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WHAT IS SCIENCE? DOES IT MATTER TO DISTINGUISH IT FROM PSEUDOSCIENCE? A REPLY TO MY COMMENTATORS*

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All of the comments on my paper boil down to the question 'What is science?' and, more particularly, 'What distinguishes science from pseudoscience?' Therefore, before endeavoring to meet the objections of my critics it will be convenient to explain what I mean by 'science.' In so doing I will borrow heavily from some of my previous work (Bunge, 1959a, 1967a, 1982a, 1983a, 1983b, 1985a, 1985b). And, of course, I will rely on my experience as a professional physicist and amateur theoretical psychologist and mathematical sociologist.

A DEFINITION OF "SCIENCE"

The epistemological question 'What is science?' was posed by Plato, and it has engaged such eminent philosophers as Aristotle, Descartes, Leibniz, and Kant. It splits naturally into two subproblems: 'What is formal science, in particular mathematics?', and 'What is factual (or empirical) science, in particular psychology?' Only the second subproblem will be dealt with here, because pseudomathematics is rather easily identifiable. (Psychologists will recall the phoney mathematical theories of the mind proposed by J.F. Herbart and K. Lewin: see Miller, 1964.)

Most philosophers believe that factual science is characterizable by a single trait, such as confirmability, refutability, practical success, or consensus. In my view this opinion is simplistic, and it excises many scientific ideas and procedures while it condones many unscientific ones. In my view factual science is an exceedingly complex object with at least ten different features. In fact, I define a *family of factual scientific research fields* as a variable collection every member $\mathscr{R}$ of which is representable by a 10-tuple,

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$$\mathscr{R} = <C,S,D,G,F,B,P,K,A,M>,$$

where, at any given moment in history,

(i) C, the *research community* of $\mathscr{R}$, is a social system composed of persons who have received a specialized training, hold strong communication links amongst them, and initiate or continue a tradition of inquiry (not just of belief);

(ii) S is the *society* (complete with its culture, economy, and polity) that hosts C and encourages or at least tolerates the activities of the components of C;

(iii) the *domain* D of $\mathscr{R}$ is composed exclusively of (certified or putatively) real entities (rather than, say, freely floating ideas) past, present or future;

(iv) the *general outlook* or *philosophical background* G of $\mathscr{R}$ consists of (a) an *ontology of lawfully changing things* (rather than, say, one of ghostly or unchanging or miraculous entities); (b) a *realist epistemology* (instead of, say, a subjectivistic one) and (c) the *ethos of the free search for truth, depth, and system* (rather than, say, the ethos of faith or that of the bound quest for utility, profit, power or consensus);

(v) the *formal background* F of $\mathscr{R}$ is a collection of *up to date* logical and mathematical theories (rather than being empty or formed by obsolete formal theories);

(vi) the *specific background* B of $\mathscr{R}$ is a collection of *up to date and reasonably well confirmed* (yet corrigible) data, hypotheses, and theories, and of reasonably effective research methods, obtained in other research fields relevant to $\mathscr{R}$;

(vii) the *problematics* P of $\mathscr{R}$ consists exclusively of *cognitive problems* concerning the nature (in particular the laws) of the members of D, as well as problems concerning other components of $\mathscr{R}$;

(viii) the *fund of knowledge* K of $\mathscr{R}$ is a collection of *up to date and testable* (though not final) theories, hypotheses, and data compatible with those in B, and obtained by members of C at previous times;

(ix) the *aims* A of the members of C include *discovering or using the laws*, trends and circumstances of the Ds, systematizing (into theories) general hypotheses about Ds, and refining methods in M;

(x) the *methodics* M of $\mathscr{R}$ consists exclusively of *scrutable* (checkable, analyzable, criticizable) and *justifiable* (explainable) procedures, in the first place the general scientific method (Background knowledge → Problem → Solution candidate → Check → Candidate evaluation → Eventual revision of either Solution candidate, Check, or Background knowledge).

(xi) There is at least one other *contiguous* scientific research field $\mathscr{R}'$ in the same family of factual scientific research fields, such that (a) $\mathscr{R}$ and $\mathscr{R}'$ share some items in their general outlooks, formal backgrounds, specific backgrounds, funds of knowledge, aims, and methodics; (b) either the domain of one of the two fields, $\mathscr{R}$ and $\mathscr{R}'$, is included in that of the other, or each member of the domain of one of the fields is a component of a system in the domain of the other.

(xii) The membership of every one of the last eight components of $\mathscr{R}$ *changes*, however slowly at times, *as a result of inquiry* in the same field as well as in related fields of scientific inquiry.

Any research field that fails to satisfy even approximately all of the above twelve conditions will be said to be *nonscientific* (examples: literary criticism and theology). A research field that satisfies them approximately may be called a *semiscience* or *protoscience* (examples: economics and political science). And if, in addition, the field is evolving towards the full compliance of them all, it may be called an *emerging* or *developing* science (examples: psychology and history). On the other hand, any field of knowledge that is nonscientific but is advertised as scientific will be said to be *pseudoscientific*, or a *fake* or *bogus* science (examples: parapsychology and psychoanalysis). The difference between science and protoscience is a matter of degree, that between protoscience and pseudoscience is one of kind.

I submit that the above definition captures the essential conceptual, empirical, social, and historical features of any basic factual science, from physics to history. Obviously, it does not concern pure mathematics, applied science, or technology. And, of course, it only goes as far as any definition can go—which is never far. In any event, it will be used in the following discussion.

I took the above definition for granted in the target article, where I criticized a fair number of widely held beliefs. This may have created the impression, in some readers, that I dismissed some of those views out of hand. I hope that the following replies to my commentators will dispel this impression at least in part. Still, here too my space is limited, so I am forced to refer to a good number of my own writings for further clarification and justification of my views.

ALCOCK ON REAL AND IDEAL SKEPTICS

One can only learn from Professor Alcock's studies on parapsychology. Being a social psychologist, he is interested in explaining scientifically not only phenomena claimed to be paranormal, but also the persistent belief in them despite the total lack of solid evidence. And, having learned some tricks of stage magicians, Alcock is capable of detecting fraud and self-deception, two faithful companions of psychical research.

I agree that my characterization of the methodological skeptic is very much like that of the good boy scout. Indeed, my characterization is normative or prescriptive, not descriptive. However, if we wish to improve our behavior, be it as scouts or as methodological skeptics, we must agree on some norms. The philosophy of science, along with grammar, logic, and ethics, is an essentially normative discipline. On the other hand, the history and sociology of science are descriptive and, in the best of cases, explanatory as well.

I also agree that "every one, to some small degree at least, is a methodological skeptic at some time or in some part of his or her life." In fact, without a modicum of critical ability one would soon be put out of commission. Thus, presumably most parapsychologists are alert to sharp car or card dealers, and they do not believe everything they read in the pulp magazines. But one cannot be critical of everything all the time: Life is too short. Hence in everyday life we take much for granted and on trust—until shown to be wrong.

But of course when it comes to scientific research nearly everything is supposed to be open to doubt and critical scrutiny—though, again, under the constraint of our limited resources, the scarcest of which is always time. What is so distressing about parapsychologists is that even the ablest and most honest of them, such as the late J.B. Rhine, are so eager to believe, that time and again they fall victims to deception and self-deception (see, for example, Kurtz, 1985, Part 2). They won't be swayed by unfavorable results, which they can always wish away as results of faulty observation or fading psychical ability.

It is precisely in cases of persistent self-deception that the philosophy of science can play a decisive role. It does so by showing that favorable evidence is always necessary but never sufficient, for unfavorable evidence can always be reinterpreted by ad hoc hypotheses. Now, an ad hoc hypothesis is admissible if it is bona fide, that is, an independently testable component of a comprehensive and successful theory. But an ad hoc hypothesis is inadmissible if it is mala fide, that is, if it is not independently testable and its only function is to protect some stray conjecture. Typically, the ad hoc hypotheses of parapsychology, for example the inhibitory effect of the presence of a skeptic, are mala fide. (For these two kinds of ad hoc hypothesis see Bunge, 1973b, 1983a.)

More on parapsychology anon.

BAUER AND v. LUCADOU ON SPOOKS

Bauer and v. Lucadou engage in an emotional defense of parapsychology. Their central claim is that ESP must be for real since there are so many publications that say so—among them half a dozen parapsychology journals. But this is of course begging the question. The question is whether there is at least *one* well certified case of paranormal ability that justifies the parapsychology industry. If there were one such fact, its finder should claim the $10,000 prize offered long ago by James Randi, and the even juicier one of $100,000 offered a couple of years ago by the Belgian Dr Jacques Thodor. (Inquiries: Sceptiques du Québec, C.P. 282, Repentigny, Québec, Canada J6A 7C6.)

Since there are no certified facts of the kind claimed by mediums and their fans, parapsychology is not a factual science, even after more than one century of active psychical research—some of which has been funded by the Pentagon. The genuine problems for the scientific psychologist with regard to ESP are (a) to construct normal explanations of the parapsychological delusions, (b) to uncover the flaws in the experimental design, statistical processing, or logical argument of the allegedly successful parapsychological experiments, and (c) to account for the individual variability in gullibility and its cultural conditioning. Some psychologists (e.g., Alcock, 1990; Hyman, 1985; Reed, 1988) have addressed these problems. But it is hard to recruit scientists to work on ideas that are admittedly incompatible with the bulk of scientific knowledge. After all, as Broad (1949) admitted long ago, if parapsychology is true, then science as we know it must be dropped—a stiff price to pay for delusion.

My critics state that I ignore "a host of challenging data and promising models," but they do not bother to mention any of them. True, they direct me to "the relevant literature," but this happens to consist almost exclusively of parapsychological journals and books. Mainstream psychological journals seldom if ever publish papers in parapsychology, just as serious physics journals do not publish paraphysical papers.

Consider the case of psychokinesis (PK), which my critics believe in. It is well known that the PK hypothesis violates all of the conservation laws. Bauer and v. Lucadou reassure us that "such effects are very weak and do not violate physics *in a strong way*" (emphasis added). What a relief! So, physicists ought only to make small corrections to their conservation laws, right? Exactly how small, and of what kind, pray? Since physical measurements are typically many times more precise than psychological measurements, how do my critics propose to compete with the former? And since all the conservation laws are entailed by variational principles, or by the field equations or the equations of motion derivable from them, how do Bauer and v. Lucadou propose to alter those principles to square with parapsychology? (Warning: Even small alterations of the fundamental equations of physics, say those of Maxwell or Schrödinger, could have measurable physical effects.) As long as parapsychologists have not produced and confirmed the whole new physics (paraphysics?) required by their far-out beliefs, scientists will exercise their right to stick to normal physics—which, by the way, keeps discovering and explaining astounding facts, instead of repeating a handful of pseudofacts.

Parapsychology will reappear in my replies to Blitz and Feyerabend.

Bauer and v. Lucadou take me to task for referring to "psi waves," which according to them no serious parapsychologist believes in. I do not know whether my critics take the yearbook *Advances in Parapsychological Research* to be a serious publication. (As far as I am concerned there is no such thing as serious pseudoscience.) In any event several contributors to that yearbook, such as K. Ramakrishna Rao (vol. 2, 1987) and Douglas M. Stokes (vol. 5, 1987), do report sympathetically on "field theories of psi." And vol. 3 (1982) contains a long paper by William G. Roll, one of the main experts on psi waves, on "The changing perspectives on life after death."

I do admit, though, that a consistent parapsychologist, such as the late Joseph B. Rhine, could not possibly accept the psi wave hypothesis, for smacking of materialism and science. I only mentioned that speculation because it is frequently proposed by *science* students as a possible physical explanation of the so-called parapsychological phenomena. However, one should never try to explain nonfacts.

My critics conclude by stating that "Parapsychology indeed has a future as a science!" A bold prophecy, since the field has not even got a present. But that, groundless faith, is the stuff pseudoscience is made of.

BLITZ ON QUASI-SCIENCE, HISTORICITY, AND DEMARCATION

I agree with most of Professor Blitz's comments. In particular, I agree that what may look nonscientific at one time may turn out later on to be scientific, and conversely. The case of phrenology, mentioned by Blitz, is indeed instructive, for the hypotheses that the mind is a collection of brain functions, and that these are localized, have been amply vindicated. However, *these* components of phrenology were seldom laughed at in medical circles, where materialism and localizationism had a tradition dating back to Hippocrates and Galen. Localizationism was even quite popular in the 16th and 17th centuries, thanks largely to Juan Huarte de San Juan's 1575 *Examen de Ingenios para las Ciencias*, a best seller in several languages.

What most scientists did object to were (a) the hypothesis that mental faculties could be read off bumps on the skull, and (b) the particular localizations proposed by Gall and Spurzheim. The former assumption was rightly regarded as false and even ludicrous, and the latter as pure fantasy. If the phrenologists had advanced their particular localizations the way Broca and Wernicke did, namely as hypotheses to be tested, and especially if they had suggested how to test them, they might have persuaded the scientific community. But they held on to these fantastic conjectures with the faith of believers and, moreover, they gave rise to a whole industry. This made them pseudoscientists in the eyes of most of their contemporaries—and in our own. This is why I regard phrenology as half protoscience and half pseudoscience.

The case of alchemy is *toto coelo* different. I do not think that it "functioned in a similar way as a protoscience to chemistry," as Blitz holds. In my view alchemy was pseudoscientific from the start, and this for the following reasons. Firstly, it was based on the four element theory, a most inadequate tool for understanding the bewildering variety of chemical reactions. Secondly, alchemists attempted to accomplish the transmutation of metals by sheer trial and error and with the help of incantations. With such theory and such methods there is no way alchemy could have developed into a science. It was, after all, an occult "science"—not an underground or heterodox one. True, the alchemists did design much of the equipment found in chemistry laboratories until the revolution introduced by electronic instrumentation. But this did not make them scientists: they only were unsuccessful prescientific industrial chemists.

Blitz goes on to examine the early phases of heliocentric planetary astronomy (Copernicus), genetics (Mendel), plate tectonics (Wegener), and quark theory (Gell-Mann). In my view these theories constitute a different ball game altogether. Neither of these theories was unscientific, and every one of them explained and predicted (or retrodicted), or was soon seen to explain and predict (or retrodict), facts that no other theory accounted for. They were merely unorthodox, and although initially many scientists regarded them as false they were generally regarded as scientific. Let us keep scientificity and truth separate, even though scientific research aims at truth. Science is often wrong, but is the best way to discover falsity and the only one to get deep truths about the world.

As for psychoanalysis and parapsychology, part of the trouble with them is that they "explain" too much and too cheaply, while on the other hand they make no predictions, or at least none that have been borne out by facts under experimental control. Blitz credits Freud and Rhine with having "set out theories in a systematic way." Theories proper or mere doctrines? In fact neither of them built any hypothetico-deductive systems, let alone scientific ones, that is, fully testable theories compatible with the bulk of extant knowledge. (Remember Ernest Nagel's, 1959, classical criticism of psychoanalysis.)

However, Rhine must be granted the merit of having designed and conducted experiments. In fact I have explicitly acknowledged that parapsychology is the only experimental pseudoscience, just as astrology is the only pseudoscience that utilizes results from a science (astronomy). But, as many critical students of parapsychology have noted, the experimental controls and the statistical processing of the data have usually been seriously flawed. The point of the experimental method is not just to set up experiments but to design and conduct them reasonably well, and in such a way that they may be replicated. As for Freud and his disciples, neither of them bothered with psychological experiments. Morover, he stated explicitly the irrelevance of "the so-called experimental psychology" to psychoanalysis and, in general, to the study of the mind–body problem (Freud, 1960, p. 25).

Blitz disputes my claim that psychoneural dualism is unscientific for postulating the existence of an immaterial entity—which of course is unattainable by any laboratory tools. He reminds us that Descartes' dualism facilitated animal experimentation. True, but dualism blocked the investigation of the neural mechanisms of the mental. Dualism has turned psychology into *the* anomalous science, the only one not concerned exclusively with the study of concrete objects—in this case higher vertebrates. It took psychology centuries to make strong contact with neuroscience, and even today this contact is not firm enough because of the interference of the myth of the immaterial mind.

Remember also that psychoneural dualism has facilitated the popularization of psychoanalysis and the birth of contemporary information-processing (in particular computationalist) cognitive psychology, which alienates psychology from neuroscience and so far has produced more promises than findings. So, on balance psychoneural dualism has had a negative effect on science, not to mention philosophy. Still, I admit that a good dualist brain is likely to make more important scientific contributions than a mediocre materialist one. (For more on dualism see Bunge, 1977a, 1980, 1985c, 1990a, 1990b; Bunge & Ardila, 1987.)

Finally, a remark on my critic's opening considerations on the so-called demarcation problem. Contrary to Blitz's claim, my demarcation problem was not that of Carnap and Popper. These two philosophers attempted to demarcate science from *metaphysics*, whereas I wanted to demarcate science from *nonscience* and, in particular, pseudoscience. Moreover, contrary to Carnap and Popper, I have always claimed that it is useless to try and find the frontier between science and metaphysics (or ontology), for it does not exist. Indeed, in my view (a) scientific research presupposes a number of metaphysical hypotheses, such as those of the existence, materiality, and lawfulness of the world; and (b) it is possible to build metaphysical theories in harmony with science, as I argued in my 1971 paper "Is scientific metaphysics possible?" Shorter: Science and metaphysics overlap partially (see, e.g. Bunge, 1971a, 1973b, 1977b, 1979a). So, it is not just that "in common with Popper [Bunge] held that metaphysical problems have their place *alongside* scientific problems" (emphasis added) as Blitz states. In my view some problems are common to science and metaphysics, and others are common to science and epistemology. The mind–body problem is one of them: it lies smack in the intersection of science with philosophy.

Finally, I have also tried to show that both Carnap's and Popper's scientificity criteria are simplistic (see Bunge, 1967a, vol. 2, 1982a, 1983b). If the definition in the first section of this reply is adequate, science is far more complex a "thing" than either of those philosophers imagined.

More on these problems will be found in my replies to Feyerabend, Harman, Laucken, Moscovici, and Thom.

BOUDON ON RATIONALITY, GAME THEORY, AND SCIENTIFICITY

As Professor Boudon himself states, the differences between us are few and nearly always a matter of nuance. Let me start with the tricky word 'rationality.' We agree that there are "several forms of rationality"—or, as I prefer to say, several concepts designated by that word. Where the mainstream economist and the game theorist see only one concept of rationality, namely self-interest, Weber saw two (instrumental rationality and value rationality), and I see another five (Bunge, 1987a).

We agree that no human being is fully rational in any of the acceptations of the word. Consequently all "rational" choice models are at best rough approximations. In order to construct realistic models we must take into account what Boudon (1989) has called elsewhere 'subjective rationality' alongside objective rationality—and much more as well. That is, people often act in a wrong way not because they are overcome by passion, or because they make unconsidered snap decisions, but because they hold false beliefs—for example beliefs in magic or witchcraft. (See Boudon, 1990, for an impressive list and analysis of false, fragile, or doubtful beliefs.)

But even injecting hypotheses about "subjective rationality" won't suffice, because every individual acts within some social system or other, and belonging to a system inhibits individual behavior in some respects and stimulates it in others. Consequently any realistic model of social behavior will include from the start assumptions concerning the composition, environment, and structure of the social system(s) concerned. Shorter: Systemism, not radical individualism, is the ticket (see Bunge, 1979a, 1979c, 1985b).

Boudon takes me to task for criticizing the applications of game theory to social science. I do not object to applied game theory just for assuming that all actors behave rationally, hence for being normative rather than descriptive. I object to it because most game-theoretic models involve (a) mathematically undefined utility "functions," that is, pseudomathematical symbols that fail to designate precise concepts, (b) the arbitrary assignment of values to such "functions," and (c) grotesque oversimplifications, for example of war as a two-person Prisoner's Dilemma (for details see Bunge, 1989b).

This holds in particular for the conceptualization of the nuclear arms race as either a Prisoner's Dilemma or as a game of 'chicken.' The arbitrary manner in which the payoff matrices are usually concocted in this case (and in the vast majority of cases as well) is such that, by suitability choosing their entries, one may "prove" that nuclear deterrence is a Nash equilibrium—although we all know that a nuclear war could be triggered by accident as well as by design. A different choice will show that deterrence is unstable, and that the best policy is nuclear disarmament. (There is "collective rationality" as well as individual rationality, and the former involves cooperation: see Rapoport, 1980.) After all, the payoffs or utilities in question are admittedly subjective, not results of measurements.

To be sure some qualitative game-theoretic models may help us understand certain situations, as Boudon (1981) and Moessinger (1991) have shown. The reason is that, contrary to microeconomic models, where every individual behaves in a vacuum and has full control of all the relevant variables, game-theoretic models concern social exchanges. But, given the fuzziness of the concept of utility, only qualitative (ordinal) game-theoretic models have any chance of success.

In a qualitative model, instead of using cardinal utilities one uses qualitative values, such as "counterproductive," "useless," "valuable," and "indispensable," or else "safe," "risky," "hazardous," and "disastrous." But even in this case it is one thing to use in *some* cases such models as heuristic or didactic tools, and another to claim that they are *always* pertinent both as true models and as effective policy tools, so that game theory would be the universal science of human behavior—a thesis certainly rejected by Boudon but admitted by many students of society.

The same holds, *mutatis mutandis*, for all the other rational choice models, particularly for those in microeconomics and in the sociological theories inspired in the latter. Take for instance Mancur Olson's model of collective action, praised by Boudon. According to Olson, in small groups "free-riders" exploit the majority, and this exploitation worsens with size, so that "the larger the group, the farther it will fall short of providing an optimal amount of a collective good" (Olson, 1965, p. 35). In short, all collective action would be bound to fail because every individual is above all a "rational" (i.e., selfish) actor. Shorter: According to Olson sociality and rationality are mutually incompatible, the supremely rational agent being a lonely parasite.

If this is so, why do people engage again and again in collective action? Why do we join or organize unions, political parties, clubs, churches, and voluntary organizations? Is it because, contrary to hypothesis, we are not that rational after all, or because we realize that masses are stronger than isolated individuals? In my view the mistake lies in Olson's model rather than in people. After all, the astonishing political changes that occurred in Eastern and Central Europe during 1989 and 1990 were outcomes of collective actions. (Incidentally, none of these events seem to have been predicted by any of the politologists addicted to game theory. Moreover, all of these events are having grave unintended consequences, i.e., consequences none of the agents desired, hence predicted.) In short, I concur with Albert O. Hirschman's evaluation of Olson's model: It is absurd. (See Hirschman's conversation with Swedberg, in Swedberg, 1990.) In addition it is contrary to fact, for underrating the efficacy of collective action, Olson's model undermines the very marrow of democracy, that is, public participation. In general, radical individualism is a threat to all forms of social order, for every one of these calls for a modicum of devotion to the common good, deliberate cooperative action, and of planning. (Recall Alexis de

Tocqueville's fears about the future of democrary in America if exclusive concern for self-interest, coupled with obsession with law and order, were to prevail.) Notwithstanding the view of libertarians and freemarketeers, competition is insufficient to ensure social cohesion and therefore political stability (see Bunge, 1989a).

Is Hotelling's theory of voting behavior scientific? Boudon holds that "Though it was neither verifiable nor falsifiable, it was genuinely scientific in the sense that it explained a real puzzle." This rings true, and for this reason it would be nice to complete it, removing the clause "in certain circumstances" that makes it just as unscientific as a gypsy's prophecy. However, explanatory power is neither necessary nor sufficient for scientificity. This is why it does not occur in the definition of "science" proposed in the first section of this reply. Indeed, some physical theories, such as thermodynamics and the classical theory of electric networks, account for huge masses of facts without explaining any, for not proposing any mechanisms of the processes they cover. (On the other hand statistical mechanics explains thermodynamics.) Other views, such as psychoanalysis and psychophysical parallelism, seem to explain everything they refer to, but they do not do so in terms of any laws, and they predict nothing.

A key concept here is that of objective law or pattern. Thermodynamics (or rather thermostatics) contains three basic laws and an unlimited number of derived laws, that is, well-confirmed formulas belonging to a theory. (One of its limitations is that these laws involve no mechanisms: this is why they have no explanatory power.) The trouble with most theories in social science is that they contain no laws proper. In particular, the so-called rationality principle, the nucleus of all rational choice models, is not a law, as shown by the fact that we act irrationally nearly as often as not. The "principle" is nothing but a (persuasive) *definition* of "rational action" or "rational agent"—notwithstanding Popper's (1985/1967), opinion that it is "a rule sufficiently near to the truth" to warrant its inclusion in social science.

So, what are we left with? I submit that we are left with an exceedingly weak principle: "If an agent acts *deliberately*, then he does so on the strength of his *beliefs* about the situation in which he finds himself, as well as of his *beliefs* about the possible consequences that his action may have for himself or others. And if his beliefs are sufficiently true, his actions are likely to bring about the desired result." I presume that Boudon concurs with this version of instrumental rationality—for, after all, I learned it from him.

But, if we admit both subjective and objective rationality, we end up with the following statement: "If an agent acts deliberately, then he does so in a manner that is either objectively or subjectively rational." This statement sounds so appealing because it is tautological, that is, logically true. Precisely for this reason it cannot be taken as a postulate of a factual theory. I ask Boudon to resolve this paradox. Suggestion: Replace "subjective rationality" with "motivation" or "intention."

Obviously, I disagree with Boudon's assertion that "it is impossible to associate to the distinction science/nonscience any definite set of explicit criteria." I suggest that my own criteria, summarized in the first section of this reply, be put to the test (I have done so in Bunge, 1985a, 1985b). If found inadequate they should be modified. But we need *some* definite and objective scientificity criteria in order to evaluate research proposals and results, and even to design science policies. I further submit that it is impossible to make true statements about impossibility in the field of knowledge unless one starts from clear-cut axioms and definitions.

On the other hand I agree with Boudon's final assertion, to the effect that certain doctrines, such as Marxism, are neither wholly scientific nor entirely unscientific. Rather, they are, as Boudon says, "theoretical patchworks," some pieces of which are scientific while others are not—at least on my own definition of "scientificity." This is why no minimally fair social scientist rejects Marxism in its totality. (The case of psychoanalysis is totally different.) Incidentally, is it not high time someone wrote a monograph on what can be salvaged from the theoretical and practical shipwreck of Marxism?

FEYERABEND ON SUBJECTIVISM, SUPERSTITION, METASCIENTIFIC SURGERY, AND PSEUDOPHYSICS

Professor Feyerabend, of epistemological anarchism fame, is tolerant to beliefs in the ghostly and the divine but does not tolerate my criteria of scientificity—nor, for that matter, any alternative criteria. He worries that my criteria would "remove large sections of science." But on the other hand he is not concerned over the threat that antiscience poses to science and, indeed, to every rational endeavor.

Why this biased tolerance? Because Feyerabend does not believe in the autonomous existence of the external world: "*Scientific entities* (and, for that matter, all entities) *are projections and are thus tied to the theory, the ideology, the culture that postulates and projects them*" (Feyerabend, 1990, p. 147, original emphasis). "Molecules, for example, the basic entities of chemistry and molecular biology, do not simply *exist*—period! They appear only under well-defined and rather complex conditions" (ibid., p. 149). Even the realistic thesis, that some things are independent of the inquiring subject, "belongs to special projecting mechanisms that 'objectivize' their ontology" (ibid., p. 147).

Feyerabend does not exhibit the "projection mechanism." Nor does he realize that, for his subjectivistic and psychoanalytic projection metaphor to work, he must posit a screen outside the projector—that is, he must admit a reality independent of the inquiring subject, and onto which the latter can "project" the products of his imagination. In short, this is a (wrong) metaphor of knowledge, not a theory of knowledge or epistemology.

What is the basis of this subjectivistic opinion? We are not told. Are skeptics expected to believe it on the authority of Professor Feyerabend? One may suspect that the whole thing rests at least partly on an elementary confusion between things in themselves and our conceptual models of them—for example, between the Moon and a theory of it. No doubt, belief in the ghostly or the divine will alter some of our behavior—but this is no proof of the independent reality of ghosts and gods. However, it must be said in fairness that Feyerabend is not the only one to incur this confusion: many constructivist and relativist anthropologists and sociologists think nowadays along the same line. (For criticisms see Boudon, 1990; Bunge, forthcoming.)

Once the anti-realist assumption has been posited, it follows that ghosts and gods are on the same footing as atoms and stars. (Nelson Goodman wrote about "worldmaking," and he included stars, not only film stars, among the human creations.) This, in turn, implies that parapsychology and theology are just as legitimate as psychology and physics. Anything goes.

The consequence of anti-realism and radical relativism for our hard-won scientific outlook is clear. Our world "was once full of gods; it then became a drab material world. It can be changed again, if its inhabitants have the heart, the determination, and the intelligence to take the necessary steps" (Feyerabend, 1990, p. 152). Is this not an invitation to go back to the Dark Ages or perhaps even earlier, skipping of course the pre-Socratics? Surprisingly, the very same author warns the reader that "Bunge's principle is disastrous for research, bad for education and scientific PR."

The "principle" Feyerabend objects to is my thesis in the target article that "it is foolish, imprudent, and morally wrong to announce, practice or preach important ideas or practices that have not been put to the test or, worse, that have been shown in a conclusive manner to be utterly false, inefficient, or harmful" (p. 132). My critic picks on the verbs 'announce,' 'practice,' and 'preach.' He is right concerning the first: one should indeed try and publish pieces containing important ideas even if they have not been tested—not however if no tests are possible or if the test results have been unfavorable. But from the context of the original sentence it is obvious that I intended to say that one should not 'announce [such ideas or practices] *as true or effective*.'

As for the signification of 'practice' and 'preach,' we disagree. *Pace* Feyerabend, one does not *practice* an idea when "applying it to a variety of cases": in this case one tries out, checks or tests the idea. This is what the psychologist does when trying out the principles of learning on animals of different species. On the other hand, he *applies* those principles in doing behavior therapy. By playing on the ambiguity of the word 'application' in ordinary language one can make even worse mistakes. One such mistake is the view that, since scientific explanation involves an "application" of a theory (i.e., its use in a deductive argument), "scientific explanation is not (pure) science but an application of science" (Van Fraassen, 1980, p. 156). Tell this to an evolutionary biologist intent on explaining the extinction of some biopopulation, or to a physiological psychologist engaged in explaining blindsight in terms of a phylogenetically older visual system involving the tectum instead of the striate cortex.

As for my injunction not to preach untested ideas or practices, it stands. Of course one tries to *persuade* people about ideas or practices which one has found to work. All I said is that honest people do not advertise or sell items that have never been tried, or that have been tried and failed. That there is a thriving market for "lemons," cultural as well as industrial, is true. I find this regrettable, whereas epistemological relativists like Feyerabend rejoice in it.

I agree that all scientists use, often tacitly, a number of irrefutable philosophical (logical, ontological, and epistemological) principles, such as the principle of lawfulness. (Kant might rightly have called them "regulative principles" and regarded them as synthetic a priori.) I have stated and defended this thesis more than once (e.g. Bunge, 1967a, vol. 1, Ch. 5, Sect. 5.9). And I have even built a system of scientific ontology (Bunge, 1977b, 1979a), and another of scientific epistemology (Bunge, 1983a, 1983b), around such principles. But such philosophical presuppositions of scientific research, though irrefutable, are confirmable. Indeed, they are confirmed or, if one prefers, vindicated every time they contribute to shaping a successful new hypothesis or method. For example, although it is impossible to prove that everything happens according to law, this principle is vindicated every time a new law is found.

Another principle that underlies scientific research is that of realism, that is, the hypothesis that there is a world out there which can be known if only partially and gradually. Without assuming this principle nobody would engage in an exploration of the world, and nobody would take precautions to minimize his interference with the object under research, as well as to minimize the risk of self-deception. But it so happens that the principle of realism does not meet with Feyerabend's approval: as we saw at the beginning of this reply, he rejects scientific realism. He states instead that scientists "are sculptors of reality" (Feyerabend, 1990, p. 151). Are we to understand that the newly born is given a formless world, which he then proceeds to endow with a definite shape? Are we Platonic demiurges rather than a highly successful animal species, so successful that we have been able to create the very tools of our own extinction?

It should be clear by now that Feyerabend and I do not mean the same by the word 'science.' For one thing, he refuses to demarcate science from nonscience. In particular, he is indifferent between science and pseudoscience, to the point of having demanded "equal time" for creationism and evolutionary biology, medicine and faith healing, and so on and so forth, in the name of his epistemological relativism and even in the name of democracy. For another, Feyerabend reads scientific formulas in quaint ways. Let me show three typical cases.

First example: According to Feyerabend's response to my article the addition of velocities in special relativity "is an operation which differs from the addition of numbers." But if the components of velocities are not numbers, what are they? We are not told. The truth is of course that they *are* numbers, since by hypothesis they are vector components and moreover measurable quantities. Only, the relativistic formula for the composition of velocities is different from the classical one.

Second example: According to Feyerabend we find "a mixture of mentalism and holism in the quantum theory." True, some popularizations of this theory, as well as certain philosophical writings about it, hold that atoms and the like do not exist by themselves but come into being as a result of acts of observation. But this subjectivistic interpretation of quantum mechanics can be laid to rest by ferreting out and analyzing the axioms of the theory regardless of any popular and philosophical accretions. The outcome of this operation is that the axioms of the theory make no reference whatsoever to any observers, let alone to their minds (Bunge 1967b, 1973a, 1985a). In short, there is nothing spooky about the quantum theory. But of course irrationalists object to axiomatics: they feel more comfortable in the dark.

As for quantum "holism," presumably Feyerabend refers to the so-called non-separability of the components of a system even after they have drifted apart. This is indeed a counter-intuitive (though true) feature of the quantum theory. Granted, its experimental confirmation in the early 1980s, via the refutation of Bell's inequalities, was initially announced as the downfall of realism. But this was a coarse misinterpretation: it boiled down to confusing "realism" with "classicism" (Bunge, 1985a, pp. 208–217). Actually that experimental result does not involve a violation of any of the philosophical principles inherent in scientific research and stated in my definition of "science" at the beginning of these replies. It simply confirmed what we knew all along: that the things I have called "quantons" do not behave like "classons."

Third example: Feyerabend's provocative and popular book *Against Method* (1975) contains just two mathematical formulas, which occur on p. 62 of the 1978 edition. He got them both wrong. The first formula, which he calls 'the equipartition principle,' is actually the Maxwell–Boltzmann distribution function for a system of particles in equilibrium. Incidentally, the constant occurring in the correct formula is not R, the universal gas constant, but Boltzmann's k. This is no small mistake, because Feyerabend's formula is dimensionally wrong.

The second formula, Lorentz's, does not give "the *energy* of an *electron* moving in a *constant magnetic* field B" (my emphases), as Feyerabend believes, but the *force* that an *arbitrary electro*magnetic field $<E,B>$ exerts on an *arbitrary* charged particle. (Incidentally, the constant c is missing in Feyerabend's copy—which, again, makes it dimensionally incorrect.) Feyerabend substitutes the second formula into the first and, of course, he gets an odd result that, in a mysterious way, leads him to speculate on the (nonexistent) magnetic monopoles imagined by his teacher Felix Ehrenhaft. But the substitution cannot be made, because (a) the second formula does not give us an energy, which occurs in the first one, (b) the first formula refers to a system of particles whereas the second concerns a single particle, and (c) unlike the energy, which is a scalar, the force is a vector, and therefore it cannot occur by itself in the argument of an exponential function, which is only defined for scalars.

This confusion of Feyerabend's between the concepts of force and energy reminds me of the discovery once announced by a professor in a South American provincial university. He combined the formula "$E = wh$" for the potential energy E of a body of weight w at a height h above the ground, with the formula "$E = h\nu$" for the energy of a photon of frequency ν, where now 'h' stands for Planck's constant, and derived his revolutionary formula "Weight = Frequency." One suspects that he would have applauded Feyerabend's famous slogan "Anything goes." At any rate, my point is that Feyerabend's science is not the one I have learned, taught, and contributed to. This explains in part why our respective philosophies of science are mutually perpendicular. By the way, this remark meets Feyerabend's demand that "we should explain the personal reasons for our dislike" of ideas or procedures.

Let us now tackle Feyerabend's methodological question: "Assuming a disembodied soul exists—is it not clear that we shall have to use special methods to identify it?" Answer: No. Firstly, the assumption collides head-on with physiological psychology, in the light of which Feyerabend's question is about as reasonable as asking 'How could we detect heart-beats independently of the heart?' Since there are so many genuine unsolved problems in psychology, why *should* any serious researcher waste his time trying to test an old groundless superstition? Secondly, how *could* anyone design and build a scientific ghost catcher if, by hypothesis, ghosts are immaterial and therefore even more elusive than neutrinos, which are hard enought to detect? And what properties would a ghostmeter measure? (For a spoof of a mathematical theory of ghosts see Bunge, 1967a, Ch. 8, Sect. 8.2.)

Laboratory instruments are designed, built, and operated on the assumption that they, and the objects they help observe or measure, satisfy exclusively physical or chemical laws. On the other hand, as the father of experimental parapsychology stated, "Parapsychology deals with experiences and behavior that fail to show regular relationships with time–space–mass and other criteria of *physical* lawfulness" (Rhine, 1960, p. 71, emphasis in the original).

Since physical instruments do not work for disembodied souls, parapsychologists resort to human ghost catchers, that is mediums to whom they attribute paranormal abilities—as long as these last. And, as they do not tire to tell us, mediums cannot perform adequately in the presence of skeptics. Thus, parapsychologists beg the question: they take the paranormal for granted. Their motivation is clear: "the search for psi is now, as it has been since the formal beginning of empirical parapsychology over a century ago, the quest to establish the reality of a nonmaterial aspect of human existence—some form of secularized soul" (Alcock, 1987, p. 565).

Finally, Feyerabend wants "to show what delicacy of mind [subtlety?, finesse?] is needed to participate in the scientific enterprise." Having participated in this enterprise, I wish to confirm that subtlety is indeed desirable. But I may add that it is not necessary to do creditable science, and that it is useless unless accompanied by a concern for clarity and evidence. The history of mathematics and theoretical physics has refuted Pascal's famous contrast between the *esprit de finesse* [Feyerabend's "delicacy of mind"] and the *esprit de géométrie*. Indeed, the most powerful theories (e.g., real analysis and quantum mechanics), and the most powerful experiments (e.g., those conducted at CERN) combine subtlety with hard-nosedness, depth with exactness, imagination with the caution inherent in the very methodological skepticism which Feyerabend rejects. Shorter: Let us not confuse subtlety with softness, or what William James called 'tendermindedness.' A hard "nose" is more potent than a soft brain.

HARMAN ON NOETIC AND HYLETIC SCIENCES

Dr Harman places himself squarely in the noetic or spiritualist tradition, and he does so with a commendable clarity that is absent from most of his mentors, from the mystics to the phenomenologists. But his is an impossible mission: to persuade his reader that ordinary science should accept all the data of untutored experience, including reports on psi phenomena, out-of-body and near-death experiences, the dogmas of traditionalist Chinese medicine, Tibetan Buddhist psychology, Native American folklore and, of course, mystical experience. He asks us to *accept all these stories at their face value.*

Scientists take these and similar stories with at least a grain of salt. If intrigued, they attempt to explain their origins in illusion, delusion, self-deception, or myth-making. Alternatively, as in the case of Neal E. Miller's classical research on yoga, they explore neurophysiological accounts—but after having made sure that the stories are true, that is, that there *are* facts to be explained. Shorter: Whereas the "noetic scientist" admits uncritically the bulk of the spiritualist tradition, the "hyletic" or materialist scientist spies this tradition with a jaundiced eye and with the conviction that all the beliefs of that kind can be accounted for in terms of scientific psychology or social science. Is it not a bit late to try and turn the calendar back by more than four centuries?

This does not entail that science must disregard subjective experience, in particular consciousness. True, behaviorists were not interested in the mental, but how many radical behaviorists are there left? Judging from the psychological literature, behaviorism is practically extinct, and the study of mental phenomena in man and in other higher vertebrates is more vigorous than ever. But all the recent findings in this field have been the product of scientific research, not of armchair (in particular phenomenological) speculation (see, for example, Bunge & Ardila, 1987, in particular the chapter on consciousness). More on this below.

However, it is one thing to admit the existence of the mental and another to conclude to the possibility of resurrecting a spiritualist world view complete with disembodied souls, spooks of various descriptions, mysterious "synchronies" and actions at a distance, and the like. True, the popular parascientific literature is full of such alleged entities and processes. But who has constructed precise enough theories about them and presented solid evidence for them?

Harman mentions the "morphogenetic fields." I heard about these alleged fields half a century before Rupert Sheldrake and others resurrected them in recent years. (We used to laugh at such "fields" in my student circle in backward Argentina before World War II.) But how are those "fields" described except by their alleged effects? Which are the field equations? What instruments, if any, have been used to measure their intensity? In short, where is the evidence? As for Sir John Eccles' "new" interactionist psychoneural dualism, it has been taught and preached since Antiquity and perhaps even earlier. But has it been articulated into a theory proper? (Could it be so articulated, given the fuzziness of the concepts of mind and of mind–matter interaction employed by Eccles?) And where is the experimental evidence for this popular view? Finally, Roger Sperry's "downward causation," or action of mind on body, is part and parcel of Platonism, psychoanalysis, and armchair psychology. But who has clarified the very notion of such a causal action? And where is the evidence for it aside from introspective reports?

Of course, Harman is likely to argue that we do not *need* any scientific evidence for such fantasies. In fact he urges us to accept nearly all "self-reports of subjective experience," and thus overcome what he calls "the parochial nature of Western science." He does admit that we must filter out some of these reports. But his own filtering mechanism, namely consensus without either theoretical criticism or controlled experiment, is unacceptable to scientists, although it has been very much in vogue ever since Thomas S. Kuhn took it from Ludwik Fleck. In fact, scientists do not organize popularity contests to evaluate hypotheses or theories. Sometimes they are quite lonely in upholding unpopular views. Moreover, any popular belief on difficult matters is likely to be false.

The central point which Harman refuses to admit is that one of the features which distinguishes science from nonscience is what Robert K. Merton (1957) called "organized skepticism." This skepticism happens to have been born in the West two and a half millennia ago. There is no such thing as ancient Eastern science, let alone an Eastern cradle of wisdom. Any historian of science knows that the East imported the science born in ancient Greece, and then reborn in Western Europe in the seventeenth century. I submit that Harman's invitation to "enrich" Western science with oriental superstition and mysticism is nothing short of an invitation to commit intellectual hara-kiri.

And now for some misunderstandings. I plead guilty, and proudly too, to the charge of scientism (see Bunge, 1983b). Contrary to Hayek and Popper, I hold that antiscience and pseudoscience, not scientism, are the enemies of reason. I also believe that indicting scientism as "the counter-revolution of science" (Hayek, 1955) is a sly attempt to ban objectivity from social studies, thus making it easier to have people accept the moth-eaten dogmas of mainstream economic theory, particularly in its aprioristic Austrian version.

But Harman's charges of "extreme positivism, determinism, and behaviorism" only provoke my hilarity. Concerning positivism, see below my reply to Serge Moscovici. As for determinism, given my work in quantum mechanics and my long-standing view on the objectivity of chance (e.g., Bunge 1951a, 1967b, 1985a, 1988a), the charge has no substance if "determinism" is equated with "classical (or Laplacian) determinism." (See Bunge, 1959b, for my broad concept of determinism, which embraces probabilistic laws.) Finally, the accusation of behaviorism is groundless as well, as anyone can check by thumbing my books on psychology (Bunge, 1980; Bunge & Ardila, 1987) or my articles "Phenomenological theories" (1964) and "From mindless neuroscience and brainless psychology to neuropsychology" (1985c). In particular, it is not true that Bunge denies psychosomatic disorders and "summarily dismisses the idea that positive emotions might positively affect the immune system and hence promote healing." I accept these facts but, unlike Harman, I claim that they can be explained in purely biological terms, in particular as actions of the cortico-limbic supersystem on the immune system (Bunge & Ardila, 1987, pp. 145–146; Bunge, 1989g, Ch. 3). I have even defended this view in a journal of psychosomatic medicine (Bunge, 1987b). In short, I am not a behaviorist.

Another mistake of Harman's is his equating positivism and materialism. The Vienna Circle, officially called the *Ernst Mach Verein*, adopted Mach's definition of a physical entity as a comparatively constant collection of sensations—a view actually taken from Mill, and which is as far from materialism as it is close to

Husserl's egology, and even closer to Husserl's later "life world" philosophy. (Rudolf Carnap refined and elaborated that definition in his famous 1928 book *Der logische Aufbau der Welt.*) Moreover, one of Mach's explicit goals was *die Überwindung des Materialismus.* He even thought he had attained this goal by "defining" the concepts of mass and force in kinematical terms—which, alas, proved to be impossible (see Bunge, 1966). In short, positivism is not materialist but phenomenalist, and as such it is close to Berkeley's immaterialism, as Lenin and Popper noted long ago.

It is also well known that logical empiricism is anti-realistic. For example, in his widely circulated *Kleines Lehrbuch des Positivismus* (1939) Richard von Mises dismissed the question "Is there a real world?" as ill-phrased and metaphysical. Hans Reichenbach regarded realism as an interesting but dispensable hypothesis. And, given Mach's phenomenalism, it is likely that he and his disciples would have approved, just as Harman does, of the constructivist thesis that "we participate in the construction of reality." In short, logical empiricism is neither materialist nor realist. Since Bunge is unabashedly both, he is definitely not a logical empiricist. Hence none of the justified attacks on positivism affect Bunge. Again, the evidence is there for all to read—for all, that is, who care for evidence.

Nor is Bunge a radical reductionist. For one thing, unlike Harman, who on the word of physicists admits the reducibility of chemistry to physics, I have challenged this thesis. I have done so on the strength of an examination of some typical formulas of the quantum theory of chemical reactions (Bunge, 1982d, 1985a). As for biology, I agree that it contains concepts and methods alien to physics and chemistry (Bunge, 1979a, 1979d, 1985b). I have even argued that the same is true of psychology, in spite of the ontological reduction of the mental to the neurophysiological (Bunge, 1989f, 1990a). However, Harman would not remain satisfied with this: he demands good old vitalism and spiritualism. He will get both from the popular New Age publications and even from some philosophers but not from the scientific research literature. Maybe this is one of the reasons that in his commentary he does not quote a single scientific paper.

In conclusion, nobody in his right mind denies nowadays that we do have minds—as have animals of other species. The question is not just to talk about the mind, but to study it seriously in order to know it better. Now, which discipline has contributed most to our knowledge of the mind: the noetic or the hyletic sciences, in particular arm chair spiritualistic (in particular phenomenological) psychology, or scientific—that is experimental and mathematical—psychology? The reader will decide.

KREWERAS ON TRUTH, MATHEMATICS, AND PROBABILITY

Professor Kreweras complains that my paper fails to discuss the nature of truth and its relation to philosophy, as well as the special status of mathematics. True, but one must not expect too much from a paper that is not explicitly devoted to those matters. I have dealt with them in my eight volume *Treatise on Basic Philosophy,* particularly in *Interpretation and Truth* (1974b), *Exploring the World* (1983a), and *Philosophy of Science and Technology, Part I* (1985a).

However, I hasten to state that I agree wholeheartedly with Kreweras's thesis that mathematics is very different from all the other sciences. It is formal, not factual: as Plato, Leibniz, Grassmann, and others thought, mathematics deals with *êtres de raison* and accordingly constructs truths of reason, not of fact (see Bunge, 1974a, 1974b, 1985a). What happens is that the English word 'science' does not ordinarily encompass mathematics. It is also true, and unfortunate, that Leibniz's distinction between *vérités de raison* and *vérités de fait* is not popular nowadays, particularly since Quine's assault on the analytic/synthetic dichotomy. But this only goes to show the sad state of the philosophy of mathematics.

I readily accept Kreweras' correction: One should say that mathematicians try to prove or disprove their *conjectures*, not their *theorems*. Still, the expression 'proof of theorem T' is common. One speaks, for example, of Fermat's last theorem, although it has not yet been proved (or disproved).

On the other hand we seem to disagree on subjective probability. I grant of course that prior probabilities are assigned before observation—by definition of "prior." But this mode of assignment need not be subjective in the way B. de Finetti, H. Jeffreys, L.J. Savage, R. Carnap, and I.J. Good thought. It should be just a provisional estimate subject to correction in the light of empirical tests. In this regard probability estimates are similar to the "eyeballing" of lengths, time intervals, or weights. And one does not improve on the accuracy of the estimate by repeated applications of Bayes's theorem but by higher-order perturbation calculations or improved experimental designs.

One is justified in hypothesizing probabilities provided (a) they occur in a stochastic context (e.g., in a random process model) and (b) one is willing to let observation or experiment decide about the truth of such hypotheses. Subjectivists ignore these two cautions. For instance, they are likely to assign a probability to such logically possible events as the U.S. armed forces launching an attack on Iraq on 6 December 1990. Since such an event would be the outcome of a carefully planned strategy and a deliberate decision, it would be anything but a member of a random process. Hence any assignment of a probability to such an event would be foolish, even more so than an assignment of utility. Psychologists, in particular Daniel Kahneman and Amos Tversky, have taught us that credences, or degrees of belief, fail to satisfy the axioms of the probability calculus. If they did we would all be fully rational beings. (For more on both legitimate and illegitimate interpretations of probability see Bunge, 1988a.)

Kreweras finds it difficult to understand what I mean by a "theoretical proof of the possibility of an empirical fact." Perhaps the following example will clarify this matter. The emission of electromagnetic radiations of certain wavelengths is regarded as impossible for corresponding to "forbidden [atomic] transitions," for example transitions involving the violation of certain "rules" (actually laws) concerning the total angular momentum of the atom in question. If nature were to ignore any such *Verbot* (as it used to be facetiously called), the quantum theories of atoms and molecules would have to be altered. True, some "forbidden transitions" become possible when the atom is immersed in an external electric or magnetic field. However, such new possibilities are theoretically predictable.

But this is not the case of paranormal phenomena: These are not supported by any such measurement, let alone by a scientific theory. Moreover, they are definitely at variance with physiological psychology, according to which the detachment of mental processes from the brain is just as impossible as the detachment of running from the limbs. Recall that, for physiological psychology, "The mind is to the brain as rotation is to the wheel" (Uttal, 1978). I agree of course with Kreweras's assertion that scientists should be willing to cooperate in parapsychological experiments—if asked, which is not often the case. But they should heed Wilhelm Wundt's advice: if you attend a spiritualist séance or a parapsychological experiment, go along with a professional stage magician—someone like James Randi or Henry Gordon, who knows the tricks of the trade and moreover is alert to possible flaws in the controls. (Better still, combine psychology with stage magic, the way Ray Hyman and James Alcock have done.) This is precisely how the Committee for the Scientific Investigation of Claims of the Paranormal and its affiliates operate.

My only strong disagreement with Kreweras concerns his assertion that the entire science of the real world is "essentially metaphorical"—implying that it does not give us a sort of picture, however distorted, of reality. If taken literally, this assertion amounts to Plato's denial, in the *Timaeus*, of the possibility of natural science and the inevitability of myth. Since this view became obsolete shortly after Plato's time, with the work of Theophrastos, Archimedes, and a few other scientists, it would be unfair to attribute it to Kreweras. Rather, I suspect that what he has in mind is the thesis that science can never give a perfectly accurate and complete account of the real world. But of course I concur with this thesis. Scientific realism is critical, not naive: the so-called picture (or mirror) theory of knowledge is false (see, for example, Bunge, 1959a, 1963, 1967a, 1973b, 1983a, 1983b).

However, if Kreweras believes that, unlike factual science, *all* of mathematics is perfectly exact and certain, then we disagree. In fact the whole of approximation theory—in particular the methods of numerical integration, and the techniques for constructing approximate solution of differential equations—speaks against the Platonic idea of overall perfect mathematical exactness. Furthermore, the aperiodic revolutions in the foundations of mathematics, such as the ε–δ, the set-theoretic and the category-theoretic revolutions, are warnings that we must not expect to attain final certainty (see Bunge, 1962). However here, just as in factual science, it is always possible, at least in principle, (a) to improve the accuracy of results with the help of known methods, or of methods to be invented, and (b) to construct, as Hilbert said, ever deeper foundations. Admittedly, this trust in the perfectibility of mathematics and science is an article of faith. Only, this faith is philosophical, not religious, and so far it has paid handsomely. Do we know of any reason for giving up this belief?

LAUCKEN ON METHOD, PHENOMENA, PHENOMENALISM, PHENOMENOLOGY, AND ARMCHAIR PSYCHOLOGY

Professor Laucken starts with a quote from Heidegger that contains the phrase *das Sein des Seiendes,* which I am unable to understand and therefore to translate. The being of being?, the being of Being?, the Being of being?, the Being of Being? And what would any of these phrases mean anyway? To take another example, what is one to make of a typically Heideggerian sentence like the following? "Das Sein des Daseins besagt: Sich-vorweg-schon-sein-in-(der-Welt-) als Sein-bei (innerweltlich begegnendem Seienden)" (Heidegger, 1927, p. 192). Why should anyone take this gibberish, which is untranslatable even into decent German, as serious philosophy?

Later on Laucken quotes approvingly one of Husserl's angry tirades against naturalism. This one I understand, particularly in the light of his later work, *Die Krisis der europäischen Wissenschaften* (1936), where he blamed the exact sciences, naturalism and objectivism for the crisis of the European sciences—rather than totalitarianism and its lackeys, in particular his star pupil Heidegger.

Anyone who, like Laucken, feels at home with Husserl's phenomenology and Heidegger's existentialism, has a clear advantage over me and can claim victory before even starting the race. In fact I confess that, even after half a century's reflection on those writers, I find Husserl obscure, Heidegger totally dark, and both of them altogether irrelevant to the pursuit of truth about anything at all—when not actively hostile to such pursuit. Worse, given the hostility of both writers not only to contemporary science but also to modern logic—the universal analytic tool par excellence—it would seem that any attempt to clarify their writings would be condemned by their followers as a matter of principle. Presumably, Laucken would condemn such attempt as a category mistake just as bad as trying to understand ourselves in a scientific way, or as attempting to construct a scientific (and even mathematical) ontology. Yet, although I know that in his eyes I am dead, I refuse to lie down.

Laucken starts his attack on my paper by stating that no research method is neutral with regard to its object. He asserts that method and object determine one another. If by 'method' is meant 'special method' or 'technique,' I agree. In fact, it would be absurd to employ, say, any special neuroscientific technique, such as magnetic resonance brain imaging, in historical research—but on the other hand that method is yielding important results in physiological psychology and neurolinguistics, which should be astonishing to anyone who, like Laucken, believes in the immaterial soul and in armchair psychology.

But if by 'method' one means the *general* scientific method, then we disagree. A study of thousands of research papers in a variety of natural, social and biosocial sciences, has persuaded me and many others that the *general démarche* is the same in all of the sciences. (By the way, the hermeneutic school, from Dilthey to Gadamer, ignores the very existence of such mixed sciences as social psychology, demography, anthropology, and bioeconomics.) Such methodological monism leaves room for a boundless variety of techniques and hypotheses fitting the special objectives of the particular disciplines. That this is so can be seen from the schematic characterization of the general scientific method proposed at the beginning of these replies.

Serious (nonphilosophical) hermeneutics, as practiced by the professional historians of ideas, for example, students and translators of the Bible, follows the same pattern. Thus, the 1990 English version of the Bible is the outcome of intense work by a team of biblical scholars who were able to solve a number of puzzles, in particular ambiguities, found in the originals as well as in the previous translations. They have achieved this not by arbitrary interpretation, let alone text "deconstruction," but by putting forth and checking precise hypotheses in the light of both the extant documents and new historical findings (Metzger, 1990).

Notwithstanding philosophical hermeneutics, from Dilthey to Gadamer and Derrida, the historian's work does not differ in *general* method from that of the student of natural historical processes, such as the cosmologist, the geologist, or the evolutionary biologist. All of the historical sciences, whether natural or social, use the general scientific method and moreover they provide nomological explanations (even if they seldom if ever find laws of their own), as Max Weber himself admitted (see, for example, Albert, 1988).

For example, Fogel and Engerman (1974) explained the prosperity of the Southern plantations, and the obstinacy with which the Confederates fought in the American Civil War, by showing that the plantation system was extremely profitable because gang-system farms were substantially more efficient than free farms. Likewise, political analysts are busy nowadays trying to explain the recent events in the former Soviet bloc in terms of a number of causes, both internal and external—much as an evolutionary biologist would attempt to explain the origin, radiation or extinction of a biopopulation. Hence Laucken is mistaken in asserting that causal analysis is exclusive of natural science.

Yet in all sciences causal analysis, though necessary, is admittedly insufficient: It must be combined with a consideration of accidents and the search for probabilistic laws, some of which tell us the probability that a given cause will have a certain effect (see, for example, Boudon, 1984; Bunge, 1982b). Think of quantum physics and chemistry, genetics, neuroscience, and learning theory. Therefore Laucken is mistaken in asserting that "The world-concept of the natural sciences is causally closed." Strict causalism is characteristic of Aristotelianism, not of modern science (Bunge, 1959b).

Laucken is mistaken too in sharing Ernst Cassirer's view that natural science is alien to everything "personal," such as emotions, thoughts, and intentions. This is what human psychology is all about, and there can be little doubt that psychology is at least in part a natural science. (See any publication in contemporary psychology, or Bunge, 1990a, 1990b; Bunge & Ardila, 1987.)

Having misunderstood the world-view of natural science, Laucken attributes to me "the materialism of the natural sciences," which in turn he identifies with physicalism, that is, vulgar materialism. According to him my materialism makes no room for phenomena (appearances), expectation, feeling, intention, pain, or any other kind of mental phenomena. That this is not true, can be seen by perusing my books *A World of Systems* (1979a), *The Mind–Body Problem* (1980), *Scientific Materialism* (1981), and *Philosophy of Psychology* (1987).

What is true is that, along with all scientific psychologists and philosophers of a scientific bent, I want the mental to be investigated scientifically. On the other hand Laucken seems to wish the mental to remain as a given, as part of what Husserl called the *Lebenswelt*, and we ordinary people call 'the world of everyday (or ordinary) experience.' But, as Kant said, what is a datum (*Gabe*) for perception is a problem (*Aufgabe*) for the understanding. In other words, the *Lebenswelt* should be investigated scientifically rather than just be talked about.

Take phenomena (or appearances), of which Laucken, following Husserl, makes so much. Physicists do not ignore phenomena: on the contrary, they often start from them. But they do not limit themselves to what appears to us. For example, they explain the apparent motion of the planets in terms of their motion around the sun, and they use exact formulas relating the two reference systems. And what are psychophysics and the physiology of perception but the scientific investigation of the relations between physical stimuli and their perceptual counterparts? Phenomenology has no room for such bridges between the physical and the phenomenal because they are testable and they link a part of the *Lebenswelt* with a part of the naturalistic world view, which Husserl despised. So much the worse for narrow-mindedness and shallowness.

Had everybody remained in the phenomenological *Lebenswelt*, typical of primitive and archaic thinking, ancient atomism, statics, and optics would never have emerged, and the Scientific Revolution would never have occurred. The distinction between appearance and reality marked the trial of Galileo, one of the first to distinguish between primary and secondary qualities.

Is it true that, as Laucken claims, "Phenomenal existence stands on its own"? Psychologists are intent on explaining phenomena as part of *reality*—sometimes as brain processes triggered by external or internal stimuli. For example, we perceive the Moon much larger in the horizon than overhead. This, known as "the Moon illusion," is a real process going on in our brains. (One of the explanations is that, when the Moon is on the horizon, we compare it with much smaller things in our visual field, such as trees and houses.) And when a Don Quiote perceives windmills as armed knights, such a perception, though wrong, really happens in his sick brain, and the psychiatrist attempts to find out why it happens. Moreover, the psychiatrist can alter his subject's perceptions by prescribing him certain drugs.

In sum, phenomena (appearances) are not self-subsisting. They are real brain processes which can be controlled and, at least in principle, understood in scientific terms. Have phenomenological philosophy and psychology made any contribution to the understanding of any phenomena at all? So far as I know, they have not even discovered any phenomena, whereas scientific psychologists have found plenty and explained some. Here are some all-time favorites: the nose illusion (credited to Aristotle), after-images, phantom limb pain, the figure-background and the Necker cube oscillations, the Zöllner illusion, the Devil's fork, the wall-paper effect, the Hermann grid, and Ames' distorted room illusion—not to mention the tragic delusions of schizophrenics and paranoiacs. What phenomena have been named after Husserl or any other phenomenologist?

Of course Laucken is right in stating that the languages of the *Lebenswelt* are different from those of the sciences. For one thing, the latter are immensely richer than the former. However, the two are not disjoint, because scientists use their senses and attempt to account for everyday life. Thus, a developmental psychologist, a sociologist, and a social historian may endeavor to explain the preferences that economists take for granted. But, in order to account for everyday occurrences, one must go beyond ordinary knowledge, and this calls for some technical language matching the technical ideas. For example, we all know that at room temperature metallic objects feel colder than wooden ones. Even school children know that the metallic object draws more heat from our hand than the wooden one because the conductivity of the former is higher than that of the latter. This explanation of a common phenomenon involves the concept of conductivity, which goes beyond phenomenal data.

Laucken attempts to prove that my philosophy (with which he is obviously not acquainted) is inconsistent. He does so by noting that I use the concepts of logical consistency, experimental test, and text and its interpretation. Laucken claims that these do not belong in material reality. Like Plato, Bolzano, Husserl, and many other idealists before him, he would like all ideas to form a "world" of their own.

My rejoinder is that ideas are no more self-subsistent than appearances. In particular, logical consistency, checking, and text interpretation are brain processes of the mental kind. By the way, contrary to Laucken's allegation, I have never derided texts. I have only criticized *textualism*, that is, the monstrous idea that the world is a huge text, whence it can only be understood with the help of semiotics or hermeneutics. Heidegger (1987/1953) expressed rather clearly, for a change, this idealist thesis: "Im Wort, in der Sprache werden und sind erst die Dinge" (p. 11).

For a contemporary scientist—nay, for a sane person—things do not become real only when talked about, and there are no ideas in a world without intelligent beings. As a matter of fact most of the ideas contemporary scientists think about were unknown, hence did not exist, only one century ago. But, since ideas are brain processes, they do exist in the material world. Even the mistaken idea of a disembodied idea exists really, namely in some brains. So do Hamlet and Asterix. But the ability to distinguish one's ideas from their external referents is one of the tests of mental sanity.

However, this does not entail that we must study ideas exclusively the way physiological psychologists intend to study them. On the contrary, we must study them in all possible ways. In particular, we must study their logical and semantical features. And this requires abstracting ideas from brains and society, that is, *feigning* that there are ideas in themselves. In other words, in order to be realistic and productive we must join a materialist (but not physicalist) ontology with a sort of methodological and semantic dualism, as I advocated at the very beginning of my *Treatise* (Bunge, 1974a, pp. 26–30).

The idea that ideas constitute a "world" in and by themselves is harmless as long as it is recognized as a useful analytical fiction. But the assumption that the ideal "world" is self-subsistent is just a case of reification—as we have known from the time of Aristotle. Worse, the conceptual closure of each "world" and the erection of barriers between the various "worlds," proposed by Laucken, is counterproductive for the following reasons. First, it blocks the communication flows between them, hence the mutual fertilization of domains of experience and research. Second, it prevents one from thinking of *the* world, hence from constructing a comprehensive world view or ontology and, particularly, one continuous with science and technology.

It is up to the interested reader to judge whether my philosophical system (Bunge, 1974–1989), is inconsistent and barren, as Laucken claims. (For different evaluations of my philosophical work see, for example, Weingartner & Dorn, 1990.) In my view these strong adjectives apply instead to the mixture of phenomenology, existentialism, hermeneutics, and armchair psychology which Laucken advocates. Add "irrationalist" and "irrelevant" for good measure.

MOSCOVICI ON MYSTERIES AND THE IRRATIONAL

I will skip over Professor's Moscovici personal attacks, but I cannot avoid answering three of his charges, because they are relevant to the matters at hand. He accuses me of being a fervent positivist, antitheoretical, and lacking in curiosity. These three charges serve Moscovici to construct a straw man, but they are groundless. (It may be remembered that Harman and Laucken resorted to the same tactics. Name calling is always easier than studying.)

First, my many explicit criticisms of positivism for giving a distorted picture of scientific research and narrowing its scope (Bunge, 1943, 1951a, 1951b, 1954, 1956, 1967a, 1983b, 1988b, etc.); my indictment of the Copenhagen interpretation of quantum mechanics for being more faithful to positivism than to the actual practice of physicists (Bunge, 1955, 1959a, 1967b, 1973a, 1977c, 1985a, 1988c, etc.); my espousal of and contribution to scientific realism both in epistemology and in the foundations of physics (Bunge, 1967a, 1967b, 1973a, 1973b, 1983a, 1983b, 1985a, 1985b, etc.); my revindication of the problematics (though not the methodics or the philosophy) of mentalist psychology (Bunge, 1977a, 1980, 1981, 1983a, 1985b, 1985c, 1990a, 1990b; Bunge & Ardila, 1987, etc.); and my work in ontology (Bunge, 1959a, 1959b, 1973b, 1977a, 1979b, etc.) should suffice to refute the charge of positivism. What happens is that, in Europe and Latin America, anyone who respects science and criticizes obscurantism passes for a positivist. This mistake has a historical explanation: for a long time positivists were the only ones to oppose the antiscientific school philosophies dominant in the universities.

Moscovici's accusation that I am an enemy of theory suggests that he means by 'theory' something quite different from what logicians, mathematicians, physicists, chemists, and other "hard" scientists mean by that word. In fact, I have been a professor of *theoretical* physics, and between 1944 and 1988 have published two books (Bunge, 1960, 1967b) and a number of papers in this field. I have also published contributions to applied mathematics (Bunge, 1971b, 1976), theoretical biology (Bunge, 1978, 1979b), theoretical psychology (Bunge, 1977a, 1980, 1985c; Bunge & Ardila, 1987), and theoretical sociology (Bunge, 1969, 1974b, 1975; Bunge & García-Sucre, 1976). Besides, nearly all of my philosophical work has been in the field of theoretical (or systematic) philosophy. It contains a number of theories proper, particularly in semantics, epistemology, ontology, metaphysics, and ethics. Moreover, I have written a lot against the antitheoretical bias of empiricists, particularly operationists and behaviorists (e.g., Bunge, 1967a; Bunge & Ardila, 1987).

But of course not every speculation is a theory, and not every theory is scientific. I submit that, for a theory about facts to be scientific, it must (a) be a theory proper, that is a hypothetico-deductive system, not a jumble of opinions; (b) be testable, however indirectly; (c) hold some promise of solving some open problems or show the way to further investigation, and (d) be compatible with the bulk of certified knowledge.

Condition (b), testability, excludes from factual theories any reference to such fictions as Descartes' *génie malin*, Maxwell's demon, the homunculus, disembodied souls, rational economic man, the perfectly competitive market in general equilibrium, and many others. Not that one may not imagine any such entities. One may certainly speculate about them, handling them either as ideal types, or for heuristic reasons, or just for fun. (For example, the fiction of the freely falling observer helped Einstein construct or at least divulge his theory of gravitation.) But such fictions must not be mistaken for facts. One hopes scientific theories to fit reality. To be sure, such hope may be dashed, but when this happens one tries again.

Finally, the charge that I am bereft of curiosity may be judged by anyone willing to scan the list of my publications since 1939. They deal with applied mathematics, nuclear and atomic physics, foundations of physics, mathematical sociology, semantics, epistemology, ontology, axiology, ethics, philosophy of physics and chemistry, philosophy of psychology and social science, the history of ideas, education, and a few other subjects. However, Moscovici is right in stating that I am not curious about either unalterable dogmas, such as Freud's, or about mysteries, that is, inscrutable entities and insoluble problems. I gladly leave all this to the gullible. The scientific credo includes the principle "In science there are unsolved problems but no mysteries."

True, once in a while one finds distinguished scientists who fall for the mysterious. The eminent physicist Wolfgang Pauli, whom Moscovici quotes, was one of them. In his old age he fell for C.G. Jung's archetypes and even published a paper about them. But this earned him no credits in the scientific community. And Moscovici omits to tell us that Pauli did not mix any Jungian fantasy with his physics. Likewise, Kepler kept his astronomy free from his astrology, Newton his mechanics from his alchemy, Penfield his neuroscience from his religion, and so on. All of them admitted tacitly something Moscovici rejects, namely that there *is* a frontier between science and nonscience. Developing a "nose" for it is part of becoming a scientist. Whoever fails to develop it is likely to be a pseudoscientific prey or predator.

That there is a frontier between science and nonscience is not a matter of fact to be discovered, say, by sociologists or historians of science. It is strictly a matter of logic: $S \neq$ *not-S*. Of course the criteria for telling science from nonscience are culture-bound, hence changeable along history. But, if anything, such criteria are becoming increasingly exacting. And in any event the changeability of the concept of science does not entail that (a) there is no overlap between the successive concepts of science or (b) that there are no and can be no demarcation criteria at any given time, as Moscovici implies.

As suggested above, scientificity criteria are not the exclusive property of philosophers but are used, albeit mostly in a tacit fashion, by scientists themselves. For example, a scientific psychologist asked to referee a research grant proposal or a paper would turn it down if it were to involve the search for the life force, souls of the dead, super-egos, ectoplasms, or even genes coding for Jung's archetypes.

Moscovici disapproves of the search for a demarcation line between science and nonscience. Like Feyerabend and Thom, he believes this search to be hopeless and even pernicious. On the other hand I believe this search to be philosophically, scientifically, and socially important. Its philosophical importance is obvious: how can we philosophize about science if we do not have some idea of what science is, hence how it differs from nonscience? Scientists too need to have some criteria of scientificity, particularly sociologists and historians of science, if they wish to make some contributions to their science and to refrain from teaching pseudoscience under the guise of science. Finally, science policy makers use some scientificity criterion every time they are asked to evaluate or implement some research or education project. That they may employ the wrong criterion, for example, immediate practical use, or use of computers, is regrettably true. But this is only a further argument in favor of the need to work out and diffuse an adequate philosophy of science, one dealing with genuine problems in authentic science.

Moscovici himself exemplifies the dangers of using an incorrect criterion of scientificity when, towards the end of his comment, he makes science and technology responsible for the brutal onslaught on the environment. If he had used a correct criterion he would have pointed his finger at industrialists, politicians, and the military, instead of blaming science. Basic science is morally neutral: it just explores the world. Technologists do figure out how to change the world, and they do so with the help of scientific findings. But they only provide blueprints for change. Such blueprints remain designs or programs unless industrialists, politicians or the brass have them implemented.

If what Moscovici wants to say is that morality is at least as important as knowledge, and that technology ought to be controlled by social values, I agree wholeheartedly (see, for example, Bunge, 1989a, 1989c, 1989d). But I go much further than Moscovici because I indict mainstream economics—which he approves of on the whole. I do so not only on scientific grounds but also on moral grounds, namely because it is unconcerned with the environment, it disregards social justice, and justifies unemployment and austerity measures that only hurt the poor (Bunge, 1982c, 1985b, 1986, 1989a).

Moscovici questions the survival value of rationality, indicts the Enlightenment, and claims, in the most radical functionalist vein, that we need what he calls *la machine à faire des dieux.* (Is this an echo of Voltaire's elitist and cynical epigram, *si Dieu n'éxistait pas, il faudrait l'inventer*?) I thought that modernity, which has doubtless triumphed in many respects, is secular and rationalist, as Max Weber did not tire to stress. And I find it hard to forget the suffering and destruction brought about by such myths as religious fanaticism, nationalism, racism, fascism, the "dictatorship of the proletariat," monetarism, and the consumerist ideal. Is it really necessary to insist on the noxious political roles of the dialectical mumbo jumbo, as well as of the *Blut und Boden* and the free market myths? Obscurantism is at best a form of escapism, at worst a smoke screen and a tool of oppression. Long live the Enlightenment!

There is no room left to justify my attacks on some of Moscovici's idols, in particular psychoanalysis, mainstream economics, and von Neumann's theory of measurement. I must refer to my earlier publications on these matters. For psychoanalysis—from which I recovered shortly before entering the university—see Bunge, 1967a, 1985b, 1990a, 1990b; Bunge & Ardila, 1987. (For more authoritative evaluations see Fisher & Greenberg, 1977, Perrez, 1979, or Van Rillaer, 1980, and his commentary on my paper.) For mainstream economics, particularly its irrelevance to today's economy, see Bunge, 1982c, 1985b, 1986. (For more authoritative evaluations see Eichner, 1983; Leontief, 1982; Morgenstern, 1972; Thurow, 1983. Even better: try to fit unemployment, inflation, stagflation, oligopolies, government subsidies to agriculture and the arms industry, corporate planning, bargaining, and social programs into the neoclassical framework.) As for von Neumann's quantum theory of measurement, see Bunge, 1967b, 1985a; Bunge & Kálnay, 1983a, 1983b. (Even better, see Bell, 1987; Cini, 1983; Lévy-Leblond, 1977.) By the way, it was a surprise to learn that Moscovici defends this highly sophisticated piece of mathematics. He must have studied and checked it carefully—unless, perish the thought, he is a true believer in ideas he does not understand.

PERREZ ON ORDINARY KNOWLEDGE

I agree almost sentence by sentence with Professor Perrez's comments. In particular, I agree wholeheartedly with his thesis that ordinary knowledge is different from both ideology and scientific knowlege. I also agree that everyday knowledge deserves being better studied by psychologists and epistemologists. Moreover, I deplore—as I am sure Perrez does too—that Piaget's trailblazing studies on the genesis and development of our key everyday concepts and hypotheses has been practically discontinued. I am afraid we must blame computerist psychology for the shift of focus and, in particular, for the neglect of psychogenesis and, in general, of developmental psychology. But I also believe that the mechanistic approach to cognitive psychology has run its course for having exhibited its barrenness.

I only wish to make a few supplementary remarks. The first is that ordinary knowledge does overlap scientific knowledge. Moreover, the intersection increases as a result of formal education and exposure to mass media. For example, it is nowadays rather common for high school youngsters to discuss subjects their parents knew little or nothing of, such as galaxies and black holes, atoms and molecules, continental drift and the greenhouse effect, biological evolution and species extinction, heredity and DNA splicing, surrogate motherhood and Alzheimer's disease, and much more.

My second point is that ideological or mythical thinking too is evolving, though not as fast. In particular, such fictions as UFOs, extraterrestrial visitors, and the magical power of crystals are part of a new secular mythology that is replacing the traditional religions. Worse, some Gallup polls show that belief in the impossible increases with education: it would seem that our schools teach superstition along with science, or perhaps that literacy opens the door to dark rooms as well as to libraries. So, we are in the paradoxical situation that our children know more secular nonsense as well as more science and technology than we did at the same age.

Clearly, teaching scraps of scientific information, or assigning a few experiments, has proved insufficient to form a scientific attitude and a scientific world view. School curricula should include courses in critical thinking, teaching not only how to recognize logical fallacies but also how to go about evaluating factual truth claims. In other words, we should be teaching not only content but also method, and this not just in special courses but in all of the courses in the sciences, technologies and humanities. In addition, we should be discussing some of the fashionable pseudoscientific beliefs. Instead, these are often taught as true in the classroom. More on this in the Conclusion.

THOM ON SCIENTIFICITY AND PLATONISM

Professor Thom, of catastrophe theory fame, is ambiguous with regard to scientificity criteria. On the one hand he declares them to be illusory just because they are historically changeable rather than absolute. (This reminds one of Lakatos's argument that, because the foundations of mathematics are changeable, they do not exist.) On the other hand he admits that "there exists a gradient of scientificity." But surely this idea of a gradation of the sciences from "hard" to "soft" presupposes that there *is* something like a set of conditions that are jointly necessary and sufficient for perfect scientificity. (Assuming, for the sake of simplicity, that all such conditions have the same weight, a field of knowledge that satisfies n out of a total of N conditions may be assigned a degree of scientificity equal to n/N.)

We seem then to agree that there are scientificity criteria after all. We only disagree on which are the proper ones. In particular, Thom accepts explicitly three of the five conditions I had proposed in the section titled "The scientist's skepticism": realism, rationality, and systemicity. But he rejects the requirements of materialism and empirical testability. Let us see why.

Thom opposes materialism because he believes in the autonomous existence of mathematical objects. Concerning mathematical objects and, in general, ideas, Thom is a Platonist, whereas I am an Aristotelian. Platonism is of course harmless in pure mathematics. Moreover, Platonism has the great virtue of leaving mathematicians the free hand they need, and which empiricism and pragmatism would like to tie up. No wonder that Platonism is the spontaneous philosophy of mathematics of mathematicians, as even Dieudonné once admitted. (For examinations of the main philosophies of mathematics see Bunge, 1962, 1985a.)

However, if one cares for systemicity—condition (e) in my paper—as Thom and I do, then a commitment to Platonism entails building or accepting an overall idealistic ontology. But it so happens that Thom (1990a) has recently "catastrophized" Aristotle's ontology, which is anything but idealistic. And this violates the condition (c) of rationality, which Thom adheres to.

In any event, Platonism has disastrous results in factual science, not only for eschewing empirical tests but also for positing self-existing forms (in particular archetypes), and even formal causes in addition to efficient causes. In fact this is the hub of Thom's famous *Stabilité Structurelle et Morphogenèse* (1972). This fascinating and irritating book was a contribution to classical morphology, which is strictly descriptive. But it eschewed the problem of disclosing the mechanisms of the emergence and transformation of biological forms as results of inner processes (such as chemical reactions accompanied by diffusion), which are constrained and in part steered by environmental factors.

In typically Platonic fashion, Thom proceeds from the outside to the inside, and from function to organ. Example: "the formation of the organ results from a conflict between a primitive field with a functional avocation (or meaning) and an organic raw material that resists it and imposes upon it genetically determined paths of realization (chreodes)" (Thom, 1972, p. 212). Moreover, because of Thom's resolute opposition to molecular biology and neo-Darwinian evolutionary biology, his book may have discouraged the search for such mechanisms.

Worse, Thom endows shapes with a formative force, something that has reminded Thom himself of Hans Driesch's ghostly entelechy. Example: "all phenomena of life could be reduced to the manifestation of a geometric being that one would call the *life field* [*champ vital*]" (Thom, 1972, p. 158). Therefore, in Thom's own words, his work "may be characterized as a sort of *geometrical vitalism*" (Thom, 1972, p. 167). I am told that the net result of the publication of that long-awaited work was a setback to theoretical biology, because biologists saw their worst fears and prejudices about mathematical biology confirmed.

Judging from the questions he addresses me, Thom believes not only in disembodied forms: he also believes in the autonomous existence of empty space, energy, and de Broglie waves. My quick reactions are as follows. Space does not exist by itself but is a certain structure of the totality of concrete things. Hence, if there were no things there would be no space—which is what Epicurus, Aristotle, Leibniz, and others thought. (For a full-fledged exact relational theory of space see Bunge, 1977b; Bunge & García-Maynez, 1977.) Energy (not to be confused with radiation) is not an entity but a property of every individual concrete thing. (The energy of a physical entity is always represented by a function or an operator whose domain is a Cartesian product, at least one of the factors of which is a set of concrete entities—see, for example, Bunge, 1967b.) And the de Broglie waves do not exist by themselves either. True, they used to be said to be "associated" with such entities as electrons and photons. But actually they are rather abstract state functions serving to construct physically meaningful quantities, such as probabilities and averages. Hence de Broglie "waves" do not propagate and, a fortiori, they carry no energy. Shorter: They are not physical waves. (To realize this it is enough to recall that the wave function for a system composed of N quantons is defined in a $3N$-dimensional space, not in ordinary space.) In sum, properties only exist insofar as their carriers exist. Better: Only things-endowed-with-properties exist (see Bunge, 1977b).

Thom asks also whether I regard general systems theory as scientific. *Respondeo*: this "theory" is actually a jumble of three ingredients: (a) a few generic, though not fully general, exact theories of systems of several kinds, such as automata theory, linear systems theory, control theory, network theory, and general Lagrangian dynamics; (b) a fully general exact theory (Bunge, 1979a); and (c) a large number of holistic pseudotheories. The theories of kind (a) are scientific or technological; (b) is philosophical though compatible with science; and the stuff in category (c) is neither scientific nor philosophical, and it has given systems theory a bad name (Bunge, 1977d).

As for the requirement (d) of empirical testability, Thom rejects it on the grounds that "science can tolerate for quite a long time hypotheses that have not been proven." Certainly, in defining "scientificity" one must only require testability in principle. It is only when evaluating truth claims that one requires empirical "proof" or, rather, confirmation by rigorous tests. And these need not be direct.

Obviously, the empirical testability condition is not required of pure mathematics, which only deals with *êtres de raison*. Hence Thom's defense of catastrophe theory, by way of showing that Rolle's intermediate value theorem has no "value for action," is beside the point. Maybe this point would not have arisen were it not for the different senses of the word 'science' in English and in French. In the target article 'science' is always used in the first sense, that is, as synonymous with "factual (or empirical) science" (see my reply to Kreweras).

Regrettably, something more than a linguistic ambiguity is involved in our argument. Indeed, Thom does not believe that theoretical factual science is in any need of empirical tests. In fact, at the 1983 World Congress of Philosophy he went so far as to propose closing all biological laboratories, for regarding all biological problems as being purely topological, hence soluble a priori. And only recently Thom proposed to substitute old natural history for experimental science; moreover, he extolled the power of empathy with physical objects combined with the phenomenological *epoché* (Thom, 1990b). For example, the student of impact should put himself "*dans la peau de la boule choquée*" ("under the skin of the target ball"). Unfortuately he omits to say how to perform this *Verstehen* feat, and he presents no evidence for the alleged superiority of this method over that of physics. After all, he once confessed: "the problem of truth has not concerned me directly" (Thom, 1983, p. 9). In any event, I regard such speculation as an instance of magical thinking.

Still, I do agree with Thom that not all theories need be directly testable. In fact, of a hypergeneral theory—such as general Lagrangian theory, or Thom's own important work on dynamical systems—we should only require that (a) it captures some important features common to all the members of a genus of things, and (b) it be useful as a foil for constructing specific testable theories. Thus we are justified in proposing a general theory provided it can be enriched with a set of subsidiary hypotheses to yield a specific theory or model. In obvious symbols, $G \cup S = M$. (For the general theory-special models relation see Bunge, 1983b.)

As I argued in 1969 against Popper in his presence, there are degrees of testability corresponding to the degrees of theory generality (Bunge, 1973b). The more specific a theory, the more directly testable it is. And in every discipline we should welcome theories of different degrees of generality, hence testability. For example, psychology needs (a) a general theory about the basic neural mechanisms common to all mental processes, and (b) a host of specific theories (models), every one compatible with the general theory and concerned with a special kind of mental process—for example, models of learning (of items of different kinds), visual perception, imagination, will, decision, and empathy. In short, Thom and I agree on the need for general and even hypergeneral theories, that need not and cannot be directly testable. But we disagree on the need to use results of observation, measurement, and experiment *at some point* in the research process.

VAN RILLAER ON HOW TO FOOL MANY PEOPLE MUCH OF THE TIME

I fully agree with Professor Van Rillaer's criticisms of psychoanalysis. They are all the more valuable for coming from a convert from psychoanalysis. Indeed, as he shows himself, this particular variety of pseudoscience (along with Marxism) induces a remarkable skill for deceiving oneself by turning every exception to a thesis A into an instance of a thesis B designed to protect A. (Your dream did not have an overt sexual symbolism because it had a latent one. You do not realize that you harbor the death wish because you are repressing it. Your criticizing psychoanalysis only exhibits the resistance phenomenon—and so on.)

But, unlike Van Rillaer, I do not think that Popper was right in attacking psychoanalysis for being irrefutable. In my view, while some psychoanalytic fantasies (particularly the pairs of mutually protecting hypotheses) are indeed irrefutable, others are refutable. And of the latter, some have been refuted while others have never been tested (Bunge, 1967a, 1985, 1990b; Grünbaum, 1984). For example, as Peter Medawar said, the death wish hypothesis goes against the grain of the entire biology. The sublimation hypothesis has been refuted by uncounted cases of happily married creative artists, scientists, and technologists. Other psychoanalytic hypotheses could be tested, experimentally or statistically, if someone bothered. For example, the convenient dogma that paying for psychoanalytical treatment has a healing effect could easily be tested by setting up two homogeneous groups of patients, one paying and the other getting the same treatment for free. But I grant that it might prove difficult to find psychoanalysts willing to participate in this or any other experiments: they are too busy seeing patients or writing fiction. We ordinary people must pay for our mistakes, whereas psychoanalysts make us pay for theirs.

Would psychoanalysis become scientific if it were purged from all its irrefutable conjectures? According to Popper, yes, for in his view refutability is the one and only seal of scientificity. In my view, no, because (a) on Popper's criterion all false theories would qualify as scientific, which is absurd; (b) direct refutability can only be required of low level hypotheses and theories, not of high level ones; and (c) there is much more to scientificity than testability. *Ad* (a): Astrology, which was refuted centuries ago, has never been scientific. *Ad* (b): General (classical or quantal) field theory, linear systems theory, general control theory, and statistical information theory are so general that they are untestable by themselves, although they may become vicariously testable upon specification. If a given case does not fit the theory, one does not reject the theory but reproaches oneself for making the wrong choice of theory or for not having enriched it with adequate subsidiary assumptions. Popper's criterion only applies to very narrow factual theories, such as a theory of a given atom, or of the learning of nonsense syllables, or of the relation between inflation and unemployment. (See above my reply to Thom, and Bunge, 1973b, Ch. 2.) *Ad* (c): Science is so complex an object that it cannot be characterized by a single property. Indeed, our definition of "science" in the first section of this paper exhibits it as a sort of decahedron, not as a point.

When weighing the merits and shortcomings of a view concerning any concrete thing or process we should check whether or not it complies with the following requirements.

(i) *Intelligibility*. Is the view clear or obscure? If obscure, can it be elucidated and eventually formalized, or is it inherently obscure and therefore not susceptible to refinement? As Van Rillaer has shown, psychoanalysis is full of fuzzy concepts, such as "libido" and "latent"—not to mention "soul" and "id." And nearly one century of psychoanalytic writing has only produced new words to designate the old vague notions. (The imprecision of Freud's texts is such, that his English translators got away with translating *Seele* [soul] as 'mind', and *es* [it] as 'id.')

(ii) *Logical consistency*. Is the view internally consistent or does it contain contradictions? If it does contain inconsistencies, can these be removed by dropping or altering some of the assumptions? Because psychoanalysis is full of fuzzy concepts—hence of vague propositions—and because such constructs do not obey the laws of logic, it is hard to say whether the doctrine in question is consistent.

(iii) *Systemicity*. Is the view a system or part of one, or is it a stray conjecture that cannot enjoy the support of any other bit of knowledge? If stray, can it be developed into a theory or embedded in one? Clearly, psychoanalysis is not a stray hypothesis but a set of conjectures. But nobody seems to have succeeded in organizing this set into a hypothetico-deductive system, that is, in the form Postulates–Definitions–Theorems–Corollaries. The believers because they do not care much for logic, and the unbelievers presumably because they have more important things to do. Be this as it may, the fact is that psychoanalysis is a doctrine and a practice but not a theory proper. And this makes it hard to test. Indeed, if one does not know whether a testable proposition B follows from an assumption A, then the empirical confirmation of B won't shore up A, and its refutation won't undermine A.

(iv) *Literalness*. Does the view make any literal statements or is it just an analogy or metaphor? If an analogy, is it shallow or deep, barren, or fertile? And is the metaphor indispensable or can it be replaced with a literal statement? It is well-known that psychoanalysis—and, for that matter, the entire mentalistic psychology—contains numerous analogies, some of them taken from physics and others from biology (see Gentner & Grudin, 1985). These are just analogies: they are not meant to be put to the test by means of experiments, but to persuade.

(v) *Testability*. Can the view be checked conceptually or empirically, or is it impregnable to criticism and experience? We know the answer to this one in the case of psychoanalysis: Some of its key components are insensitive to experience. The Oedipus complex–repression pair is a classical example.

(vi) *Empirical support*. If the view has been tested, have the test results been favorable, unfavorable, or inconclusive? We know the answer with regard to psychoanalysis: few if any of its component hypotheses have turned out to be true in the only way recognized in scientific psychology, namely by replicable experiment.

(vii) *External consistency*. Is the view compatible with the bulk of knowledge in all fields of scientific research? Clearly, psychoanalysis makes no contact, nay, refuses to make any contact with experimental psychology, social psychology, or neuroscience. In particular, the psychoneural dualism inherent in psychoanalysis collides head-on with physiological psychology and neuroscience.

(viii) *Originality*. Is the view novel? And does it solve any outstanding problems? With regard to psychoanalysis, the answer to the first question is this. Psychoanalysis was original—nay, far out—when first proposed. But, as Van Rillaer states, it has not evolved over the past half century: For decades it has been dead scripture and fodder for schoolmen. The answer to the second question is, in my view, that psychoanalysis has not solved satisfactorily any psychological problem. Instead, it has created a big problem: that of cleaning up the intellectual pollution it has created in the soft areas of the humanities and in the mass culture.

(ix) *Heuristic power*. Is the view barren or does it raise new and interesting research problems? In an effort to be fair, Van Rillaer claims that psychoanalysis has stimulated research in scientific psychology. But he only quotes one work of 1939 and another of 1943, and does not tell us whether either of them is still valid. (Psychologists know that most of the results of psychological research done at that time have proved to be ephemeral.) In any event, I have never heard of any psychoanalytic laboratory.

(x) *Philosophical soundness*. Is the view compatible with the philosophy underlying scientific research? That is, is it epistemologically realistic or does it involve a priorism? And is the view naturalistic or does it posit ghostly entities such as immaterial things or processes, which by hypothesis are inaccessible to experimental control? I believe that Van Rillaer and I agree that psychoanalysts proceed in an aprioristic way and that they posit immaterial entities, such as the unconscious and the superego, which are experimentally inaccessible—which of course is not to deny that we undergo unconscious mental processes or suppress some desires in an unconscious fashion.

When subjected to this battery of ten tests, psychoanalysis hardly scores two points out of ten: one for consistency (giving it a very generous benefit of the doubt) and another for originality (forgetting that we are in 1991 not in 1901). Not an impressive score compared with the achievements of scientific psychology over the past few decades.

In sum, I feel amply vindicated by Van Rillaer's evaluation of psychoanalysis, and I wish it were better known wherever this pseudoscience is still popular, namely in the countries where psychological research is still embryonic.

CONCLUSION

Judging from some of the spirited attacks on methodological skepticism, I was right in claiming that there are plenty of burrows of dogmatism and obscurantism in the groves of academia. Voltaire and Mark Twain would have been shocked, for they took it for granted that obscurantism would recede with the advancement of science, technology, and liberty. They did not count on the active promotion of obscurantism by the entrepreneurs of junk culture, let alone on the help these would get from some academics. Nor did those distinguished skeptics count on the disillusionment with science caused by unbridled industralization, war, and environmental degradation, a disillusion helped by the popular confusion between science and technology—a confusion commonly found in the faculties of arts.

The mass production, marketing, and consumption of counterculture has become a social phenomenon that deserves close scientific examination. In particular, social psychologists should construct gullibility and popularity indicators and check them for reliability. They could use them in designing gullibility tests and popularity surveys. (GQ tests might turn out to be more valid, valuable, and cheaper to administer than IQ tests, and P scores would be helpful to educators.) Developmental and social psychologists should build and test theories concerning the factors that promote, and those that inhibit, the formation of a critical attitude in childhood and adolescence.

Social scientists too should become involved in the study of the counterculture. For example, economists should try to measure and correlate the total outputs of genuine culture and junk culture. (Is junk culture, unlike genuine culture, subject to diminishing returns? If yes, the pair cannot satisfy a Cobb–Douglas production function.) And cultural historians, sociologists, and political scientists should try and disclose the mechanisms that link the rise of the counterculture with economic and political crises. The current mushrooming of superstitions of all kinds in the U.S.S.R. and Eastern Europe, under the wing of glasnost and in the wake of the bankruptcy of Marxism, offers a unique opportunity for studying the process *in vivo*. And the decline of enrollment in the North American schools of science and technology since about the mid-1970s calls for studies that may serve as a basis for the measures required to reverse this disquieting downward trend (see Bunge, 1989e).

We need plenty of good scientific studies of antiscience and pseudoscience, as well as of their public "perception," not only to halt the contamination of the scientific and educational communities, but also to defend and expand democracy. Inded, since there is no democracy without a well informed, alert, and politically involved public, rationality is just as essential to democracy as it is to science, technology, and the humanities. Only rational people can engage in the inquiry, reasoning, evaluation, criticism, debate, and cooperation called for by modern democracy. Raise sheep, and you'll prepare for dictatorship. Only goats are good democracy material.

But in modern society the raising of goats starts at the top, that is, at the university, which is where teachers are trained. It involves not only lighting candles but also fighting those who try to blow them out. In other words, we have to face resolutely the thankless task of criticizing those who have taken the facile path of uncritical thinking and pride themselves of being so "open minded" that they can absorb and teach all manner of intellectual garbage. We must insist that university professors have the duty to live up to ever higher standards of intellectual rigor, as well as to refrain from teaching pseudoscience and antiscience. Academic freedom only concerns the search for truth and its teaching. It is not a licence to talk hot air.

However, any research into the popular "perception" of science presupposes some clear answer to the philosophical question 'What is science?' In turn, any such answer must be put to the test of the sciences, pseudosciences, and antisciences of the day. Tell me which pseudoscientific and antiscientific beliefs you hold and I'll tell you what your philosophy of science is worth.

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