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Some Contributions of Philosophy to Behavioral Sciences

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Philosophical analyses can aid scientists in several ways. For example, (a) they can help resolve disagreements among scientists about issues such as the relative value of facts versus theories and observations versus inferences; (b) they provide historical descriptions of how science went when it went well or badly and scientists can imitate these descriptions as though they were prescriptive rules; (c) they identify "families" of theories and methodologies on the basis of common uses of key words, which can help scientists understand theories and methodologies other than their own; and (d) they can provide essential backgrounds for scientists' debates about issues such as final causality, chance causality, and context effects. However, philosophical analyses cannot provide support for empirical findings or theoretical concepts.

This article is about some of the ways in which philosophy can benefit science and some of the ways in which it is irrelevant to science. Although I often refer to "science" in this article without specifying which science, the reference is usually to behaviorism and other behavioral sciences and not the physical, biological, and social sciences. The analysis is based on a world-view approach, which is described in the first section of the article. A preliminary point is that the world-view approach has nothing to offer any unified science, which can be unified only if it is consistent with a single world view, but it has much to offer the behavioral sciences because they are not unified (e.g., Overton, 1998).

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The second section of the article is on debates among philosophers about whether science has an irrational component. The third section is on debates among behavioral scientists that could be usefully advanced by philosophical analyses; I give three examples — debates about final causality, chance causality, and context effects — in which relevant philosophical issues have often been ignored by one side or both sides. The fourth section is a brief comment about misuse of philosophical analysis as support for empirical or theoretical matters in science, and the final section is a brief summary of the article.

World Views and Science

The World-View Approach

Some philosophers of science have seen little or no value in the world-view approach (e.g., Suppe, 1977); but my impression is that most of these philosophers were either mechanists, reflecting the dominant Western world view, or Marxists, reflecting a world view that seems to be still dominant in the Russian confederation, is dominant in modified form in the People's Republic of China, and is the basis for several versions of "action theory" in European psychology (for discussion of European action theory, see Chapman, 1984; Eckensberger and Meacham, 1984). These philosophers generally divide people into two groups — right-thinkers who are mechanists or Marxists or whatever, and the rest of the population, who are either confused or misguided. According to the world-view approach, in contrast, all world views that are evaluated and found to be about equally adequate in scope and precision are equally right-minded.

Pepper (1942) identified four world views as about equally adequate — contextualism, formism, mechanism, and organicism. I use his definitions of these world views in this article. The following characterizations are rough; for details, see Pepper's Chapters 8 through 11. Contextualism is the same as American pragmatism (e.g., James, Dewey) and is closely related to dialectical materialism (Reese, 1993b); it is exemplified by Tolman's purposive behaviorism (Hahn, 1942; Zuriff, 1985, pp. 258–259) and by Skinner's radical behaviorism (e.g., Barnes and Roche, 1994; Hayes, Hayes, and Reese, 1988; E. K. Morris, 1988, 1993b). Formism is exemplified by the Platonic cave (Plato, *The Republic*, Book 7 [514–517]; 1952 version, pp. 388–389), Goethe's (1790/1946) theory that all parts of a plant are metamorphoses of an ur-leaf, and Chomsky's (e.g., 1980, Chapter 4) concepts of deep and surface structures of language. Mechanism is seen in the Newtonian machine and stimulus–response learning theory; and organicism is seen in Hegelian objective idealism and Piaget's theory of cognitive development. One of the basic principles of the world-view approach is that eclectic mixing of world

views cannot advance understanding, it can only lead to confusion (Pepper, 1942, p. 104; Leont'ev, 1974, made the same point). The argument is discussed immediately below.

Eclecticism

Pepper's theory. Every relatively adequate world view has a set of basic concepts referring to ontological and epistemological issues such as the nature of substance, stability, change, causality, and truth. Therefore, eclecticism must be confusing because it involves mixing different ontologies and different epistemologies. For example, a mechanistic ontology cannot be usefully mixed with a contextualistic ontology or an organic ontology, because in mechanistic ontologies the parts are real and the whole is an epiphenomenon and in contextualistic and organic ontologies the whole is real and its parts are abstract analytical concepts. An eclectic view would have a whole that is simultaneously both derived and basic and parts that are simultaneously both concrete and abstract.

Similarly, a contextualistic epistemology cannot be usefully mixed with a mechanistic epistemology or an organic epistemology, because in contextualism a theory is true if it is successful for some specified purpose, but in mechanism a theory is true if the statements in it are accurate descriptions of reality — the correspondence theory of truth — and in organicism a theory is true if the statements in it form a coherent, internally consistent network — the coherence theory of truth. A concrete example is that some cognitive eclectics have said that successful predictions indicate that a theory is true, but only if the cognitive concepts are consistent with — correspond with — what is known about neurophysiology (for discussion and references, see Reese, 1989, p. 25).

A theory can refer to more than one ontology and can avoid the confusing kind of eclecticism by restricting the different ontologies to different domains and specifying when each ontology is to be used. For example, light has wave-like properties in certain phenomena, such as interference in the double-slit apparatus, and it has particle-like properties in certain other phenomena, such as the photoelectric effect (e.g., Ohanian, 1995, pp. 86–90, Chapter 5; Radin and Folk, 1982, pp. 693–698, 764–768). The received theory is that light always consists of waves or "wave packets" (Ohanian, 1995, p. 159; Radin and Folk, 1982, pp. 772–780), but for most purposes one could theorize that light really consists of waves in the one set of phenomena and really consists of particles in the other set of phenomena. Similarly, in psychology a number of theorists have mixed the reactive-organism and active-organism ontologies, but have avoided the confusing kind of eclecticism by specifying that the organism is reactive in routine, habitual tasks and

is active in problem-solving tasks. Examples include the American psychologists Robert Woodworth (1918) and Sheldon White (1965) and the Soviet psychologists Alexander Luria (1929/1978) and Dmitrii Uznadze (1966).

Critique of Pepper's theory. In a review of Pepper's (1942) theory, Burtt (1943) argued that one kind of eclecticism — taking the best parts of different world views and applying them simultaneously to a given domain — has led to highly fruitful advances. With one exception, however, he gave only very general examples, and he said that the most influential cases were "reconcilers of reconcilers . . . [such as] Aristotle, St. Thomas, and Kant" (pp. 600–601), that is, philosophers who developed new systems rather than eclectic systems. The exception was an assertion that the concept of law in science "preserves something vital in the formistic conception of 'form' as well as something vital in the mechanistic conception of 'regular interrelationship' among the parts of a machine" (p. 600). I suppose he was referring to the hoped-for universal applicability of scientific laws, which would require that the concepts in the laws refer to abstract rather than concrete forms, that is, universal forms abstracted from specific concrete embodiments of forms. However, when interpreted formistically, the abstract concepts in these laws refer to both ontological and epistemological forms; and when interpreted mechanistically, the abstract concepts are in the epistemological domain and their ontological counterparts are the specific concrete embodiments.

In a reply to Burtt's review, Pepper (1943) explained why eclecticism is wrong, beginning with a summary of the world-view theory. The world-view theory is based on the principle that dogmatism is wrong, which implies that evidence needs corroboration. Corroboration can be "multiplicative" or "structural." Multiplicative corroboration is agreement among qualified observers; in other words, it is reliable observation. Structural corroboration is agreement among facts; that is, a fact in question is made believable by being shown to fit into a network of other facts. Structural corroboration can be conclusive only if the purported network indeed fits the definition of a *network* (i.e., a coherent structure) and even then only if the network includes all the relevant facts. These two requirements lead to a need for world views, which are the only potential sources of conclusive structural corroboration.

Every world view is grounded on a root metaphor; however, the evaluation of a world view is based not on its root metaphor but on the adequacy of the structural corroboration the world view provides. A structural corroboration is adequate if it has reasonable scope and precision, which Pepper identified as sufficient criteria of adequacy (1942, pp. 74–77) and said "are the cognitive aims of structural corroboration" (1943, p. 603). That is, a world view is adequate if it successfully fulfills its function of providing an adequate structural corroboration. Burtt said that eclecticism is often successful in this sense; therefore, he questioned Pepper's rejection of eclecticism. The answer

is that structural corroboration is based on a structure, structural corroboration is confusing unless the structure is coherent, and a root metaphor constitutes a coherent structure. A mixture of root metaphors cannot provide a coherent structure because any mixture must consist of relatively independent networks of facts — otherwise, only one world view is involved rather than a mixture of world views. In short, a mixture of two or more world views either constitutes an incoherent structure that cannot yield a conclusive structural corroboration or the mixture is consistent with a new root metaphor and therefore constitutes a new world view. In the latter case, the mixture may or may not yield a conclusive structural corroboration, depending on the scope and precision of the new world view.

Prevalence of World Views

Some scientists and other scholars have deliberately adopted a specific world view. For example, many psychologists in the old Soviet Union, including Vygotsky and Leont'ev but not Pavlov, deliberately adopted Marxism as their world view (Graham, 1987, pp. 161, 169, 211). Another example is that several North American behaviorists, including Hull (1943b), Spence (1960; Bergmann and Spence, 1941), and Spiker and McCandless (1954), deliberately adopted logical positivism, which is most clearly consistent with the mechanistic world view.[1] However, other scholars — maybe the majority —

[1]Smith (1986) argued that behaviorists did not deliberately adopt logical positivism, based on his analysis of the work of Hull, Tolman, and Skinner, and he noted that John B. Watson founded behaviorism some 15 years before the founding of logical positivism (p. 5). However, the argument can be challenged on two grounds. (a) Before the cognitive revolution, psychology was dominated by the stimulus–response learning-theory kind of behaviorism (e.g., White, 1970). Most of the stimulus–response learning theorists were mechanists and adopted logical positivism. Hull's work was in this tradition, and on Smith's own account (Chapter 7), Hull tried to use logical positivism but was only partially successful because he did not fully understand it. One of the major theorists in the Hullian tradition was Kenneth Spence and as Coleman (1987) pointed out, Smith did not follow up the implications of a long-lasting collaboration between Spence and the logical positivist Gustav Bergmann. Others in the Hullian or stimulus–response learning theory tradition, such as the child psychologists Boyd McCandless and Charles Spiker, also used aspects of logical positivism (Spiker, 1986; Spiker and McCandless, 1954). (b) Tolman and Skinner were not representatives of this tradition. Tolman's "purposive behaviorism" was a stimulus–stimulus rather than stimulus–response learning theory (e.g., Leahey, 1992, pp. 325–329; Spence, 1951) and it was consistent with contextualism rather than mechanism (Hahn, 1942; Pepper, 1934; Zuriff, 1985, pp. 258–259). Also, and perhaps more directly relevant, Hull had many disciples and Tolman had none (Leahey, p. 335). After the cognitive revolution, behaviorism was dominated by Skinner's "radical behaviorism," but by then behaviorism was no longer the mainstream of psychology. Skinner (1945, 1974, p. 16) explicitly disavowed logical positivism, and his approach was consistent with contextualism even though it had mechanistic overtones (the "overtones" point is controversal; mechanistic: Marr, 1993a, 1993b; Reese, 1986, 1993a, 1996; Staddon, 1993; mechanistic but should not be: Barnes and Roche, 1994; not mechanistic: Carr, 1993; Hineline, 1980, 1992; Lee, 1993; E. K. Morris, 1993a, 1993c, 1997).

have only implicitly adopted a specific world view, in accepting a set of principles that turn out on analysis to be consistent with a specific world view (Reese and Overton, 1972).

Some of these scholars seem not to realize that they have a world view and some deny that anyone has a world view or even deny that anyone *should* have a world view. For example, Pap (1953) argued that science is not based on "metaphysical" presuppositions, and Lindworsky (1932, pp. 6–10) argued specifically that psychologists should avoid philosophical presuppositions. Similarly, after reading the manuscript of Skinner's (1953) *Science and Human Behavior*, Percy Bridgman commented, "I would not like to say, as seems implied, that science has to assume that the universe is lawful and determined, but rather that science proceeds by exploiting those lawfulnesses that it can discover. Anything smacking of faith I think we can do without" (as quoted by Skinner, 1983, p. 60; italics deleted). For similar reasons, Suppe (e.g., 1977) argued that world views do not have any role in science.

In contrast, William James (1907/1981, p. 7) said that everyone has a philosophy, and some philosophers of science and historians of science have pointed out that scientists are dominated by a world view whether they want to be or not and whether they know it or not (e.g., Graham, 1987, p. 26). This position is consistent with Kuhn's (1970a, 1970b, 1977) analysis of the history of science and with analyses by Overton and me showing that some schools of psychology are comprehensible only from the world-view perspective (Overton, 1991a, 1991b, 1998; Overton and Reese, 1973; Reese, 1986; Reese and Overton, 1970; see also White, 1977).

Ontological Functions of World Views

World views have two general functions, one ontological and the other epistemological. As an ontology, a world view is a metaphorical representation of the empirical domain to which it is applied; and as an epistemology, a world view is a set of rules about concept formation, rules about induction of general principles or laws that interrelate concepts, rules of explanation, rules for determining the truth value of explanations, and so forth.

The ontological question is "What is the real world like?" and I would stress "like" because it indicates the metaphorical nature of any ontology. If an ontology is interpreted as a description rather than as a metaphor, it has no demonstrable value unless it is verified because if it is not verified, it can be held only dogmatically. The problem is that it cannot be verified because, as Hume, Kant, and others argued, our empirical knowledge is about sensations, not about things-in-themselves, and therefore we can test our empirical knowledge against our sensations but not against reality (Hume, 1739/1967, Book 1, Part 2, Section 6, pp. 66–68; Kant, 1787/1899, I, Part 2,

Division 1, Book 2, Chapter 3, pp. 156–157; Pearson, 1911/1969, pp. 63, 67). The subjective idealists, including George Berkeley (1734/1957, e.g., Sections 17–20), John Stuart Mill (1872/1979, pp. 178–187), and Karl Pearson (1911/1969, p. 68) argued further that statements about reality are therefore not justified.

In contrast, Lenin (1927, pp. 124–129) argued that we are justified in assuming that sensations have a basis in reality and in making assumptions about the nature of that reality. Accordingly, ontological statements are justified even though they are empirically unverifiable. I would add that because they are empirically unverifiable, they have no knowable truth value *as descriptions;* but if they are interpreted as metaphors rather than as descriptions, they have a kind of value that can be tested. The test is the extent to which adopting a specific ontology furthers the understanding of a specific empirical domain. In short, a description is useless unless it is true, but a metaphor may be useful even if it is descriptively false. An example is the aphorism "In the land of the blind the one-eyed man is king" (Macchiavelli, *The Mandrake,* Act 3; quoted from Harbottle and Dalbiac, 1958, p. 325). The metaphor is a good representation of the relativity of misfortune, even though H.G. Wells (1952) cast the one-eyed man as a disadvantaged misfit in the land of the blind.

Looking at an ontology from the world-view perspective helps avoid some misunderstandings. For example, in the mechanistic ontology, the world is represented as analogous to a machine. The possible operations of a machine are completely determined by its constituent parts and their interrelations; therefore, we understand the possible operations of the machine as a whole by analyzing the parts and their interrelations. This principle is sometimes called reductionism, and it is occasionally misinterpreted to mean reduction of biological or psychological phenomena to physical or chemical elements and laws of their operations (e.g., by Pronko, 1969, p. 488; Teyler, 1975, pp. 5–6). The mechanistic world view does not preclude the assumption that psychological operations, for example, can be understood only by reducing them to physical or chemical operations. However, this assumption reflects a kind of physicalism and it is not a necessary assumption in mechanism. The principle is reduction to parts and their interrelations (Teyler, ibid.), and the parts can be at the same level of observation as the whole.

A specific example of the ontological role of world views is that behavioral scientists have disagreed about the relative merits of facts versus theories and of observations versus inferences, among other issues. Mechanists tend to prefer facts because the ontology they usually adopt is a materialist realism and the epistemology is a copy-theory of knowledge. Therefore, observations are copies of ontologically real, material objects and events, and inferences and theories are mental constructions referring to observations but are not

themselves copies of anything that is ontologically real. Therefore, in mechanistic materialism, observations are valued over inferences and theories. In contrast, organicists adopt an idealist ontology in which ideal, disembodied forms are real and the material world is mere appearance — the material world is a distorted image of reality. Therefore, reality must be inferred by reasoning based on observations of mere appearances. Any inference is theoretical in a way that observation is not. Specifically, inference is *based on* premises and the premises constitute at least a low-level theory, but observation is only *influenced by* theory (or expectation). Therefore, organicists value theory over inferences and inferences over observations.

Epistemological Functions of World Views

Descriptive versus prescriptive rules. The rules formulated by philosophers and historians of science are descriptive, but they are often interpreted as prescriptive. The logical positivists may have been especially prone to this error, but it is also evident in Lakatos's (1978) view of psychology as prescientific because it does not fit his conception of physics and chemistry. Also, Kuhn (1970b) commented that his own rational reconstruction of the history of science is descriptive but that it is therefore also prescriptive: "If I have a theory of how and why science works, it must necessarily have implications for the way in which scientists should behave if their enterprise is to flourish" (p. 237). Nevertheless, prescribing rules for success is not one of the roles of philosophy in science (Spiker, 1986).

Philosophers and historians of science formulate epistemological rules to describe how science went when it went well and sometimes how it went when it went badly. However, as Butts (1983) noted in discussing Victorian science, the rules are merely descriptions and as such they do not necessarily have any prescriptive value. Thus, knowing how physics or chemistry went when it went well or badly in the past does not necessarily inform current physicists, chemists, psychologists, and so on, about how to do their science now or in the future. For example, science went better in the past when it was not tied to religious beliefs, but one reason was that many scientists with dissident religious beliefs were executed, or at least their works were suppressed (e.g., Whewell, 1847/1967, pp. 200–201). Furthermore, although Christian Science is bad science, it is not bad science because it violates the rule against tying science to religious beliefs; it is bad science because it lacks adequate precision, according to Pepper's (1942, Chapter 4) criteria.

A methodological role. Despite the foregoing comments, world views have a methodological role that involves prescriptive rules. In this role, a world view prescribes rules of research, such as whether the approach used should be analytic or synthetic and whether primacy should be given to facts

obtained through observation or facts obtained through inference or rational argument. However, the rules of research that are *prescribed* by a world view are very general. They are based on concepts of substance, causality, truth, and so on, that are categorical in a world view, and therefore they are not subject to empirical rules of success. In other words, they are *prescriptions* for any scientist whose research is consistent with a particular world view, because they are part of the "hard core" of the scientist's research program (Lakatos, 1978, p. 48).

In contrast, the rules that Butts (1983) discussed are descriptions of what scientists did, not descriptions of world-view entailments. John Herschel, for example, used an analytic approach in his optical research, and he was successful; but as Butts pointed out, Herschel's success with the analytic approach does not warrant *prescription* of the analytic approach for all research. In short, the mechanistic world view *prescribes* use of an analytic approach, and therefore all thoroughgoing mechanists must use this approach, but the prescription comes from adoption of the world view, not from empirical success in using the analytic approach.

"Families" of scientists. Some philosophers of science analyze how scientists in a discipline use key words such as *cause, effect, chance, purpose, verification* versus *falsification* of a finding, an expectation, or a theory, *explanation* or *understanding,* and *well-designed* versus *flawed* research. Analyses of the verbal practices of behavioral scientists have shown that they tend to cluster in groups, or "families," defined by how they use the key words. The members of a family use the key words in the same or similar ways, and the members of other families use some of the key words in other ways. The shared meanings within a family define the basic concepts of the family, and so far these basic concepts have always been found to be reasonably consistent with some specific world view and inconsistent with other world views.

Philosophical analyses have also identified differences within each family, but not in the basic meanings of the key concepts (e.g., Reese and Overton, 1970; White, 1977). The within-family differences identify different theories, but the commonalities identify the different theories as members of a single family. The results of this kind of philosophical analysis are very useful, because effective communication depends on shared meanings (e.g., Feyerabend, 1978, Chapter 17; Kuhn, 1970b, 1977, pp. xxii–xxiii). Communication between theorists and researchers within a family is relatively easy because they use the key concepts in the same or similar ways, but communication between theorists and researchers from different families is very difficult because they use some of the key concepts in different ways. Knowing the source of the difficulties can aid efforts to alleviate them.

An example of the difficulty of between-family communication can be seen in debates about the nature of scientific understanding. The debates

arise because of failure to realize that scientific understanding is conceptualized differently in different world views. The different understandings are illustrated in the following paragraphs.

(a) Contextualists in the tradition of B.F. Skinner understand a phenomenon when they can control its occurrences, because control is in itself successful working. Successful prediction is important only when it demonstrates successful control.

(b) Formists in the tradition of Noam Chomsky understand a phenomenon when they identify the idealized type that the phenomenon exemplifies. Control is not an issue, and successful prediction is important only to the extent that it demonstrates the "psychological reality" of the idealized type. An example is that "verb phrase" and "noun phrase" are idealized types in the deep structure of sentences, and if these types have psychological reality, their surface-structure manifestations should be separated by boundaries in the surface structure of sentences. Research has confirmed this expectation: when clicks are superimposed on sentences heard through earphones, the research participants tend to report hearing the clicks during phrase boundaries even when the clicks were actually presented within a phrase (e.g., Bever, Lackner, and Kirk, 1969).

(c) Mechanists in the tradition of John B. Watson understand a phenomenon when they can predict it, because successful prediction confirms that the theoretical description corresponds to reality.

(d) Organicists in the tradition of Jean Piaget understand a phenomenon when they persuade others that statements about the phenomenon are consistent with statements about other aspects of the domain that includes the phenomenon. Successful prediction or control is not necessary, and when it occurs it merely supports the argument that the network is coherent by adding another statement that fits into the network.

Irrationality in Science

As shown in the preceding section, the behavioral sciences are not unified but are nevertheless rational because they fall into subsets that are consistent with different world views. Despite this rationality, however, the behavioral sciences — and all other sciences — have irrational aspects. These aspects constitute the second topic in this article; as shown in the present section the issue is about the demarcation between science and pseudoscience, or what makes scientific knowledge *special*. According to Lakatos (1978), science is special because it is completely rational, but according to others, such as Popper (1965) and Kuhn (1970a), it is special in other ways. I will give examples showing that it is not completely rational, but I will not discuss the issue of how it *is* special other than to say that a fact is scientific if it meets

two criteria. A scientific fact meets a methodological criterion of careful observation under carefully described conditions, which is the scientific method, and it meets a theoretical criterion of consistency with a scientific theory. That is, scientific facts are obtained more carefully than ordinary facts and they are explained more carefully than ordinary facts. Finally, although science is not completely rational, neither is it completely irrational. In fact *irrational* may be too strong a word in this context, but it seems to be the best word we have. I use it to mean *not derived exclusively from valid reasoning based on corroborated evidence*. Defined in this way, it includes dogmatism.

Dogmatism in World-View Selection

Pepper defined dogmatism as "a demand for belief in excess of the evidence for it, or without allowing a critical examination of the evidence for it" (1966, p. 3). That is, a dogmatist is "one whose belief exceeds his cognitive grounds for belief" (Pepper, 1942, p. 11). The deliberate adoption of a specific world view is partly dogmatic, because belief in any specific world view must exceed the cognitive grounds for this belief. In any world view the grounds for belief are based on evidence about correspondence, coherence, usefulness, or whatever the relevant truth criterion is, but a world view can be definitively supported by evidence only when all the relevant evidence is known, and as almost everyone acknowledges, not all the relevant evidence is known yet.

Therefore, we should reject any assertion that choosing any one specific world view is demanded by the evidence. For example, Manicas and Secord (1983) described a philosophy variously called the realist theory of science, fallibilist realism, and transcendental realism. In a commentary, Mulaik (1984) argued that realism "is acceptable only on pragmatic grounds as a tentative working hypothesis" (p. 919); but Manicas and Secord (1984) replied that this philosophy has no viable alternative:

> It is not, that is, as if one can simply decide to be a realist on the grounds that there are practical reasons for doing so or that in the last analysis our belief in a "real, outer world" is a matter of faith. Neither is the case . . . . The objective world at the object end of sensation is itself a scientific conclusion. (p. 923)

In other words, Manicas and Secord's argument is that realism is adopted not really on pragmatic grounds, but on scientific grounds. This argument is not compelling because the phrase "a scientific conclusion" should actually be "an assumption in some theories." If the assumption is useful, it is used; but the decision is pragmatic, as Mulaik argued.

Another example is the following statement by a British mechanist: "The advocate of physical explanations does not choose a mechanistic explanation gratuitously, or from prejudice, but is driven to it in his search for an adequate explanation or one which covers the most facts by the fewest postulates and leaves the fewest anomalies outstanding" (Craik, 1943, p. 48). Contextualists, formists, and organicists also have these goals, and they believe that their world view provides a better approach to these goals than does mechanism.

Even though acceptance of a particular world view is partly irrational because it is partly dogmatic, the dogmatism should not become a dictatorial claim that this world view is *the* correct one. Conversely, the dogmatic aspect should not lead to an utterly skeptical claim that only disbelief in a world view is justified. What is needed is a moderate agnosticism, which acknowledges that belief in any particular world view must be based on incomplete evidence, that is, that the grounds for belief consist of partial knowledge because complete knowledge is unavailable. Alternatively, one can adopt a particular world view not as a *belief* but as a deliberate fiction, just to see where it leads. This approach is consistent with a Wittgensteinian game plan (Chapman, 1987) and especially with Vaihinger's (1925) philosophy of "as if." In Vaihinger's philosophy, a world view as a belief is an hypothesis and as such it needs *verification*, but an "as if" world view is acknowledged to be a fiction and as such it needs *justification* (pp. 74–76, 85–90). The justification is that it is a *useful* fiction.

Vaihinger's as-if approach seems clearly more reasonable that the belief approach, but the belief approach seems to be the norm. Furthermore, most of those who give the matter any thought believe that their world view is the only correct one; for example, Lenin (1927, pp. 127–128) said that both idealists and materialists believe that their position is demanded by scientific evidence. Lenin may have been aware that the selection of any particular world view is optional rather than obliged by proofs (Graham, 1987, pp. 43–46). If so, his awareness was not passed on to later Soviet philosophers (such as Fedoseyev, 1977, and Oizerman, 1977), who believed that dialectical materialism has scientific proof.

Other Irrationalities

I have given other examples of irrationalities elsewhere (Reese, 1991). Additional ones, which might better be called "lapses in rationality," are discussed in the present section.

Although the selection or adoption of a world view can be a conscious, rational process, it is not necessarily so (Reese and Overton, 1972). In fact,

the norm seems more likely to be that scientists do not do a comparative study of world views and then rationally select the best one for their purposes. Instead, the norm seems to be an unstudied, therefore incompletely rational, adoption of the world view prevalent in the society in which the scientists were raised (Reese and Overton, 1972). I grant that this conclusion does not explain how dissidents come to reflect a different world view, but these apparent exceptions call for an empirical explanation — they pose a problem more for the psychology of science than for the philosophy or history of science.

Some philosophers and historians of science have rejected the possibility of this kind of irrationality in science, often with highly emotional attacks on the "irrationalists." They want science to be special in order to demarcate it from such "unspecial" domains as art, drama, and religion. They believe that if science is in any way irrational, it is not special; and instead of rejecting the idea that it is special, they reject the conclusion that it is irrational in any way.

Actually, the position of the "irrationalists," such as Thomas Kuhn, Paul Feyerabend, and Larry Laudan, was not as extreme as represented by the "demarcationists" such as Imre Lakatos, who accused Kuhn of attributing scientific change to "mob psychology" (Lakatos, 1978, footnote 2, p. 55). Almost all scientific activity is rational according to Kuhn, Feyerabend, and Laudan, even though it has a partially dogmatic basis in the "paradigm" (Kuhn, 1970a) or "party line" (Feyerabend, 1970, p. 169) adopted. Furthermore, even Lakatos admitted that the history of science reveals occurrences of irrationalities; for example, he (1978, pp. 53–55) cited a 19th century research program by William Prout that was abandoned by some chemists in that era because it became tiresome to them. (Prout's program dealt with the ideas that all pure chemical elements have whole-number atomic weights and that empirically obtained discrepancies reflect use of impure chemicals [Lakatos, 1978, p. 53; compare Prout, 1855, pp. 108–112]. Prout used chemical and other regularities to argue for design in nature, and he argued from design to deity [e.g., 1855, Book 1, Chapter 10]; but in the 19th century, the argument from design to deity was widely accepted among scientists and doubts about it seem unlikely to have contributed to abandonment of Prout's program.)

Examples of Philosophical Contributions to Science

Three issues are discussed below as examples of how misunderstandings by scientists can be resolved by philosophical analysis. The issues are about final causality, chance causality, and context effects.

Final Causality

Few of us would deny *purpose* in human behavior, and many scientists and philosophers see purpose even in the behavior of nonhuman animals. For example, Samuel Butler (1894/1968, pp. 83–84) attributed purposes to flies and cats. The attribution of a purpose to nonhuman animals is problematic because they do not demonstrably have minds (Alverdes, 1932, pp. 19–21). However, the issue discussed here is not this one but the issue of how effects of purposes can be explained by scientists who reject the concept of final causality. The latter issue arises because purpose is a teleological concept and therefore seems to impute time reversal to causality (Reese, 1994, 1997).

Antecedent "final" causality. Rignano (1923) said that what looks like final causality is actually antecedent causality based on habit (pp. 28–29), which is essentially similar to Skinner's position (e.g., Reese, 1994, 1997) except that Rignano believed that habit can be transmitted genetically (p. 17, citing Lamarck). The following statement by Bunge (1959) is consistent with Skinner's position:

> [Behaviors] are actually determined by the immediately previous states and by the whole past history of the organism, as well as by its environment; organs, functions, and behaviors could not be determined by future, still nonexistent needs; they are presumably determined by past and present conditions and are *adapted* beforehand to coming conditions, though not with foresight or conscious planning, but as a result of a long and blind past history of successes and failures. (p. 302)

In this view, teleological laws are regulative rather than constitutive; that is, as explained in the next paragraph, they are "as if" laws.

Constitutive versus regulative final causality. A *constitutive* principle refers to real, objectively true elements (Janet, 1884, pp. 7, 317–318; G.S. Morris, 1875, p. 11; Vaihinger, 1925, p. 273). A *regulative* principle is a principle of inquiry (Prosch, 1964, p. 281; Vaihinger, ibid.); it is an a priori rule of reasoning, reflecting, thinking, or the like, and it is *applied* to experience rather than *derived* from experience (Kant, 1790/1982, Sections 61 and 66; see also Vaihinger's, 1925, pp. 273–275, and Cassirer's, 1938/1970, pp. 333–335, discussions of Kant's position). It is subjectively necessary; but no reality or objective truth is known or even necessarily assumed to correspond to it. (*Constitutive* and *regulative* also have other meanings, which are not discussed herein.)

For example, Alverdes (1932) attributed consciousness and purpose to nonhuman animals, but he said that he did so because the attribution is reasonable and useful even though it is a "fiction." He considered the attribution to be fictional because the question of consciousness and purpose in nonhuman animals cannot be answered on the basis of scientific evidence

(pp. 19–21). He said, "A fiction [of this kind] is a construction which brings into connection with one another various kinds of processes or things in a manner which enables us to think about them" (p. 137). In this view, which is also the modern scientific view, teleological laws are regulative, or epistemological, because they are reducible to constitutive laws in which the ontological counterpart of purpose is an *efficient* cause. Thus, this position defines away teleology as an ontological principle.

Teleology must be denied in both the ontology and the epistemology of materialistic realism (e.g., dialectical materialism): explanations must refer to real matter or real matter in motion, and a future event does not yet have material reality and therefore cannot cause a past or present event. Teleology is accepted in idealistic realism (e.g., Plato; Hegel) and in rationalism (e.g., Piaget), but in different senses. Idealistic realism requires constitutive teleology to explain phenomena because the ideal forms, or structured networks of ideal forms, are conceptualized as real and therefore as real *teloi*. In contrast, rationalism requires regulative teleology because the postulated forms or structured networks of forms have no ontological existence — they are epistemological concepts.

Bunge's (1959) position as expressed in the statement quoted above is consistent with contextualism and mechanism in rejecting teleology as an ontological principle. It is inconsistent with versions of organicism in which the teleological principle is interpreted as constitutive, that is, as ontological; but it is consistent with modern versions of organicism, in which the teleological principle is regulative, that is, epistemological rather than ontological (Overton and Reese, 1973; Reese, 1994). In modern organic psychology, for example, the endpoint that explains the directionality of development is not conceptualized as an efficient cause of development; rather, it is an "as if" representation introduced solely to put the data into epistemological order (Murray, 1991; Overton, 1991a, 1991b).

Chance Causality

The issue. Scientists acknowledge the occurrence of chance events in two related senses: they acknowledge error of measurement, or error variance (Raymond C. Russ, personal communication, June 5, 1998), and they acknowledge that specific individual events are unpredictable. Error variance is so pervasive that it probably does not need to be exemplified, but one perhaps nonobvious example is that operant behavior is variable even in its relatively stable state (Skinner, 1989, p. 124). Unpredictability is also pervasive and also probably does not need to be exemplified, but common examples are: (a) in physics the movements of an individual molecule of a gas are unpredictable even though the movements of the gas as a whole are pre-

dictable; (b) in psychology the times between individual keypecks by a pigeon in a Skinner box are unpredictable even though the overall rate or pattern of a series of keypecks is predictable; and (c) in political science and sociology the way an individual person will vote is unpredictable even though group voting patterns are predictable.

The issue, however, is not whether unpredictable events occur, but how unpredictable events can be explained by scientists and philosophers who insist on determinism and reject true chance as a determinant. Many scientists fail to see the issue. On the one hand they point out that not every event can be predicted because of chance, but on the other hand they point out that every event is caused and that true chance is not a cause. They do not realize that unless they explain unpredictability without reference to chance causality, their concept of causality is an empty verbalism. This issue is illustrated in the following paragraphs by examining mechanistic analyses of chance. As will be seen, the analyses are actually various ways of denying true chance, because even accidents are determined in the mechanistic world view and the apparent unpredictability of individual events is in principle denied.

Chance and causality. A very clear example of the mechanistic analysis of the relation between chance and causality is the following statement by Jonathan Edwards (1754/1957):

> For though the die's falling in such a manner be accidental to him that casts it, yet none will suppose that there is no cause why it falls as it does . . . . The involuntary changes in the succession of our ideas, though the cause may not be observed, have as much a cause, as the changeable motions of the motes that float in the air, or the continual, infinitely various, successive changes in the unevennesses on the surface of the water. (p. 200)

The "young Hegelian" Ludwig Feuerbach (1841/1957, p. 188) criticized this view as only *appearing* to deny chance causality, in that it merely substitutes the mysterious workings of a deity for the mysterious workings of chance. However, for a mechanistic theologian like Edwards, chance *must* be denied even at the cost of invoking a mysterious deity.

A statement by Skinner (1953, p. 20) provides a nontheological example: he said that the flight path of a fly is generally considered to be unpredictable but that it could be predicted if anyone was interested enough to do the required calculations. He commented that the calculations have never been done because the needed time and expense exceed the scientific value of doing them. Nonmechanists might challenge Skinner's statement, but not on empirical grounds because the attempt has apparently never been made.

Skinner's statement might be challenged on theoretical grounds, for example, by invoking Heisenberg's indeterminacy principle, which Niels Bohr and some other physicists interpreted as requiring acceptance of true chance (Wallace, 1974, pp. 307–308). However, even though the indeterminacy

principle is ontological, it does not undermine the concept of complete determinism (Bunge, 1959, pp. 14, 328) and thus it does not require acceptance of true chance. The indeterminacy principle means that simultaneous measurement of the position and the velocity of a particle is impossible, which means that its position cannot be determined if its velocity is determined, and vice versa. However, "determined" here does not mean "caused," it means "measured" or "assessed"; thus, the indeterminacy principle does not contradict the mechanistic concept of causality. It nevertheless has an *epistemological* consequence: the mechanistic concept of causality is not completely testable and therefore must be accepted on faith. Actually, concepts of causality are always accepted without proof because they are basic, categorical concepts in any world view — in Lakatos's (1978) terminology, they are part of the "hard core" of a research program.

Chance and predictability. Chance is equated with unpredictability in several ways in mechanism. Five ways described by Pauling and Zuckerkandl (1972) are discussed in the following paragraphs. The first four refer to various kinds of ignorance, which is the usual reference in mechanism, and the fifth refers to a cause that is problematic in mechanism.

(a) A phenomenon is unpredictable if some of the relevant laws are not known or some of the relevant variables are not known. Here, chance is not explained but rather is explained away by an appeal to lack of knowledge — epistemological ignorance. Hume also explained apparent chance as reflecting ignorance (1777/1902, first sentence of Section VI).

According to this meaning, chance causality is a practical matter rather than a matter of principle—a position endorsed by, for example, the logical positivist Gustav Bergmann (1957, p. 122) and the geneticist Theodosius Dobzhansky (1974, p. 313). This position is consistent with Jonathan Edwards's statement quoted above and also with a statement by Voltaire: "Chance exists nowhere. It was invented only to explain a known effect, the causes of which are unknown" (my translation from the French quoted by Rensch, 1974, p. 243). The same point has also been made by others (e.g., Henry Lord Brougham, according to editorial note *, pp. 51–52 in Paley, 1838; but Brougham, 1840/1864, pp. 117–118, gave "chance" and "luck" a role in discovery and invention).

(b) A phenomenon is unpredictable if some of the relevant variables are not controllable. This case is an appeal to a methodological shortcoming—another kind of ignorance.

(c) A phenomenon is unpredictable if "we ourselves leave it outside the field of the predictable" (Pauling and Zuckerkandl, 1972, p. 122), that is, outside the system we are interested in. This case is an appeal to narrowness of theoretical scope — a third kind of ignorance. An example is the flight path of a fly, as Skinner's comment indicates.

(d) A phenomenon is unpredictable if assessment of the relevant variables interferes with production of the phenomenon. This case refers to the indeterminacy principle, discussed above.

(e) A phenomenon is unpredictable if it is produced by "the exercise of a *will* [i.e., a mind] distinct from our own," which may be that of another human or "of another animal" (Pauling and Zuckerkandl, 1972, p. 122). This case involves a deus ex machina.

In all five cases, according to Pauling and Zuckerkandl, "all that is due to chance is at the same time *intrinsically determined*" (p. 123; emphasis added). Therefore, in mechanism chance causality is not a *basic* concept; it is a derived concept and consequently it is not strictly a category of causality.

Pauling and Zuckerkandl's including the effect of will, or mind, as "intrinsically determined" means that they rejected the concept of *free* will, that is, will that can operate in an intrinsically unpredictable manner. This position is consistent with Max Planck's (1947/1949) position: "If a historian wanted to ascribe the decision of Julius Caesar to cross the Rubicon not to his political deliberations and his innate temperament, but to free will, his view would be tantamount to a renunciation of scientific understanding" (p. 73). That is, as viewed by mechanistic scientists, the will must be assumed to be causally determined—even if the person who exercises it views it as free (p. 75).

Nagel's analysis of chance. Like Pauling and Zuckerkandl (1972), Nagel (1961, pp. 324–335) identified five meanings of chance. One meaning he identified is essentially the same as their first two meanings, but the other four are not similar to any of Pauling and Zuckerkandl's meanings. The four dissimilar ones are discussed in the following paragraphs.

(f) Chance can be understood in the commonsense meaning of luck. This commonsense meaning is irrelevant here because it is neither philosophical nor scientific.

(g) Another meaning of chance in Nagel's analysis is irrelevant because it is trivial: the occurrence of an event is attributable to chance "if in a given context of inquiry the statement asserting its occurrence is not derived from anything else" (Nagel, 1961, p. 329). Nagel's example was that predicting a future position and velocity of the planet Mars by means of Newtonian gravitational theory requires that an initial position and velocity be specified, and that the initial position and velocity are chance occurrences in this instance. As Nagel pointed out, this meaning of chance is context-specific.

This meaning of chance is important only if Nagel's phrase "is not derived" is changed to "is not derivable" and even then only if "is not derivable" is understood to imply "because of true emergence." With this implicit qualifier, this meaning of chance is contrary to mechanism; without it, "is not derivable" is a matter of ignorance and therefore is consistent with mechanism.

(h) Chance can refer to an event "at the intersection of two independent causal series" (Nagel, 1961, p. 326). Aristotle (*Physics*, Book 2, Chapter 5) conceptualized chance in this way. Bandura (1982) also used this meaning, but Nagel argued against it. However, Nagel's argument was sophistical: any specific event is determined not by *two* but by an *indefinite* number of distinct chains of causes (p. 327). Actually, although Nagel's assertion is probably often true, its truth is an empirical matter and not, as Nagel apparently believed, a matter of logic. Furthermore, Nagel overlooked the possibility that for practical purposes, and perhaps theoretical purposes as well, no more than two or a few independent causal chains need be identified to provide a fully adequate understanding of an apparently chance event. Most importantly, philosophers and scientists who talked about chance as the intersection of only two causal chains were, I feel sure, aware of the possibility that a multiplicity of intersecting determinants can influence an event. Perhaps they talked about only two determinants because they believed that all but two of the multiple determinants are irrelevant to the apparent *chance* nature of an event. Alternatively, they may have talked about only two determinants merely as a simplification for the sake of exposition.

A final point regarding this meaning of chance is that Nagel concluded that it "does *not* entail that the event is uncaused, or even that we are ignorant of the conditions which determine its occurrence" (p. 329). This conclusion agrees with Aristotle's (and Pepper's, 1942) position, but only if "are ignorant" is changed to "were ignorant and may remain ignorant." That is, according to this meaning of chance, prior to the event attributed to chance we were ignorant of the conditions that would determine its occurrence, and after its occurrence we may identify these conditions — or we may not identify them (Pepper, 1942, pp. 255–260).

(i) Finally, chance can refer to an uncaused event, that is, an event designated as a chance event because it has no determining conditions (Nagel, 1961, p. 331). Contextualists might argue that chance in this absolute sense does not exist because true chance can be conceptualized in contextualism as a determinant. An example is Peirce's (1892a, 1892b) concept of *tychism*. However, Pepper seems to have conceptualized true chance as an uncaused *outcome* rather than a cause of outcomes (Pepper, 1942, pp. 233–236, 255–260).

Nagel argued on epistemological grounds that debate about chance in the absolute sense is inconclusive: events are attributable to chance in the absolute sense if they are not predicted by any current theory, but absolute chance would cease to be relevant if a later theory predicted the events (pp. 333, 335). Thus, in mechanism even absolute chance turns out to be relative.

Context Effects

Capaldi and Proctor (1994) argued that contextualism is not unique because, among other things, noncontextualists also look at context effects. Capaldi and Proctor misunderstood the issue, which is not whether context effects occur but how to explain context effects. Everyone acknowledges that context effects occur — even in Piaget's mentalistic cognitivism the social context determines the content through which the universal stage of formal operational thought is manifested (Piaget, 1972; Piaget and Inhelder, 1969, pp. 152–159).

Mechanists in both physics and psychology look for universal laws. Universal laws are universally applicable, that is, context-free; therefore, mechanists must explain away each context effect they find so that it does not contradict universality. An example in psychology is Clark L. Hull, who took pains to explain the so-called "arpeggio paradox." The arpeggio paradox means that after a specific tone presented by itself has become a conditioned stimulus for a given response, the tone reliably elicits the response during test phases, but the same tone presented as part of an arpeggio or part of a melody does not elicit the response. In an arpeggio or a melody, the tones are presented sequentially and therefore each tone in an arpeggio or a melody is temporally isolated from the others, but the sequential context of the other tones nevertheless inhibits the conditioned-stimulus function of the conditioned tone. To explain this paradox, Hull (1943a, pp. 372–374) introduced a principle of "afferent neural interaction."

Philosophical "Evidence"

Some behavioral scientists attempt to support empirical findings or theoretical concepts by citing philosophical or "metatheoretical" analyses. However, these attempts are illegitimate. Metatheoretical analyses of the meanings of *fact, observation, inference, explanation, concept formation, theory development, methodology,* and so forth, can reveal whether these words and phrases are used consistently by various groups of scientists, or whether a particular theoretical explanation is consistent with a particular kind of logic. However, such analyses cannot yield factual information about behavior and cannot reveal whether any given theoretical explanation of behavior is correct or incorrect.

An example is that metatheoretical analyses have shown that much of what behavior analysts do is consistent with contextualism (e.g., Barnes and Roche, 1994; Hayes et al., 1988; E.K. Morris, 1988, 1993b), but these analyses do not demonstrate that behavior analysts *are* contextualists — they could be sloppy mechanists — and do not demonstrate that behavior analysts

should be contextualists — maybe they would be more successful if they were formists. Another kind of example is a remark by Skinner (1945) that if the experimental analysis of verbal behavior turns out not to be consistent with logic, the problem is not with the experimental analysis but with the logic. In the same vein, Hayes (1991) said, "Logic is . . . social behavior. As such, it is up to behavioral scientists to explain logic, not to appeal to it as an explanation of psychological events" (p. 35). Finally, as Mao Tse-Tung (1937/1965, p. 300) pointed out, "If you want to know the taste of a pear, you must change the pear by eating it yourself." You do not ask a philosopher because, as Hume said, "we cannot form to ourselves a just idea of the taste of a pine-apple, without having actually tasted it" (1739/1967, Book 1, Part 1, Section 1, p. 5).

Summary

Scientists have disagreed about the relative merits of facts versus theories and of observations versus inferences, among other issues. Pepper's (1942) philosophical analysis of evidence helps to understand the disagreements. For example, in mechanistic materialism, observations are valued over inferences and theories because observations are interpreted as copies of real objects and events, and inferences and theories are interpreted as mental constructions. In organicism, in contrast, theory is valued over inferences and inferences over observations because ideal forms are real and are "given" theoretically but instantiations must be inferred from observations of objects and events that are actually mere appearances.

Philosophers formulate rules describing how science went when it went well or badly, but the rules do not necessarily have any prescriptive value. That is, knowing how physics or chemistry went when it went well or badly in the past does not necessarily inform current scientists how to do their science now or in the future. Many philosophers interpret the rules they formulate as prescriptive, but prescribing rules for success is not one of the roles of philosophy in science.

Philosophers have analyzed how scientists use key words such as cause, chance, purpose, and explanation. Scientists tend to cluster in groups, or "families," defined by how they use the key words. The members of a family use the key words in the same or similar ways, and the members of other families use some of them in other ways. The shared meanings within a family are reasonably consistent with a given world view and inconsistent with other world views. Identifying the families is useful because the sharing of meanings makes communication within a family relatively easy and the nonsharing of meanings makes communication between families difficult. Knowing the source of the difficulties can help alleviate them.

Scientists have debated about issues such as final causality, chance causality, and context effects. Philosophical analyses that distinguish between ontological and epistemological aspects of these issues, and that identify world-view differences in these aspects, can be highly useful for such debates.

Some scientists cite philosophical analyses as support for empirical findings or theoretical concepts, but these uses of philosophy are illegitimate. Philosophical analyses of fact, inference, methodology, and so on, cannot yield factual information about behavior and cannot reveal whether any theoretical explanation of behavior is correct, although they can reveal that a particular theoretical explanation is or is not consistent with a particular kind of logic. A philosopher cannot inform you what a pear or a pineapple tastes like; if you want to know, you must eat it yourself.

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