistic and nonfoundational aspects of current (scientific) realist philosophy of science. They depend, roughly, upon the aspects of the scientific realist's program which make it plausible for the scientific realist to claim that philosophy is an empirical inquiry continuous with the sciences and with, e.g., history and empirical social theory. I have argued elsewhere (Boyd 1981, 1982, 1983, 1985a, 1985b, 1985c) that these aspects of scientific realism are essential to the defense of scientific realism against powerful empiricist and constructivist arguments.

If we now ask how one should decide between scientific realism and its rivals, I am inclined to think that the answer is that the details of particular technical arguments will not be sufficient to decide the question rationally; instead, one must assess the overall conceptions of knowledge, language, and understanding which go with the rival conceptions of science (I argue for this claim in Boyd 1983). *One* important constraint on an acceptable philosophical conception in these areas is that it permit us to understand the obvious fact that moral reasoning is not nearly so different from scientific or other factual reasoning as logical positivists have led us to believe. It is initially plausible, I think, that a constructivist conception of science is favored over both empiricist and realist conceptions insofar as we confine our attention to this constraint. If what I have said here is correct, this may well not be so. Thus the successful development of the arguments presented here may be relevant not only to our assessment of moral realism but to our assessment of scientific realism as well. Here is a kind of methodological unity of philosophy analogous to (whatever it was which positivists called) "unity of science".

My second methodological point is that the arguments for moral realism presented here depend upon optimistic empirical claims both about the organic unity of human goods and about the possibility of reliable knowledge in the "human sciences" generally. Although I have not argued for this claim here, I believe strongly that any plausible defense of naturalistic moral realism would require similarly optimistic empirical assumptions. I am also inclined to believe that insofar as moral anti-realism is plausible its plausibility rests not only upon technical philosophical arguments but also upon relatively pessimistic empirical beliefs about the same issues. I suggest, therefore, that our philosophical examination of the issues of moral realism should include, in addition to the examination of technical arguments on both sides, the careful examination of empirical claims about the unity and diversity of human goods and about our capacity for knowledge of ourselves. That much of philosophy ought surely to be at least partly empirical.

5
ADDENDUM

5.1
History

This paper, in the form in which it appears here, was written in 1982. Since that time it has undergone a transformation into a work, *Realism and the Moral Sciences* (Boyd, forthcoming, henceforth *RMS*) much too long to publish or excerpt for the present volume. I do however want to indicate briefly the direction in which the line of argument presented here has been developed in that later work. I shall briefly summarize three ways in which *RMS* goes beyond the argumentative strategy of this essay: a further characterization of homeostatic property-cluster definitions, a response to an apparent circularity resulting from the employment of a sample substantive moral theory, and an indication of the most general evidence favoring moral realism.

5.2
Homeostatic property clusters again

In *RMS* I add an additional clause to the account of homeostatic property-cluster definitions as follows:

12 The homeostatic property cluster which serves to define *t* is not individuated extensionally. Instead, property clusters are individuated like (type or token) historical objects or processes: certain changes over time (or in space) in the property cluster or in the underlying homeostatic mechanisms preserve the

identity of the defining cluster. In consequence, the properties which determine the conditions for falling under *t* may vary over time (or space), *while t continues to have the same definition*. (To fall under *t* is to participate in the (current temporal and spatial stage of) the relevant property clustering. The historicity of the individuation criteria for definitional property clusters of this sort reflects the explanatory or inductive significance (for the relevant branches of theoretical or practical inquiry) of the historical development of the property cluster and of the causal factors which produce it, and considerations of explanatory and inductive significance determine the appropriate standards of individuation for the property cluster itself. The historicity of the individuation conditions for the property cluster is thus essential for the naturalness of the kind to which *t* refers.

This modification is suggested by the example of biological species definitions. The property cluster and homeostatic mechanisms which define a species must in general be individuated nonextensionally as a process-like historical entity. This is so because the mechanisms of reproductive isolation which are fundamentally definitional for many sexually reproducing species may vary significantly over the life of a species. Indeed, it is universally recognized that selection for characters which enhance reproductive isolation from related species is a significant factor in phyletic evolution, and it is one which necessarily alters over time a species' defining property cluster and homeostatic mechanisms (Mayr 1970).

I propose in *RMS* that the homeostatic property-cluster definition of moral goodness exhibits this sort of historicality. This additional factor increases the complexity of that definition considerably. Moreover there are failures of bivalence in the individuation of homeostatic property-cluster definitions, especially across possible worlds, just as there are for other sorts of historically individuated entities. These bivalence failures with respect to the individuation of the definition of moral goodness increase the range of counterfactual cases for which there will be failures of bivalence in the application of the term "good". The resources available to the moral realist for explaining divergent moral opinions, especially with respect to counterfactual cases, are thus enhanced.

5.3
Hard cases, cultural variability, and an apparent circularity of argumentation

In part 4 of the present essay, I defend moral realism from the perspective of a sample substantive moral theory, homeostatic consequentialism. I argue that since this substantive theory is defensible and since it affords the basis for a reasonable defense of a moral realism, moral realism is itself philosophically defensible. In *RMS* I consider a possible objection to this argumentative strategy.

According to the objection, the defense of moral realism offered here requires a realistic understanding of homeostatic consequentialism, since otherwise, for example, the moral properties to which epistemic access is demonstrated might be purely socially constructed or conventional, as constructivist anti-realists in ethics maintain. The realist understanding of homeostatic consequentialism, so the objection goes, begs the question against the anti-realist so that the defense of moral realism is not even prima facie successful.

I examine this objection in the light of the corresponding objection to arguments for scientific realism. I argue that a defense of scientific realism requires that the realist articulate and defend a *theory of epistemic contact* and a *theory of error* for those traditions of inquiry for which she offers a realist account. Each of these theories must necessarily rest upon the best available theories of the relevant subject matter, realistically understood. In order to see why the question is not necessarily begged against the anti-realist we need to distinguish two sorts of anti-realist arguments from the diversity of opinions or intractability of issues within the relevant area of inquiry.

The first sort of argument, which I call the *external argument from theoretical diversity,* challenges the realist to explain the diversity of theoretical conceptions and the difficulty of their resolution within the relevant tradition of inquiry. An adequate realist response to this challenge will consist of an account of the epistemically significant causal relations between inquirers and the supposed theory-independent subject matter of that tradition, together with a theory of the sources of error within the tradition which account for the observed diversity of theoretical conceptions and for whatever difficulty exists in resolving the resulting theoretical disputes. These theories of epistemic contact and of error will necessarily and properly reflect the best available current theories of the subject matter in question realistically understood. No question is begged against the antirealist because the realistically understood theories of epistemic contact and of error do not *by themselves* constitute the argument for realism. Instead, the philosophical contest is between larger-scale *philosophical packages:* a realist package which incorporates the realistically understood theories of epistemic contact and of error into a larger account of the metaphysics, epistemology, semantics, methodology, and historical development of the relevant areas of inquiry and various competing anti-realist packages of comparable scope.

Indeed, I argue, an understanding of scientific realism according to which it is grounded in realistically understood theories of epistemic contact and of error is essential not only for a fair presentation of the case *for* realism but also for a fair presentation of the various cases *against* it. I conclude, by analogy, that no questions are begged with respect to the external argument from theoretical diversity in the moral realist's reliance upon realistically understood theories of epistemic contact and of error.

I also identify a fundamentally different anti-realist argument from theoretical diversity, which I call the *internal argument from theoretical diversity* and which

represents, I suggest, an important but largely inexplicit consideration in arguments against realism, whether scientific or moral. The internal argument is directed against the evidential acceptability of the realist's philosophical package even by its own standards. The argument proceeds by identifying a widely accepted and (so far as I can see) unobjectionable methodological principle according to which the prevalence of a variety of competing theoretical conceptions within a subject area would reduce one's confidence in the truth (or approximate truth) of any one of them. This principle is then applied against the realist's theories of epistemic contact and of error. It is argued that the existence of a diversity of competing theories in the relevant area(s) of inquiry renders epistemically illegitimate the particular theoretical commitments which underlie whichever theories of epistemic contact and of error the realist chooses to adopt.

The problem raised by this criticism of the realist's philosophical package is not that it does a poorer job than that of some anti-realist competitors in explaining theoretical diversity; rather the objection is that the realist must, by her own standards of evidence, hold that there is little evidence favoring the theoretical conceptions which underlie her own philosophical position.

It is important to recognize that because it appeals to standards of evidence which are prephilosophically generally accepted and are presumed to be internal to the relevant discipline (s), the internal argument from theoretical diversity is cogent only with respect to the theoretical diversity represented by those competing theories which are plausible candidates given the best current methodological standards in the relevant disciplines. It thus contrasts sharply with the external argument. To respond appropriately to the latter, the realist must be in a position to adequately explain *all* of the diverse opinions (however implausible by current standards) within the history of the tradition of inquiry regarding which she defends a realist conception. In responding to the internal argument, by contrast, she need only respond to the challenge to her theoretical commitments which is raised by the diversity within currently plausible theoretical conceptions, where plausibility is assessed by the best available contemporary standards.

I suggest in *RMS* that the way in which scientific realists have tacitly met this (itself inexplicit) objection can be reconstructed as follows: Instead of offering, for the discipline (s) in question, a single theory of epistemic contact and a single theory of error derived from one of the plausible alternative theoretical conceptions, the realist should be thought of as offering a family of such pairs of theories, one pair grounded in each of the alternative conceptions. She then should be thought of as arguing that these alternative theories of epistemic contact and of error participate sufficiently in a relationship of *(partial) mutual ratification* sufficiently deep that an adequate realist philosophical package can be grounded in their disjunction. By their partial mutual ratification I intend the relationship which obtains if the rival theories of epistemic contact and of error agree about a large number of particular cases of epistemic success and of error and if they give similar accounts about the nature of evidential relationships

between data and doctrines within the relevant field(s) without, of course, agreeing in all of the theoretical details of their accounts of those relationships.

The levels of mutual ratification between competing theories of epistemic contact and of error which are required for the defense of realism will depend on the broader dialectical interactions of the competing philosophical packages, realist and anti-realist. It is nevertheless possible to identify an extremely strong pattern of mutual ratification which seems to characterize the methodological situation of those mature sciences regarding which realism is agreed to be an especially plausible position.

For any particular body of inquiry we may construct a conditionalized theory of epistemic contact and of error as follows. First, we form each of the propositions of the form "If T then (C and E)", where T is one of the currently plausible theoretical conceptions in the relevant field and C and E are the theories of epistemic contact and of error for the history of the relevant body of inquiry which are best suited to a defense of a realist conception of that body of inquiry, on the assumption that T is the (largely) correct choice from among the competing plausible alternatives. We then form the conditionalized theory of epistemic contact and of error by taking the conjunction of each of these propositions. Let us say that a situation of mutual conditional ratification obtains if (a) the individual theories of epistemic contact and of error obtained from the various plausible theories agree on many actual cases of evidential judgments and (b) the conditionalized theory of epistemic contact is rationally acceptable given the standards of evidence common to *all* the competing theoretical conceptions. It is this strong pattern of *conditional mutual ratification* which seems to characterize those areas of inquiry about which realism seems especially plausible.

Having characterized mutual conditional ratification in *RMS,* I then develop the claim of the present essay that a defense of moral realism along roughly the lines developed in part 4 is possible on the basis of any of the genuinely plausible general moral theories. I consider the theories of epistemic contact and of error which would be appropriate to such theories, and I conclude that to a *very* good first approximation conditional mutual ratification obtains with respect to the spectrum of general moral theories which are genuinely plausible by the best current standards. Indeed, I argue, an especially strong form of conditional mutual ratification obtains which is characterized by three additional features:

1 To an extremely good first approximation, moral judgments regarding actual cases of actions, policies, character traits, etc. are—given prevailing standards of moral argument—dictated by judgments regarding nonmoral factual questions (including, for example, questions about human nature, about the nature of social, political, and economic processes, about whether or not there are any gods, and about their natures if there are any…). In consequence, moral disagreements regarding such actual cases can be seen (on a philosophically appropriate rational reconstruction) as stemming from

disagreements over nonmoral factual matters. (I call this relationship the *rational supervenience* of the relevant moral judgments on nonmoral factual judgments.)

2 Rational supervenience appears to fail for a few actual cases and for many counterfactual ones. For almost all of these it is plausible to argue that the cases in question are ones in which there is unrecognized failure of bivalence. For the few remaining cases of apparent failures of rational supervenience realist explanations in terms of nonculpable inadequacies in methodology or theoretical understanding are readily available. This conception of the sources of failures of rational supervenience is itself ratified by all of the genuinely plausible competing general moral theories.

3 The conditionalized theory of epistemic contact and of error upon which the plausible competing general moral theories agree is such that it attributes differences in judgments regarding *general* moral theories to differences over nonmoral factual matters. Thus, rational supervenience upon nonmoral factual judgments obtains for general moral theories as well as for particular moral judgments.

It is upon this quite striking form of conditional mutual ratification that, in my view, the moral realist's response to the internal argument from theoretical diversity properly rests.

5.4
The evidence for moral realism

In the present essay I argue that moral realism can be defended on the basis of a particular substantive moral theory (homeostatic consequentialism) which is itself defensible. I conclude that moral realism is itself a defensible position worthy of further development, and of criticisms appropriate to the epistemological, semantic, and metaphysical arguments in its favor which the analogy with scientific realism suggests. I suggest here (and argue in *RMS*) that the same sort of defense can be formulated on the basis of any of the other plausible competing moral theories. Thus, if the arguments I offer are correct, there is reason to believe that the defender of any of the currently plausible general moral theories should defend her theory on a realist understanding of its content and should herself be a moral realist. The question remains what the attitude toward moral realism should be of the philosopher who is, as yet, not committed to any particular general moral theory.

I address this question in *RMS*. I maintain that the best argument for moral realism in the present philosophical context probably would consist of a more thoroughgoing defense of a particular naturalistic and realistic substantive moral theory much like homeostatic consequentialism. I also conclude, however, that there is powerful evidence favoring moral realism whose persuasive force does not depend upon establishing the case for any particular moral theory. Indeed, I

suggest that the strongest such evidence is provided by the phenomenon of conditional mutual ratification just discussed and especially by the apparent rational supervenience of moral opinion upon nonmoral factual opinion which it reveals.

Three considerations suggest that the phenomenon in question provides especially good prima facie evidence for moral realism. In the first place, the current philosophical setting is one in which answers are seen to be readily available to the more abstract epistemological and semantic objections to moral realism (those raised by the issues of the nature of the analog in moral inquiry of observations in science, of the epistemic roles of moral intuitions and of reflective equilibrium, of the nature of the definitional and referential semantics of moral terms). In such a setting the arguments from the diversity of moral theories and from the corresponding intractability of moral disputes—just the arguments addressed by the articulation of a family of conditionally mutually ratifying theories of epistemic contact and of error—emerge as the strongest arguments against moral realism.

Second, the anti-realist arguments from diversity and intractability are especially persuasive because they appear to establish that even the philosopher with substantial initial moral commitments will be forced to the conclusion that the non-reality (or the purely socially constructed nature) of her subject matter provides the only plausible explanation for the diversity of moral opinions and the intractability of moral disputes. What the finding of conditional mutual ratification of theories of epistemic contact and of error and the associated rational supervenience of moral opinions upon nonmoral factual opinions indicates is that, by contrast, there is an alternative realist explanation for divergence and intractability which is ratified by all the currently plausible moral theories.

Finally, the most convincing evidence against moral realism stemming from divergence and intractability seems (at least for many professional philosophers) to come from an examination of the many counterfactual cases regarding which "moral intuitions" sharply diverge. The foundational role which many philosophers assign (if only tacitly) to philosophical intuitions and especially to moral intuitions makes this evidence against moral realism seem especially strong. It is precisely with respect to such cases that the treatment of the epistemic role of moral intuitions and the identification of sources of bivalence failures for counterfactual cases which are incorporated in the various conditionally mutually ratifying theories of epistemic contact and of error are most effective. Thus, the realist resources for explaining divergence and intractability reflected in those theories seem especially well suited to rebut the most convincing of the anti-realist arguments in question.

I should add that in *RMS* I examine in detail a related objection to moral realism: that the moral realist is (in contrast to the constructivist moral irrealist) compelled to adopt an implausible and objectionable chauvinist attitude toward moral communities (especially prescientific communities) whose moral views

depart sharply from her own. A tendency toward such chauvinism was certainly a feature of logical positivist treatments of scientific objectivity and it is initially plausible to conclude that it will mark the moral realist's conception as well.

By way of examining the question of chauvinism, I define three relations of commensurability which might obtain within a tradition of inquiry. *Semantic commensurability* obtains just in case there is a common subject matter for all the temporal stages of the tradition and its various subtraditions. *Global methodological commensurability* obtains just in case the differences between the prevailing theoretical conceptions between any two tradition (or subtradition) stages are always resolvable by the appropriate application of research methods endorsed by each. *Local methodological commensurability* obtains just in case this sort of resolution is always possible for the differences between consecutive tradition stages or between contemporaneous stages of different subtraditions within the tradition of inquiry in question.

I argue that the tendency toward chauvinism within positivist philosophy of science—insofar as its origins were internal to technical philosophy rather than more broadly social—stemmed from a tendency for positivists to hold that semantic commensurability entails (or at any rate strongly suggests) global methodological commensurability and from a consequent tendency to apply contemporary standards of scientific methodology when assessing the rationality of members of different earlier communities of inquirers. By contrast, I argue, scientific realism predicts wholesale failures of global methodological commensurability and makes only highly qualified predictions of local methodological commensurability, even where global semantic commensurability obtains. Thus, the chauvinist tendencies internal to the positivist tradition are not only absent from the realist tradition but corrected within it.

I conclude, by analogy, that contemporary moral realism likewise embodies an appropriate antichauvinist conception of methodology, which is not to say that it is proof against chauvinism deriving from external social influences. Finally, I argue that the alternative constructivist relativist approach is in important respects chauvinist and uncritical. It holds the current stages in the relevant research traditions just as much immune from criticism as it does earlier and prescientific stages and it precludes the diagnosis of culpable methodological errors (culturally chauvinist errors among them) when these do occur, whether in the current stages of the relevant tradition or in its earlier stages. If it is otherwise defensible, realism then represents the preferred antidote to cultural chauvinism.

Finally, I further develop the theme suggested in the present essay that moral realism is an optimistic position. I argue that, given available evidence, the most plausible way in which the doctrine here identified as moral realism could prove to be wrong would be for the broad family of basic human goods to be incapable of a suitably strong homeostatic unity. The non-realist alternative I envision as most plausible would have "relativist" features and would entail the dependence of (some) moral truths upon the moral beliefs actually held in the relevant moral

communities. What I have in mind is a situation in which the following are both true:

1 (The relativist component.) The sorts of fundamental human goods typically recognized as relevant in moral reasoning lack the sort of homeostatic unity tacitly presupposed in moral discourse: there is no psychologically and socially stable way of ameliorating the conflicts between them and adjudicating those which remain which are satisfactory by reasonable prevailing moral standards. Instead, there are two (or more) stable ways of achieving homeostasis between those goods, each capable of sustaining a morality (and moral progress) of sorts, but in each (all) of them certain human goods are necessarily slighted with respect to the others in a way certainly unacceptable by contemporary moral standards. This plurality of morally compromised forms of moral homeostasis is not remediable by future moral, economic, or political developments: it reflects nonmaleable features of human nature. Most difficult disagreements in substantive moral philosophy reflect the tacit adherence of the disputants to one or the other of these stable "moralities" or their unsuccessful attempts to formulate viable alternatives comprising the best features of both (several), or both. Resolution of those disagreements requires that we recognize the conflation of moral standards that caused them and that we (relativistically) disambiguate our uses of moral terms.
2 (The belief dependence component.) Actually practiced stable moral arrangements will necessarily approximate one rather than the other (s) of the available stable forms of moral homeostasis. Insofar as we think of participants in such an arrangement as reasoning about the features of their own particular form of moral homeostasis when they engage in moral reasoning (as the first component suggests that we should), we will find that the truth of some of their important moral beliefs (so construed) will depend quite strongly on their having generally adopted the moral beliefs peculiarly appropriate to the tradition of moral practice in which they function. This will be so for two reasons. First, it will be generally true on their moral conception that the goodness (justice, permissibility…) of actions, practices, policies, character traits, etc. will depend upon the ways in which they contribute (or fail to contribute) to the satisfaction of fundamental human needs. Second, the nature of fundamental human needs (at least within the relevant moral communities) will be significantly determined by the moral beliefs held within the community: needs accorded a prominent role in the community's moral scheme will (in consequence of the effects of moral and social teaching on individual development) be more strongly felt than those needs assigned a less prominent role, even when, for those raised in (one of) the alternative sort(s) of moral community, the psychological importance of the needs might be reversed. Morally important human needs (and their relative importance) are thus significantly created by one's participation in

one or the (an) other sort of moral community: such communities make among their members the moral psychology appropriate to their moral practices. Because of the limitation of homeostasis between human goods specified in (1), no more encompassing moral psychology is possible.

It is, I think, evident why the conception of our moral situation envisioned in (1) and (2) is properly described as pessimistic. What I argue in *RMS* is that it is nevertheless only in a relatively uninteresting sense *non-realistic*. The dependence of the truth of moral propositions upon moral beliefs envisioned in (2) would be, I argue, an ordinary case of causal dependence and not the sort of logical dependence required by a constructivist conception of morals analogous to a Kuhnian neo-Kantian conception of the dependence of scientific truth on the adoption of theories or paradigms. The subject matter of moral inquiry in each of the relevant communities would be theory-and-belief-independent in the sense relevant to the dispute between realist and social constructivists.

The relativism envisioned in (1) would then, I argue, properly be seen as an ordinary realist case of partial denotation (in the sense of Field 1973). Thus, although the situation envisioned in (1) and (2) would refute moral realism as that doctrine is ordinarily construed (and as it is construed in the present essay), it would not undermine a generally realistic conception of moral language in favor of a constructivist one. The case for the former conception, I suggest, is quite strong indeed.

NOTES

An early version of this paper, incorporating the naturalistic treatments of the roles of reflective equilibrium and moral intuitions in moral reasoning and a naturalistic conception of the semantics of moral terms (but not the homeostatic property cluster formulation of consequentialism), was presented to the Philosophy Colloquium at Case-Western Reserve University in 1977. I am grateful to the audience at that colloquium for helpful criticisms which greatly influenced my formulation of later versions.

In approximately the version published here, the paper was presented at the University of North Carolina, the University of Chicago, Cornell University, the Universities of California at Berkeley and at Los Angeles, the University of Washington, Dartmouth College, and Tufts University. Papers defending the general homeostatic property-cluster account of natural definitions were presented at Oberlin, Cornell, and Stanford. Extremely valuable criticisms from the audiences at these universities helped me in developing the more elaborate defense of moral realism presented in *Realism and the Moral Sciences* and summarized in part 5 of the present essay.

My interest in the question of moral realism initially arose from my involvement in the anti-Vietnam War movement of the late 1960s and was sustained in significant measure by my participation in subsequent progressive

movements. I have long been interested in whether or not moral relativism played a progressive or a reactionary role in such movements; the present essay begins an effort to defend the latter alternative. I wish to acknowledge the important influence on my views of the Students for a Democratic Society (especially its Worker-Student Alliance Caucus), the International Committee against Racism, and the Progressive Labor party. Their optimism about the possibility of social progress and about the rational capacity of ordinary people have played an important role in the development of my views.

1 This is the only section of part 3 which advances naturalistic and realistic positions not already presented in the published literature. It represents a summary of work in progress. For some further developments see section 5.2.

2 Two points of clarification about the proposed homeostatic consequentialist definition of the good are in order. In the first place, I understand the homeostatic cluster which defines moral goodness to be social rather than individual. The properties in homeostasis are to be thought of as instances of the satisfaction of particular human needs among people generally, rather than within the life of a single individual. Thus, the homeostatic consequentialist holds not (or at any rate not merely) that the satisfaction of each of the various human needs within the life of an individual contributes (given relevant homeostatic mechanisms) to the satisfaction of the others in the life of that same individual. Instead, she claims that, given the relevant homeostatic mechanisms, the satisfaction of those needs for one individual tends to be conducive to their satisfaction for others, and it is to the homeostatic unity of human need satisfaction in the society generally that she or he appeals in proposing a definition of the good.

  Homeostatic consequentialism as I present it here is, thus, not a version of ethical egoism. I am inclined to think that individual well-being has a homeostatic property-cluster definition and thus that a homeostatic property-cluster conception of the definition of the good would be appropriate to the formulation of the most plausible versions of egoism, but I do not find even those versions very plausible and it is certainly not a version of egoism to which I mean to appeal in illustrating the proposed strategy for defending moral realism.

  Second, I owe to Judith Jarvis Thomson the observation that, strictly speaking, the homeostatic consequentialist conception of the good does not conform to the

more abstract account of homeostatic property-cluster definitions presented in section 3.8. According to that account, the homeostatically united properties and the definitionally relevant properties associated with the relevant mechanisms of homeostasis are all properties of the same kind of thing: organisms, let us say, in the case of the homeostatic property-cluster definition of a particular biological species.

By contrast, some of the properties which characterize human well-being and the mechanisms upon which its homeostatic unity depends are (on the homeostatic consequentialist conception) in the first instance properties of individuals, whereas others are properties of personal relations between individuals and still others are properties of large-scale social arrangements. Homeostatic unity is postulated between instances of the realization of the relevant properties in objects of different logical type.

It should be obvious that the additional logical complexity of the proposed homeostatic property cluster definition of the good does not vitiate the rebuttals offered here to anti-realist arguments. For the record, it seems to me that Professor Thomson's observation in fact applies to the actual case of species definitions as well: some of the homeostatically united properties and homeostatic mechanisms which define a species are in the first instance properties of individual organisms, some properties of small groups of organisms, some of larger populations (in the standard sense of that term), and some of the relations between such populations.

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