CHAPTER 13

Reading 13.1	**Kupfer, D.J., First, M.B., and Regier, D.E. (2002). Introduction. In *A Research Agenda for DSM-V* (ed. D.J. Kupfer, M.B. First, and D.E. Regier). Washington, DC: American Psychiatric Association, pp. xv–xvii (Extract).**
Reading 13.2	**Kupfer, D.J., First, M.B., and Regier, D.E. (2002). Introduction. In *A Research Agenda for DSM-V* (ed. D.J. Kupfer, M.B. First, and D.E. Regier). Washington, DC: American Psychiatric Association, pp. xviii–xix (Extract).**
Reading 13.3	**Hempel, C.G. (1994). Fundamentals of taxonomy. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press (Extract pp 317–318).**
Reading 13.4	**Hempel, C.G. (1994). Fundamentals of taxonomy. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press (Extract page 318).**
Reading 13.5	**Hempel, C.G. (1994). Fundamentals of taxonomy. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press (Extracts pp. 318–319).**
Reading 13.6	**Hempel, C.G. (1994). Fundamentals of taxonomy. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press (Extracts pp. 319–320).**
Reading 13.7	**Hempel, C.G. (1994). Discussion, Various Contributors (1961). In *Field Studies in the Mental Disorders* (ed. J. Zudin). New York: Grune and Stratton. (Extract page 34).**
Reading 13.8	**WHO (1974). *Glossary of Mental Disorders and Guide to their Classification, for use in Conjunction with the International Classification of Diseases*, 8th revision. Geneva: World Health Organization**
Reading 13.9	**Hempel, C.G. (1994). Fundamentals of taxonomy. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press (Extract pages 320–321).**

Reading 13.10 **Hempel, C.G. (1994). Fundamentals of taxonomy. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press (Extract page 322).**

Reading 13.11 **Szasz, T. (1972). The myth of mental illness. *The Myth of Mental Illness*. Paladin (Extract pages 14–17).**

Reading 13.12 **Williams, B. (1978). *Descartes: the project of pure inquiry*. London: Penguin (Extracts pp. 64–65, 244–246).**

Reading 13.13 **McDowell, J. (1998). Aesthetic value, objectivity, and the fabric of the world. In *Mind, Value and Reality*. Cambridge, MA: Harvard University Press, pp. 112–130 (Extract pp. 122–3).**

Reading 13.15 **Boyd, R. (1999). On the current status of scientific realism. In *The Philosophy of Science* (eds. R. Boyd, P. Gasker, and J.D. Trout). Cambridge, MA: MIT Press, pp. 195–222 (Extract p207).**

Reading 13.16 **Cartwright, N. (1999a). The reality of causes in a world of instrumental laws. In *The Philosophy of Science* (eds. R. Boyd, P. Gasker, and J.D. Trout). Cambridge, MA: MIT Press, p. 379–386 (Extract p380).**

Reading 13.17 **Hacking, I. (1999). Experimentation and Scientific realism. In *The Philosophy of Science* (eds. R. Boyd, P. Gasker, and J.D. Trout). Cambridge, MA: MIT Press, pp. 247–260 (Extract p248).**

Reading 13.18 **Fine, A. (1999). The natural ontological attitude. In *The Philosophy of Science* (eds. R. Boyd, P. Gasker, and J.D. Trout). Cambridge, MA: MIT Press, pp. 261–277 (Extract p271–2).**

Reading 13.1

EXERCISE 1

Extract from: Kupfer, D.J., First, M.B., and Regier, D.E. (2002). Introduction. In *A Research Agenda for DSM-V* (ed. D.J. Kupfer, M.B. First, and D.E. Regier). Washington, DC: American Psychiatric Association, (Extract pp. xv–xvii)

The chapters were produced under a partnership between the American Psychiatric Association (APA) and the National Institute of Mental Health (NIMH), with the goal of providing direction and potential incentives for research that could improve the scientific basis of future classifications. Given the relatively short time frame for generating breakthrough research findings between now and the probable publication of DSM-V in 2010, it is anticipated that some of the research agendas suggested in these chapters might not bear fruit until the DSM-VI or even DSAI-VII revision processes! Nonetheless, we feel that we cannot ignore this opportunity to identify and stimulate broad research fields that could fundamentally alter the limited classification paradigm now in use. Those of us who have worked for several decades to improve the reliability of our diagnostic criteria are now searching for new approaches to an understanding of etiological and pathophysiological mechanisms—an understating that can improve the validity of our diagnoses and the consequent power of our preventive and treatment interventions.

Background

There were two primary reasons for supporting designated work groups responsible for the development of these chapters: 1) to stimulate research that would enrich the empirical database before the start of the DSM-V revision process and 2) to devise a research and analytic agenda that would facilitate the integration of findings from research and experience in animal studies, genetics, neuroscience, epidemiology, clinical research, and cross-cultural clinical services—all of which would lead to the eventual development of an etiologically based, scientifically sound classification system.

Reading 13.2 EXERCISE 2

2 Extracts from: Kupfer, D.J., First, M.B., and Regier, D.E. (2002). Introduction. In *A Research Agenda for DSM-V* (ed. D.J. Kupfer, M.B. First, and D.E. Regier). Washington, DC: American Psychiatric Association, (Extracts pp. xviii–xix)

Extract 1: page XVIII

Need to Explore the Possibility of Fundamental Changes in the Neo-Kraepelinian Diagnostic Paradigm

The DSM-III diagnostic system adopted a so-called neo-Kraepelinian approach to diagnosis. This approach avoided organizing a diagnostic system around hypothetical but unproven theories about etiology in favor of a descriptive approach, in which disorders were characterized in terms of symptoms that could be elicited by patient report, direct observation, and measurement. The major advantage of adopting a descriptive classification was its improved reliability over prior classification systems using nonoperationalized definitions of disorders based on unproved etiological assumptions. From the outset, however, it was recognized that the primary strength of a descriptive approach was its ability to improve communication among clinicians and researchers, not its established validity.

Disorders in DSM-III were identified in terms of syndromes, symptoms that are observed in clinical populations to covary together in individuals. It was presumed that, as in general medicine, the phenomenon of symptom covariation could be explained by a common underlying etiology. As described by Robins and Guze (1970), the validity of these identified syndromes could be incrementally improved through increasingly precise clinical description, laboratory studies, delimitation of disorders, follow-up studies of outcome, and family studies. Once fully validated, these syndromes would form the basis for the identification of standard, etiologically homogeneous groups that would respond to specific treatments uniformly.

In the more than 30 years since the introduction of the Feighner criteria by Robins and Guze, which eventually led to DSM-III, the goal of validating these syndromes and discovering common etiologies has remained elusive. Despite many proposed candidates, not one laboratory marker has been found to be specific in identifying any of the DSM-defined syndromes. Epidemiologic and clinical studies have shown extremely high rates of comorbidities among the disorders, undermining the hypothesis that the syndromes represent distinct etiologies. Furthermore, epidemiologic studies have shown a high degree of short-term diagnostic instability for many disorders. With regard to treatment, lack of treatment specificity is the rule rather than the exception.

Extract 2: page XIX

Concerns have also been raised that researchers' slavish adoption of DSM-IV definitions may have hindered research in the etiology of mental disorders. Few question the value of having a well-described, well-operationalized, and universally accepted diagnostic system to facilitate diagnostic comparisons across studies and to improve diagnostic reliability. However, reification of DSM-IV entities, to the point that they are considered to be equivalent to diseases, is more likely to obscure than to elucidate research findings.

All these limitations in the current diagnostic paradigm suggest that research exclusively focused on refining the DSM-defined syndromes may never be successful in uncovering their underlying etiologies. For that to happen, an as yet unknown paradigm shift may need to occur. Therefore, another important goal of this volume is to transcend the limitations of the current DSM paradigm and to encourage research agenda that goes beyond our current ways of thinking to attempt to integrate information from a wide variety of sources and technologies.

Reading 13.3

EXERCISE 3

Extract from: Hempel, C.G. (1994). Fundamentals of taxonomy. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press (Extract pp 317–318).

In our discussion, we will distinguish, in a manner widely accepted in contemporary logic, between *concepts* and the *terms* that stand for them; for example, the term *soluble in alcohol*, which is a linguistic expression, stands for the concept of solubility in alcohol, which is a property of certain substances. Collectively, the terms used by empirical science in general or by one of its branches will be referred to as its *vocabulary*.

Description and Theoretical Systematization as Two Basic Functions of Scientific Concepts

Broadly speaking, the vocabulary of science has two basic functions: first, to permit an adequate *description* of the things and events that are the objects of scientific investigation; second, to permit the establishment of general laws or theories by means of which particular events may be *explained* and *predicted* and thus *scientifically understood*; for to understand a phenomenon scientifically is to show that it occurs in accordance with general laws or theoretical principles.

In fact, granting some oversimplification, the development of a scientific discipline may often be said to proceed from an initial "natural history" stage, which primarily seeks to describe the phenomena under study and to establish simple empirical generalizations concerning them, to subsequent more and more theoretical stages, in which increasing emphasis is placed upon the attainment of comprehensive theoretical accounts of the empirical subject matter under investigation. The vocabulary required in the early stages of this development will be largely observational; it will be chosen so as to permit the description of those aspects of the subject matter which are ascertainable fairly directly by observation. The shift toward theoretical systematization is marked by the introduction of new, "theoretical" terms, which refer to various theoretically postulated entities, their characteristics, and the processes in which they are involved; all these are more or less removed from the level of directly observable things and events. For example, the electric and magnetic fields of physics and the propagation of waves in them; chemical valences; molecular and atomic structures; elementary physical particles; quantum states: all these are typical of the sorts of things and processes to which the theoretical vocabulary of physics and of chemistry refers.

In medical science, the development from a predominantly descriptive to an increasingly theoretical emphasis is reflected, for example, in the transition from a largely symptomatological to a more and more etiological point of view. Etiology should not be conceived as dealing with the causes of disease in a narrow sense of that term. In the physical sciences, the search for causes in that sense has been replaced by a search for explanatory laws and theories; and etiology has been moving in the same direction. Indeed, the various theoretical approaches to disease have brought with them a variety of theoretical concepts. For example, the *Diagnostic and Statistical Manual* (1952) characterizes the concept of conversion reaction as follows:

> Instead of being experienced consciously . . . the impulse causing the anxiety is "converted" into functional symptoms in organs or parts of the body, usually those that are mainly under voluntary control. The symptoms serve to lessen conscious (felt) anxiety and ordinarily are symbolic of the underlying mental conflict. Such reactions usually meet immediate needs of the patient and are, therefore, associated with more or less obvious "secondary gain." (pp. 32–33)

Clearly, several of the terms used in this passage refer neither to directly observable phenomena, such as overt behavior, nor to responses that can be elicited by suitable stimuli but rather to theoretically assumed psychodynamic factors. Those terms have a distinct meaning and function only in the context of corresponding theory, just as the terms *gravitational field, gravitational potential*, and so on have a definite meaning and function only in the context of a corresponding theory of gravitation.

Reading 13.4 EXERCISE 4

Extract from: Hempel, C.G. (1994). Fundamentals of taxonomy. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press (Extract page 318).

Let us now survey some of the requirements which the two major objectives of description and theoretical systematization impose upon scientific concepts, and in particular upon the concepts used for classificatory purposes.

Empirical Import of Scientific Terms: Operational Definition

Science aims at knowledge that is *objective* in the sense of being inter-subjectively certifiable, independently of individual opinion or preference, on, the basis of data obtainable by suitable experiments or observations. This requires that the terms used in formulating scientific statements have clearly specified meanings and be understood in the same sense by all those who use them. One of the main objections to various types of contemporary psychodynamic theories, for example, is that their central concepts lack clear and uniform criteria of application and that, as a consequence, there are no definite and unequivocal ways of putting the theories to a test by applying them to concrete case.

Reading 13.5

EXERCISE 5

2 Extracts from: Hempel, C.G. (1994). Fundamentals of taxonomy. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press (Extracts pp. 318–19)

Extract 1: pages 318–319

A method that has been widely recommended to avoid this kind of deficiency is the use of so-called *operational definitions* for scientific terms. The idea was first set forth very explicitly by the physicist P. W. Bridgman in his 1927 book, *The Logic of Modern Physics*. An operational definition for a given term is conceived as providing objective criteria by means of which any scientific investigator can decide, for any particular case, whether the term does or does not apply. To this end, the operational definition specifies a testing "operation" *T* that can be performed on any case to which the given term could conceivably apply, and a certain outcome *O* of the testing operation, whose occurrence is to count as the criterion for the applicability of the term to the given case. Schematically, an operational definition of a scientific term *S* is a stipulation to the effect that *S* is to apply to all and only those cases for which performance of test operation *T* yields the specified outcome *O*. To illustrate: A simple operational definition of the term *harder than* as used in mineralogy might specify that a piece of mineral *x* is called harder than another piece of mineral *y* if the operation of drawing a sharp point of *x* under pressure across a smooth surface of *y* has as its outcome a scratch *y* whereas *y* does not thus scratch *x*

Extract 2: page 319

Bridgman argues in effect that if the meanings of the terms used in a scientific discipline are operationally specified then the assertions made by that discipline are capable of objective test. If, on the other hand, a proposed problem or hypothesis is couched in terms some of which are not thus tied to the firm ground of operationally ascertainable data, operationism rejects it as scientifically meaningless because no empirical test can have any bearing on it, so that the proposed formulation in turn can have no possible bearing on empirical subject matter and thus lacks empirical import (see for example, Bridgman, 1927, p. 28). The operationist insistence that meaningful scientific terms should have definite public criteria of application is thus closely akin to the empiricist insistence that meaningful scientific hypotheses and theories should be capable, in principle, of intersubjective test by observational data.

Reading 13.6

EXERCISE 6

2 Extracts from: Hempel, C.G. (1994). Fundamentals of taxonomy. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press (Extracts pp. 319–20)

Extract 1: page 319

Most diagnostic procedures used in medicine are based on operational criteria of application for corresponding diagnostic categories. There are exceptions, however. For example, it has been suggested that the occurrence of a characteristic "praecox-feeling" in the investigator may count as one indication of dementia praecox in the patient he is examining; but this idea does not meet the requirements of operationism because the occurrence of the specified outcome, the praecox-feeling in regard to a given patient, is *not* independent of the examiner.

Extract 2: pages 319–320

The methodological tenets of operationism and empiricism have met with especially keen, and largely favorable, interest in psychology and sociology. Here, an operational specification of meaning is often achieved by formulating definite testing procedures that are to govern the application of terms such as *IQ* and of terms pertaining to various aptitudes and attitudes.

The concern of many psychologists and social scientists with the *reliability* of their terms reflects the importance attributed to objectivity of use. The reliability of a concept (or of the corresponding term) is usually understood as an indicator of two things: the consistency shown in its use by one observer, and the agreement in the use made of it by different observers. The former feature is often expressed in terms of the correlation between the judgments made by the same observer when he is asked to judge the same case on several occasions; the latter feature is expressed in terms of the correlations obtaining among the judgments of several observers judging the same cases, the *judgments* here referred to being made in terms of the concept whose reliability is under consideration.

Reading 13.7 EXERCISE 8

Extract from: Discussion, Various Contributors (1961). In *Field studies in the Mental Disorders* (ed. J. Zudin). New York: Grune and stratton. (Extract p 34).

It seems to me that we might properly distinguish between public classifications and private classifications. Public classifications are the kind that are most valuable for epidemiological work, since we need to make comparisons of findings in different countries, and unless there is uniformity of usage, that is impractical. Therefore, the public classification is likely to be one that will not lead to any ambiguity, whereas the private classification used by smaller groups for their own purposes may very well be only suitable for their use, since they have a uniform background, perhaps, and have agreed among themselves as to the usage of the terms.

The application of this lies in the classification based on theory, which is the controversial part, really, of Dr. Hempel's presentation. In psychiatry to make a classification based on theory is what we all would like, and what we believe we cannot at the moment attain—because, as Dr. Hempel clearly stated, the requirements are not met by any of the theories prevailing in psychiatry at the present time.

Therefore I would suggest that for the purpose of public classification we should eschew categories based on theoretical concepts and restrict ourselves to the operational, descriptive type of classification, whereas, for the purposes of certain groups, the private classification, based on a theory which seems a workable, profitable one, may be very appropriate.

Reading 13.8

EXERCISE 9

WHO (1974). *Glossary of Mental Disorders and Guide to their Classification, for use in Conjunction with the International Classification of Diseases*, 8th revision. Geneva: World Health Organization

Foreword

Compiling glossaries has been a respectable profession since the 2nd century B.C., as the article in the Encyclopaedia Britannica makes abundantly clear. This is not surprising when the multifarious needs for classification and interpretation are considered. But there is a reverse side to the coin: to "gloss over", or "to gloze", a term derived from the same root as glossary, denotes a disreputable activity. "Classification" likewise has a pejorative as well as a respectable flavour. Psychiatric usage of the relevant terms attests their ambiguity: "mere labelling", "the neat complacency of classification", "nosological stamp collecting", "a medical hortus siccus". Such damning phrases arise in part from revulsion against the excesses to which classification was pushed in the late 18th and early 19th century.

A modern psychiatric glossarist has to cope basically with the same uncertainties and pitfalls as beset the compilers of other medical classifications, but they are aggravated by hazards arising from the paucity of the objective data on which definition and diagnosis must depend. He has to contrive appropriate criteria for differentiating one disease from another; ideally he aims at constructing a consistent schema into which they will all fit. Such a schema may be based on clinical patterns (syndromes) or on clinical course; it may be psychodynamic, etiological (genetic), or pathological. And, since diseases are in any case abstract concepts, it is no wonder that the disease constructs which psychiatrists work with have shimmering outlines and overlap. Observer variation is disconcertingly in evidence; reliability is too low for scientific comfort; discrepancies may be in some cases lessened, in others minimized, depending on whether they arise from inexact perception, personal bias, or divergency of the nosological systems or terms used.

The picture is no longer black. The glossary put forward here, when faithfully applied, reduces the scope of error. It would seem, however, that accurate observation is still the gate that needs the closest guard. A. R. Feinstein put it bluntly: "the current psychiatric debates about systems of classification, the many hypothetical and unconfirmed schemas of "psychodynamic mechanisms", and the concern with etiological inference rather than observational evidence are nosologic activities sometimes reminiscent of those conducted by the mediaeval taxonomists." Since the disorders listed in this glossary are identified by criteria that are predominantly descriptive, its use should encourage an emphasis on careful observation.

This glossary still contains some compromises and anomalies, but the emergence of an agreed version from an international group of collaborators and advisers of such diversity of background and outlook was possible only because of a generous spirit of cooperation and common recognition of an urgent need for better means of communication.

Sir Aubrey J. Lewis, M.D., F.R.C.P.
1974

Reading 13.9

EXERCISE 10

From: Hempel, C.G. (1994). Fundamentals of taxonomy. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press (Extract pages 320–321).

The operationist emphasis on clear and precise-public criteria of application for scientific terms is no doubt sound and salutary. But the customary formulations of operationism require certain qualifications, two of which will be briefly mentioned here because they are relevant to the subject matter of this paper.

First, the operational criteria of application available for a term often amount to less than a full definition. For example, criteria of application for the term *temperature* may be specified by reference to the operation of putting a mercury thermometer into the appropriate place and noting its response; or by similar use of an alcohol thermometer, or of a thermocouple, and so on. These instruments have different, though partly overlapping, ranges within which they can be used, and none covers the full range of theoretically possible temperatures. Each of them thus provides a *partial definition*, or better, *a partial criterion of application*, for the term under consideration (or for the corresponding concept). Such partial criteria of application for the terms occurring in a given hypothesis or theory will often suffice to make an empirical test possible. Indeed, there are reasons to doubt the possibility of providing *full* operational definitions for all theoretical terms in science, and the operationist program needs therefore to be liberalized, so as to call only for the specification of partial criteria of application.

Second, if the insistence on an *operational* specification of meaning for scientific terms is not to be unduly restrictive, the idea of operation has to be taken in a very liberal sense which does not require manipulation of the objects under consideration: the mere observation of an object, for example, must be allowed to count as an operation, for the criteria of application for a term may well be specified by reference to certain characteristics which can be ascertained without any testing procedure more complicated than direct observation. Consider, for example, the check list of characteristics which Sheldon gives for dominant endomorphy. That list includes such directly observable features as roundness and softness of body; central concentration of mass; high, square shoulders with soft contours; short neck, short tapering limbs. This is a satisfactory way of determining the concept of predominant endomorphy and thus the class of predominantly endomorphic individuals, provided that the terms used to specify the distinctive characteristics of endomorphs have a reasonably precise meaning and are used, by all investigators concerned, with high intersubjective uniformity—that is, provided that for any given subject there is a high degree of agreement among different observers as to whether or not the subject has soft body contours, a short neck, tapering limbs, and so on. And indeed, Bridgman's insistence on operational tests and their outcomes is no doubt basically aimed at making sure that the criteria of application for scientific concepts be expressed in terms which have a very high uniformity of usage.

It would be unreasonable to demand, however, that *all* of the terms used in a given scientific discipline be given an operational specification of meaning, for then the process of specifying the meanings of the defining terms, and so forth, would lead to an infinite regress. In any definitional context (quite in dependently of the issue of operationism), some terms must be antecedently understood; and the objectivity of science demands that the terms which thus serve as a basis for the introduction of other scientific terms should be among those used with a high degree of uniformity by different investigators in the field.

Reading 13.10

EXERCISE 11

From: Hempel, C.G. (1994). Fundamentals of taxonomy. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press (Extract page 322).

In the interest of the objective, it may be worth considering whether, or to what extent, criteria with valuational overtones are used in the specification of psychiatric concepts. Consider, for example, the characterization of the category Inadequate Personality as given in tie *Diagnostic and Statistical Manual* (1952, p. 35): "Such individuals are characterized by inadequate response to intellectual, emotional, social, and physical demands. They are neither physically nor mentally grossly deficient on examination, but they do show inadaptability, ineptness, poor judgment, lack of physical and emotional stamina, and social incompatibility." Such notions as inadequacy of response, inadaptability, ineptness, and poor judgment clearly have valuational aspects, and it is to be expected that their use in concrete cases will be influenced by the idiosyncrasies of the investigator. This will reduce the reliability of these concepts and of those for which they serve as partial criteria of application.

Reading 13.11

EXERCISE 12

Extract from: Szasz, T. (1972). The myth of mental illness. *The Myth of Mental Illness*. Paladin (Extract pages 14–17).

The term 'mental illness' is also widely used to describe something quite different from a disease of the brain. Many people today take it for granted that living is an arduous affair. Its hardship for modern man derives, moreover, not so much from a struggle for biological survival as from the stresses and strains inherent in the social intercourse of complex human personalities. In this context, the notion of mental illness is used to identify or describe some feature of an individual's so-called personality. Mental illness—as a deformity of the personality, so to speak—is then regarded as the cause of human disharmony. It is implicit in this view that social intercourse between people is regarded as something inherently harmonious, its disturbance being due solely to the presence of 'mental illness' in many people. Clearly, this is faulty reasoning, for it makes the abstraction 'mental illness' into a cause of, even though this abstraction was originally created to serve only as a shorthand expression for, certain types of human behaviour. It now becomes necessary to ask: What kinds of behaviour are regarded as indicative of mental illness, and by whom?

The concept of illness, whether bodily or mental, implies deviation from some clearly defined norm. In the case of physical illness, the norm is the structural and functional integrity of the human body. Thus, although the desirability of physical health, as such, is an ethical value, what health is can be stated in anatomical and physiological terms. What is the norm, deviation from which is regarded as mental illness? This question cannot be easily answered. But whatever this norm may be, we can we certain of only one thing: namely, that it must be stated in terms of psychosocial, ethical, and legal concepts. For example, notions such as 'excessive repression' and 'acting out an unconscious impulse' illustrate the use of psychological concepts for judging so-called mental health and illness. The idea that chronic hostility, vengefulness, or divorce are indicative of mental illness is an illustration of the use of ethical norms (that is, the desirability of love, kindness, and a stable marriage relationship). Finally, the widespread psychiatric opinion that only a mentally ill person would commit homicide illustrates the use of a legal concept as a norm of mental health. In short, when one speaks of mental illness, the norm from which deviation is measured is a *psychosocial and ethical* standard. Yet the remedy is sought in terms of *medical* measures that—it is hoped and assumed—are free from wide differences of ethical value. The definition of the disorder and the terms in which its remedy are sought are therefore at serious odds with one another. The practical significance of this covert conflict between the alleged nature of the defect and the actual remedy can hardly be exaggerated.

Having identified the norms used for measuring deviations in cases of mental illness, we shall now turn to the question, Who defines the norms and hence the deviation? Two basic answers may be offered: First, it may be the person himself—that is, the patient—who decides that he deviates from a norm; for example, an artist may believe that he suffers from a work inhibition; and he may implement this conclusion by seeking help *for himself* from a psychotherapist. Second, it may be someone other than the 'patient' who decides that the latter is deviant—for example, relatives, physicians, legal authorities, society generally; a psychiatrist may then be hired by persons other than the 'patient' to do something *to him* in order to correct the deviation.

These considerations underscore the importance of asking the question, Whose agent is the psychiatrist? and of giving a candid answer to it. The psychiatrist (or non-medical mental health worker) may be the agent of the patient, the relatives, the school, the military services, a business organization, a court of law, and so forth. In speaking of the psychiatrist as the agent of these persons or organizations, it is not implied that his moral values, or his ideas and aims concerning the proper nature of remedial action, must coincide exactly with those of his employer. For example, a patient in individual psychotherapy may believe that his salvation lies in a new marriage; his psychotherapist need not share this hypothesis. As the patient's agent, however, he must not resort to social or legal force to prevent the patient from putting his beliefs into action. If his *contract* is with the patient, the psychiatrist (psychotherapist) may disagree with him or stop his treatment, but he cannot engage others to obstruct the patient's aspirations. Similarly, if a psychiatrist is retained by a court to determine the sanity of an offender, he need not fully share the legal authorities' values and intentions in regard to the criminal, nor the means deemed appropriate for dealing with him; such a psychiatrist cannot testify, however, that the accused is not insane, but that the legislators are—for passing the law that decrees the offender s actions illegal. This sort of opinion could be voiced, of course—but not in a court-room, and not by a psychiatrist who is there to assist the court in performing its daily work.

To recapitulate: In contemporary social usage, the finding of mental illness is made by establishing a deviance in behaviour from certain psychosocial, ethical, or legal norms. The judgement may be made, as in medicine, by the patient, the physician (psychiatrist), or others. Remedial action, finally, tends to be sought in a therapeutic—or covertly medical—framework. This creates a situation in which it is claimed that psychosocial, ethical, end legal deviations can be corrected by medical action. Since medical interventions are designed to remedy only medical problems, it is logically absurd to expect that they will help solve problems whose very existence have been defined and established on non-medical grounds.

Reading 13.12

EXERCISE 13

2 Extracts from: Williams, B. (1978). *Descartes: the project of pure inquiry*. London: Penguin (Extracts pp. 64–65, 244–246).

Extract 1: pages 64–65

Knowledge does have a problematical character, and does have something in it which offers a standing invitation to scepticism. Attempts to uncover this just in terms of the relations between the concepts knowledge, doubt, certainty and so forth seem nevertheless to fail, and characteristically to rely, like the last argument, on thoroughly implausible or question-begging assumptions. The source of the invitation lies deeper. What exactly it is, is a difficult question; I will try to sketch an approach which seems to me to lead in the direction of the source. This starts from a very basic thought, that if knowledge is what it claims to be, then it is knowledge of a reality which exists independently of that knowledge, and indeed (except for the special case where the reality known happens itself to be some psychological item) independently of any thought or experience. Knowledge is of what is there *any way*. One might suppose this thought to be incontestable, but its consequences can seem to be both demanding and puzzling. Suppose *A* and *B* each claims to have some knowledge of the world. Each has some beliefs, and moreover has experiences of the world, and ways of conceptualizing it, which have given rise to those beliefs and are expressed in them: let us call all of this together his *representation* of the world (or part of the world). Now with respect to their supposed pieces of knowledge, *A*'s and *B*'s representations may well differ. If what they both have is knowledge, then it seems to follow that there must be some coherent way of understanding why these representations differ, and how they are related to one another. One very primitive example of this would be that *A* and *B* were in different places; another might be that they were both correctly predicting the movements of the planets, but by different, geometrically equivalent, systems. In either case, a story can be told which explains how A's and B's can each be perspectives on the same reality. To understand this story, one needs to form a conception of the world which *contains A* and *B* and their representations; *A* and *B* are not debarred from taking this standpoint themselves, but it involves their standing back from their original ways of representing these aspects of the world. But this process, it seems, can be continued. For if *A* or *B* or some other party comes in this way to understand these representations and their relation to the world, this will be because he has given them a place in some more inclusive representation; but this will still itself be a representation, involving its own beliefs, conceptualizations, perceptual experiences and assumptions about the laws of nature. If this is knowledge, then we must be able to form the conception, once more, of how this would be related to some other representation which might, equally, claim to be knowledge; indeed we must be able to form that conception with regard to *every* other representation which might make that claim. If we cannot form that conception, then it seems that we do not have any adequate conception of the reality which is there 'anyway', the object of any representation which is knowledge; but that conception appeared at the beginning as basic to the notion of knowledge itself. That conception we might call the absolute conception of reality. If knowledge is possible at all, it now seems, the absolute conception must be possible too.

What does that require? Here what was a natural, if very abstract, progression seems to have led to a basic dilemma. On the one hand, the absolute conception might be regarded as entirely empty, specified only as 'whatever it is that these representations represent'. In this case, it no longer does the work that was expected of it, and provides insufficient substance to the conception of an independent reality; it slips out of the picture, leaving us only with a variety of possible representations to be measured against each other, with nothing to mediate between them. On the other hand, we may have some determinate picture of what the world is like independent of any knowledge or representation in thought; but then that is open to the reflection, once more, that that is only one particular representation of it, our own, and that we have no independent point of leverage for raising this into the absolute representation of reality.

Extract 2: pages 244–246

If we do think that we have reason lo lay aside, with regard to the conception of an unobserved world, descriptions in terms of secondary qualities, what reason have we to think that we can do better with primary qualities, the properties of the world as characterized by natural science? Can we really distinguish between some concepts or propositions which figure in the conception of the world without observers, and others that do not? Are not all our concepts ours, including those of physics? Of course: but there is no suggestion that we should try to describe a world without ourselves using any concepts, or without using concepts which we, human beings, can understand. The suggestion is that there are possible descriptions of the world using concepts which are not peculiarly ours, and not peculiarly relative to our experience. Such a description would be that which would be arrived at, as C. S. Peirce put it, if scientific enquiry continued long enough; it is the content of that 'final opinion' which Peirce believed that enquiry would inevitably converge upon, a 'final opinion . . . independent not indeed of thought in general, but of all that is arbitrary and individual in thought'.[17] The representation of the world that would be so arrived at must, if it is to fill,

17. From *A Critical Review of Berkeley' Idealism: in Charles S. Peirce, Selected Writings* (*Values in a World of Chance*), ed. Philip P. Wiener (Dover, edition. New York, 1966), p. 82. Cf. also the passage from Peirce quoted by Wiggins, op. cit. Peirce's formulations of the idea tend to make the convergent progress of enquiry sound more simply cumulative, linear, and merely inevitable, than we have any reason, or need, to believe it could be.

the role required by the traditional distinction between primary and secondary qualities, be more than some minimal picture which merely offers the most that a set of very different observers could arrive at, like some cosmic United Nations resolution. Its power to be more than this would lie in its being explanatory, and in the way in which it would be explanatory. The picture, that offered by natural science, would explain the phenomena: it would explain them, moreover, *even as they present themselves in other, more local, representations*. It is this consideration that gives the content to the idea, essential to the traditional distinction, that the scientific picture presents the reality of which the secondary qualities, as perceived, are appearances.[18]

But this means that we need more than a conception of the world without observers; we need an equally impartial conception that includes not just the material world, as so far characterized, but its observers as well. The scientific representation of the material world can be the point of convergence of the Peircean enquirers precisely because it does not have among its concepts any which reflect merely a local interest, taste or sensory peculiarity. However, while these various particular modes of experience are not projected on to the description of the world in this representation, nevertheless the experiences themselves, the tastes and interests from which the investigators have abstracted, do actually exist, are something in the world. So the representation of the world without consciousness must be capable of being extended so as to have a place for consciousness within the world; moreover it must be extended in such a way as to relate the various points of view comprehensibly to each other and to the material world. This extended conception will then be that absolute conception of reality, the idea of which was introduced in Chapter 2 as something, putatively at least, presupposed by the possibility of knowledge.[19] The absolute conception should explain, or at least make it possible to explain, how the more local representations of the world can come about—it is this that would enable us to relate them to each other, and to the world as it is independently of them. For instance, it should enable us to understand how certain things can seem green to us and not to others. Moreover, this conception of the world must make it possible to explain how it itself can exist. This conception is not something transcendental, but is an historical product of consciousness in the world, and it must at least yield a comprehension of men and of other rational creatures as capable of achieving that conception. It thus involves a theory of knowledge and of error: it serves to explain how members of these species might come to have or fail to have a true conception of themselves and of the world.

It is not less than this, I think—or not much less[20]—that is involved in the distinction between primary and secondary qualities, where that is interpreted in the traditional and the only interesting way, as claiming that it is primary and not secondary qualities that characterize the material world as it really is.

18. This is the aspect which is precisely left out in Ryle's unfortunate analogy between the physicist and the accountant of a library, commonsense perception being analogous to the reading and appreciation of the books (*Dilemmas* (Cambridge, 1954), Chapter 5). The hardiest historical materialist would not claim that the accountant could explain the contents of the books from his figures.

19. It can now be seen that this presupposition does not mean that each thing we know must figure, at least as ii stands, in the absolute conception of things: we can know that something is green. Rather, our knowledge as a whole must be rooted in that conception, and white some of our knowledge must represent the world as it (absolutely) is, other things we know must merely be comprehensibly related to that conception. This is already a very strung requirement.

Reading 13.13

EXERCISE 15

Extract from: McDowell, J. (1998). Aesthetic value, objectivity, and the fabric of the world. In *Mind, Value and Reality*. Cambridge, MA: Harvard University Press, pp. 112–130 (Extract pp. 122–3).

4. The absolute conception owes what credentials it has, as the frame for all reflection about our cognitive relations with the world, to its explanatory aspirations. The conception of the world as it is in itself is not supposed to be a mere highest common factor, "the most that a set of very different observers could arrive at, like some cosmic United Nations resolution" (p. 244). Mere consensus could not by itself justify the claim to present the reality of which the non-agreed residues are appearances. The claim of the conception of the world as it is independently of observers (the objective conception of the world) to monopolize the "reality" side of the distinction between reality and appearance depends on the possibility of extending it so as to become the absolute conception: that is, extending it so as to embrace and explain the particular points of view it transcends. (See especially pp. 245–6.)

But there is room for doubt about this extension. Can the expansion to embrace the various local points of view be undertaken in the objective spirit that would be required for its upshot to sustain the correlation between objectivity and reality? Or would it necessitate—surely defeating the project—that we lapse back from trying to transcend particular points of view, in order to achieve an undistorted picture of reality as it is in itself, into unregenerately occupying the points of view that were to be transcended?

Take the case of colour. Williams considers an account of colour properties as dispositions to look red, green, etc., in certain circumstances; but he expresses scepticism about it, on the ground that "it leaves us with the discouraging task of explaining '. . . looks green' in some way which does not presuppose any prior understanding of '. . . is green' " (p. 243). This pessimism seems well placed; and what it amounts to is the thought that the content of the appearances to be explained in this case—how it is that things appear from the point of view in question—is not so much as intelligible except on the basis of occupying the point of view. (Not that one has to suppose always that things are coloured the way they appear to be. But only someone who has, or at least might have, a use for ". . . is green" can understand what it is for something to look green.) Thus an explanation of the appearances in this case would have to address itself exclusively to occupants of the point of view in question, on pain of unintelligibility in its formulation of its explicandum. And how could such an explanation help show us how to transcend that point of view, let alone help convince us that transcending it is necessary if we are to achieve a correct conception of our relation to reality?

Williams writes of "understanding . . . , at a general and reflective level, why things *appear variously coloured* to various observers" (p. 242, my emphasis). Of course there is no disputing the possibility of such understanding, on the basis of information about the behaviour of light and the construction of visual equipment. But it seems to be an illusion to suppose that such understanding could still be forthcoming after we had definitively left behind a view of the world that represents colours as properties things have (it would be a mere pleonasm to say "really have"): in such a position, we would no longer understand what it was that we were supposed to be explaining. And it is mysterious how we are to be sustained in our resolve to abandon, or at least disparage, a point of view by a thought that is not thinkable anywhere else.

Reading 13.15

EXERCISE 18

Extract from: Boyd, R. (1999). On the current status of scientific realism. In *The Philosophy of Science* (eds. R. Boyd, P. Gasker, and J.D. Trout). Cambridge, MA: MIT Press, pp. 195–222 (Extract p207).

6. Defending Scientific Realism

I have elsewhere (Boyd 1972, 1973, 1979, 1982, unpublished (a), (b)) offered a defense of scientific realism against empiricist antirealism which proceeds by proposing that a realistic account of scientific theories is a component in the only scientifically plausible explanation for the instrumental reliability of scientific methodology. What I propose to do here is to summarize this defense very briefly and to indicate how it also constitutes a defense of scientific realism against constructivist criticisms, and how it avoids the weaknesses in the traditional rebuttals to antirealist arguments.

The proposal that scientific realism might be required in order to adequately explain the instrumental reliability of scientific methodology can be motivated by reexamining the principal constructivist argument against scientific realism (2a in table 11.1). The constructivist asks, "What must the world be like in order that a methodology so theory-dependent as ours could constitute a way of finding out what's true?" She answers: "The world would have to be largely defined or constituted by the theoretical tradition which defines that methodology". It is clear that another answer is at least possible: the world might be one in which the laws and theories embodied in our actual theoretical tradition are approximately true. In that case, the methodology of science might progress dialectically. Our methodology, based on approximately true theories, would be a reliable guide to the discovery of new results and the improvement of older theories. The resulting improvement in our knowledge of the world would result in a still more reliable methodology leading to still more accurate theories, and so on (see Boyd 1982).

What I have argued in the works cited above is that this conception of the enterprise of science provides the only scientifically plausible explanation for the instrumental reliability of the scientific method. In particular, I argue that the reliability of theory-dependent judgments of projectibility and degrees of confirmation can only be satisfactorily explained on the assumption that the theoretical claims embodied in the background theories which determine those judgments are relevantly approximately true, and that scientific methodology acts dialectically so as to produce in the long run an increasingly accurate theoretical picture of the world. Since logical empiricists accept the instrumental reliability of actual scientific methodology, this defense of realism represents a cogent challenge to logical empiricist antirealism.

Reading 13.16

EXERCISE 19

Extract from: Cartwright, N. (1999a). The reality of causes in a world of instrumental laws. In *The Philosophy of Science* (eds. R. Boyd, P. Gasker, and J.D. Trout). Cambridge, MA: MIT Press, p. 379–386 (Extract p380).

1. Explaining by Causes

Following Bromberger, Scriven, and others, we know that there are various things one can be doing in explaining. Two are of importance here: in explaining a phenomenon one can cite the causes of that phenomenon; or one can set the phenomenon in a general theoretical framework. The framework of modem physics is mathematical, and good explanations will generally allow us to make quite precise calculations about the phenomena we explain. Rene Thom remarks the difference between these two kinds of explanation, though he thinks that only the causes really explain: 'DesCartes with his vortices, his hooked atoms, and the like explained everything and calculated nothing; Newton, with the inverse square of gravitation, calculated everything and explained nothing'.

Unlike Thom, I am happy to call both explanation, so long as we do not illicitly attribute to theoretical explanation features that apply only to causal explanation. There is a tradition, since the time of Aristotle, of deliberately conflating the two. But I shall argue that they function quite differently in modem physics. If we accept Descartes's causal story as adequate, we must count his claims about hooked atoms and vortices true. But we do not use Newton's inverse square law as if it were either true or false.

One powerful argument speaks against my claim and for the truth of explanatory laws—the *argument from coincidence*. Those who take laws seriously tend to subscribe to what Gilbert Harman has called inference to the best explanation. They assume that the fact that a law *explains* provides evidence that the law is true. The more diverse the phenomena that it explains, the more likely it is to be true. It would be an absurd coincidence if a wide variety of different kinds of phenomena were all explained by a particular law, and yet were not in reality consequent from the law. Thus the argument from coincidence supports a good many of the inferences we make to best explanations.

The method of inference to the best explanation is subject to an important constraint, however—the requirement of non-redundancy. We can infer the truth of an explanation only if there are no alternatives that account in an equally satisfactory way for the phenomena. In physics nowadays, I shall argue, an acceptable causal story is supposed to satisfy this requirement. But exactly the opposite is the case with the specific equations and models that make up our theoretical explanations. There is redundancy of theoretical treatment, but not of causal account.

There is, I think, a simple reason for this: causes make their effects happen. We begin with a phenomenon which, relative to our other general beliefs, we think would not occur unless something peculiar brought it about. In physics we often mark this belief by labeling the phenomena as effects—the Sorbet effect, the Zeeman effect, the Hall effect. An effect needs something to bring it about, and the peculiar features of the effect depend on the particular nature of the cause, so that—in so far as we think we have got it right—we are entitled to infer the character of the cause from the character of the effect.

But equations do not bring about the phenomenological laws we derive from them (even if the phenomenological laws are themselves equations). Nor are they used in physics as if they did. The specific equations we use to treat particular phenomena provide a way of casting the phenomena into the general framework of the theory. Thus we are able to treat a variety of disparate phenomena in a similar way, and to make use of the theory to make quite precise calculations. For both of these purposes it is an advantage to multiply theoretical treatments.

Reading 13.17

EXERCISE 20

Extract from: Hacking, I. (1999). Experimentation and Scientific realism. In *The Philosophy of Science* (eds. R. Boyd, P. Gasker, and J.D. Trout). Cambridge, MA: MIT Press, pp. 247–260 (Extract p248).

In this paper I leave aside questions of methodology, history, taxonomy, and the purpose of experiment in natural science. I turn to the purely philosophical issue of scientific realism. Simply call it 'realism' for short. There are two basic kinds: realism about entities and realism about theories. There is no agreement on the precise definition of either. Realism about theories says that we try to form true theories about the world, about the inner constitution of matter and about the outer reaches of space. This realism gets its bite from optimism: we think we can do well in this project and have already had partial success. Realism about entities—and I include processes, states, waves, currents, interactions, fields, black holes, and the like among entities—asserts the existence of at least some of the entities that are the stock in trade of physics.

The two realisms may seem identical. If you believe a theory, do you not believe in the existence of the entities it speaks about? If you believe in some entities, must you not describe them in some theoretical way that you accept? This seeming identity is illusory. *The vast majority of experimental physicists are realists about entities but not about theories*. Some are, no doubt, realists about theories too, but that is less central to their concerns.

Experimenters are often realists about the entities that they investigate, but they do not have to be so. R. A. Millikan probably had few qualms about the reality of electrons when he set out to measure their charge. But he could have been skeptical about what he would find until he found it. He could even have remained skeptical. Perhaps there is a least unit of electric charge, but there is no particle or object with exactly that unit of charge. Experimenting on an entity does not commit you to believing that it exists. Only manipulating an entity, in order to experiment on something else, need do that,

Moreover, it is not even that you use electrons to experiment on something else that makes it impossible to doubt electrons. Understanding some causal properties of electrons, you guess how to build a very ingenious, complex device that enables you to line up the electrons the way you want, in order to see what will happen to something else. Once you have the right experimental idea, you know in advance roughly how to try to build the device, because you know that this is the way to get the electrons to behave in such and such a way. Electrons are no longer ways of organizing our thoughts or saving the phenomena that have been observed. They are now ways of creating phenomena in some other domain of nature. Electrons are tools.

There is an important experimental contrast between realism about entities, and realism about theories. Suppose we say that the latter is belief that science aims at true theories. Few experimenters will deny that. Only philosophers doubt it. Aiming at the truth is, however, something about the indefinite future. Aiming a beam of electrons is using present electrons. Aiming a finely tuned laser at a particular atom in order to knock off a certain electron to produce an ion is aiming at present electrons. There is, in contrast, no present set of theories that one has to believe in. If realism about theories is a doctrine about the aims of science, it is a doctrine laden with certain kinds of values. If realism about entities is a matter of aiming electrons next week or aiming at other electrons the week after, it is a doctrine much more neutral between values. The way in which experimenters are scientific realists about entities is entirely different from ways in which they might be realists about theories.

Reading 13.18

EXERCISE 21

Extract from: Fine, A. (1999). The natural ontological attitude. In *The Philosophy of Science* (eds. R. Boyd, P. Gasker, and J.D. Trout). Cambridge, MA: MIT Press, pp. 261–277 (Extract p 271–2).

What then of the realist, what does he add to his core acceptance of the results of science as really true? My colleague, Charles Chastain, suggested what I think is the most graphic way of stating the answer—namely, that what the realist adds on is a desk-thumping, foot-stamping shout of "Really!" So, when the realist and antirealist agree, say, that there really are electrons and that they really carry a unit negative charge and really do have a small mass (of about 9.1×10^{-28} grams), what the realist wants to add is the emphasis that all this is really so. "There really are electrons, really!" This typical realist emphasis serves both a negative and a positive function. Negatively, it is meant to deny the additions that the antirealist would make to that core acceptance which both parties share. The realist wants to deny, for example, the phenomenalistic reduction of concepts or the pragmatic conception of truth. The realist thinks that these addenda take away from the substantiality of the accepted claims to truth or existence. "No," says he, "they *really* exist, and not in just your diminished antirealist sense." Positively, the realist wants to explain the robust sense in which *he* takes these claims to truth or existence; namely, as claims about reality—what is really, really the case. The full-blown version of this involves the conception of truth as correspondence with the world, and the surrogate use of approximate truth as near-correspondence. We have already seen how these ideas of correspondence and approximate truth are supposed to explain what *makes* the truth *true* whereas, in fact, they function as mere trappings, that is, as superficial decorations that may well attract our attention but do not compel rational belief. Like the extra "really," they are an arresting foot thump and, logically speaking, of no more force.

It seems to me that when we contrast the realist and the antirealist in terms of what they each want to add to the core position, a third alternative emerges—and an attractive one at that. It is the core position itself, *and all by itself*. If I am correct in thinking that, at heart, the grip of realism only extends to the homely connection of everyday truths with scientific truths, and that good sense dictates our acceptance of the one on the same basis as our acceptance of the other, then the homely line makes the core position, all by itself, a compelling one, one that we ought to take to heart. Let us try to do so and see whether it constitutes a philosophy, and an attitude toward science, that we can live by.

The core position is neither realist nor antirealist; it mediates between the two. It would be nice to have a name for this position, but it would be a shame to appropriate another "ism" on its behalf, for then it would appear to be just one of the many contenders for ontological allegiance. I think it is not just one of that crowd but rather, as the homely line behind it suggests, it is for commonsense epistemolog—the natural ontological attitude. Thus, let me introduce the acronym NOA (pronounced as in "Noah"), for *natural ontological attitude*, and, henceforth, refer to the core position under that designation.

To begin showing how NOA makes for an adequate philosophical stance toward science, let us see what it has to say about ontology. When NOA counsels us to accept the results of science as true, I take it that we are to treat truth in the usual referential way, so that a sentence (or statement) is true just in case the entities referred to stand in the referred-to relations. Thus, NOA sanctions ordinary referential semantics and commits us, via truth, to the existence of the individuals, properties, relations, processes, and so forth referred to by the scientific statements that we accept as true. Our belief in their existence will be just as strong (or weak) as our belief in the truth of the bit of science involved, and degrees of belief here, presumably, will be tutored by ordinary relations of confirmation and evidential support, subject to the usual scientific canons. In taking this referential stance, NOA is not committed to the progressivism that seems inherent in realism. For the realist, as an article of faith, sees scientific success, over the long run, as bringing us closer to the truth. His whole explanatory enterprise, using approximate truth, forces his hand in this way. But, a "NOAer" (pronounced as "knower") is not so committed. As a scientist, say, within the context of the tradition in which he works, the NOAer, of course, will believe in the existence of those entities to which his theories refer. But should the tradition change, say, in the manner of the conceptual revolutions that Kuhn dubs "paradigm shifts," then nothing in NOA dictates that the change be assimilated as being progressive, that is, as a change where we learn more accurately about *the same things*. NOA is perfectly consistent with the Kuhnian alternative, which counts such changes as wholesale changes of reference. Unlike the realist, adherents to NOA are free to examine the facts in cases of paradigm shift, and to see whether or not a convincing case for stability of reference across paradigms can be made without superimposing on these facts a realist-progressivist superstructure. I have argued elsewhere (Fine 1975) that if one makes oneself free, as NOA enables one to do, then the facts of the matter will not usually settle the case; and that this is a good reason for thinking that cases of so-called incommensurability are, in fact, genuine cases where the question of stability of reference is indeterminate. NOA, I think, is the right philosophical position for such conclusions. It sanctions reference and existence claims, but it does not force the history of science into prefit molds.