

Philosophical Problems of the Internal and External Worlds

Essays on the Philosophy
of Adolf Grünbaum

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Sciences and Pseudosciences

An Attempt at a New Form of Demarcation

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The Demise of the Demarcation Problem

In an article appearing in an earlier *Festschrift* honoring Adolf Grünbaum (Laudan 1983), Larry Laudan argued persuasively that the attempt to distinguish science from pseudoscience by employment of one or another of the proposed demarcation criteria is now dead. What he had specifically in mind is that demarcation criteria employing verificationist and falsificationist theories of meaning have failed to accomplish the required goal.¹ However, he obviously retained confidence in what he called “our intuitive distinction between the scientific and the non-scientific” (ibid., 124) and admitted that some future successful attempt to mark the difference could not be ruled out. The intuition referred to provides a rich catalog of pseudosciences, “flat Earthers, biblical creationists, proponents of laetrile or orgone boxes, Uri Geller devotees, Bermuda Triangulators, circle squarers, Lysenkoists, charioteers of the gods, *perpetuum mobile* builders, Big Foot searchers, Loch Nessians, faith healers, polywater dabblers, Rosicrucians, the-world-is-about-to-enders, primal screamers, water diviners, magicians, and astrologers” (ibid., 121). Evidently, these assorted pseudosciences make “crank claim[s] which make ascertainably false assertions” (ibid.). One might reasonably add: *if literally construed*.²

The concept of literal construal of claims to knowledge will play an important role in my argument for a new kind of demarcation criterion. But first, I want to review some ways of distinguishing between science and pseudoscience that do not in any obvious ways invite exhumation of the corpses Laudan has rightly buried. What continues to nag at me is Laudan's adoption of a centuries-old intuition. Many of us have this same intuition. I am convinced that in some way we can express its message in fairly precise terms. Although we may regard as inadequate certain forms of verificationist and falsificationist *criteria* of demarcation, the *problem* of demarcation seems to remain.

I will briefly review four philosophical strategies that have some merit as attempts to distinguish science from nonsense. All four of the strategies have a bearing on the new strategy I will propose.

Forms of Reliable Knowing, Science, and Mental Aberration: The Kantian Strategy

Kant's well-known critique of classical substantive metaphysics employs an explicit form of demarcation between science and non-science of the following kind (see Butts 1986b for a more detailed account). First, Kant's analysis of human cognition seeks to locate those species-universal mental structures that define reliable knowing. These structures are the now familiar forms of space and time and the categories functioning as a priori, hence necessary, forms of the possibility of knowledge. Kant argues that this complex cognitive machinery, applying as it does only to items given in sensation, effectively distinguishes between reliable knowledge (compliant with the cognitive structures) and pseudoknowledge (such as that alleged to be knowable in classical metaphysics). Furthermore, it was Kant's view that the premier instantiation of the formalism of spacetime and the categories is given by Newtonian physics: *The most reliable form of human knowing is mathematical physics* (see Friedman 1986 and Butts 1986a, c).

At various stages in his career, acceptance of this conclusion meant for Kant that biology can only be construed as a research strategy pursued in the interests of obtaining good taxonomy, not a full science (the position of *Critique of Judgment*), and that chemistry is not a science but an artful classification scheme (the position of *Critique*

of Pure Reason and *Metaphysical Foundations of Natural Science*).³ It also meant that substantive metaphysics has no reliable epistemic credentials, and that some claims to knowledge of that which lies beyond experience are indistinguishable from claims characteristic of mentally deranged persons (Kant's judgment on the claims of Swedenborg to possession of ways of access to a world of spirits). It was also part of Kant's strategy to propose that various forms of mental activity that deviate from, ignore, or distort application of the epistemic formalism can be classified as maladaptive mental disorders. Any such forms of maladaptive behavior will produce claims that cannot be justified as scientific (see Butts 1986b, 282–318).

In summary, here is Kant's strategy: identify the norms of reliable knowing, show that these norms are instantiated by the best science of the day, then classify pseudosciences as aberrations of these norms (metaphysics and other claims to transcendent knowledge), which in some cases involve genuine mental illnesses. There is much to recommend Kant's strategy, but unfortunately in its Kantian form it cannot be sustained. Surely we cannot require that the norms of reliable knowing must be instantiated by any science of a particular period if what we take science to be is the *content* of a particular successful theory. This defect has led many to look for distinguishing marks of science in its *methods* and what have come to be called its *virtues*. Before turning attention to such strategies, it must also be remarked that the Kantian form of demarcation can only be applied with profit after cognitive science and psychopathology have joined forces. In the end, the Kantian strategy cannot be successfully accomplished philosophically; it needs to be itself a part of science. This consequence should be applauded, for it cautions us to look for demarcation and the definition of cognitive normalcy, if they are to be found at all, not in a priori philosophical arguments, but in the results of combined theoretical and empirical investigation. The failure of the Kantian program as a philosophical account of demarcation itself points the way to some empirically ascertainable distinguishing marks of the sciences.

Prediction and Control: The Rescher Strategy

Many philosophers and scientists have directed our attention away from the specific content of sciences, and toward the expressed goals

of scientific inquiry. The goals are many: accuracy of description, mathematical precision in the formulation of theory, retrodiction, and taxonomical adequacy. Two goals, however, are thought to occupy prominent positions largely unique to science: accuracy in prediction, and success in control of the environment. Nicholas Rescher (1970) presents a particularly clear and persuasive argument to the conclusion that what he calls "explanatory frameworks" (pp. 135–45), including the frameworks of the sciences as well as, for example, numerology, astrology, and black magic, can each be criticized *en bloc*, but only a posteriori. The sciences win in the competition, not because of superior content, but because of pragmatic success in prediction and control that is unmatched by other explanatory frameworks (see Rescher 1977).

Part of Rescher's strategy is clearly correct. We have learned that metaphysics (and Kant's transcendental arguments) provide no a priori grounds for favoring certain sciences over alleged nonsciences, or, alternatively stated, for preferring a definition of cognitive normalcy over claims to the possibility of heightened modes of awareness, or altered states of consciousness. Furthermore, the failed empiricist effort to locate a demarcation criterion that would single out science as the sole dispenser of cognitively significant sentences further confirms the absence of a priori grounds that would allow a decision between explanatory frameworks.

It seems unlikely, however, that one can dismiss astrology, appeals to narrative discourse, or fundamentalist creationism, or psychoanalysis, solely because these explanatory frameworks fail as accurate systems of prediction leading to at least partial control of aspects of the environment. It all depends on what is at stake. Explanatory frameworks operate to further chosen objectives. If we know what these objectives are, we can show, by admittedly a posteriori means, that those ends cannot be realized in ways promised by the given explanatory framework. Pseudosciences are shown to be defective in explanatory potency by discovering that they possess impoverished means for realizing *aims they themselves articulate*.

In different ways, the failure of the Kantian strategy and the partial success of the Rescher strategy point in the same direction. We must look for demarcation by paying attention to what kinds of things conceptual frameworks are attempting to accomplish by saying what they do.

Science and "Cognitive Accountability": The Grünbaum Strategy

Adolf Grünbaum's many writings on the epistemological status of psychoanalysis (see, for example, Grünbaum 1980, 1983, 1984) offer no new theory of demarcation. However, in other papers, his work does seek to illuminate the kinds of problems that must be faced by those who would deal with the demarcation problem. In several papers (see, for example, Grünbaum, 1989a), he has argued vigorously against Popper's claim that psychoanalysis is a pseudoscience because it is not falsifiable. Popper's conclusion is of course based on his acceptance of the falsificationist criterion of demarcation. What Grünbaum shows is that a thoroughgoing *inductivist* study of psychoanalysis reveals that some of its hypotheses are indeed falsifiable, and falsified, thus refuting Popper's claim that Freud's theories are not falsifiable, and at the same time casting doubt on the merits of the falsificationist criterion of demarcation.

Grünbaum's (1989b, 1990) study of the hermeneuticians' attempts to free Freud's theories of what Freud himself insisted is the *natural* scientific status of those theories also has an indirect bearing on the problem of demarcation. Jaspers, Habermas and Ricoeur think of the clinical situation in psychoanalysis as one eventuating in hermeneutical insight into kinships of meaning, where such affinities are also regarded as *causal*. In this way, the hermeneuticians seek to free Freud's theories from the corpus of natural sciences so that they can take their place among the human sciences. Grünbaum demonstrates, again by employment of inductivist methods of analysis of causal connection, that connections between meanings, even when very strong, do not suffice of themselves to establish causal connections. Thus the hermeneuticians' efforts to show that psychoanalysis is a pseudo *natural* science are unavailing. Such efforts, in effect, simply reinstate long-standing epistemological problems associated with the human sciences, for example, epistemic claims on behalf of empathy or *Verstehen*.

Grünbaum's contribution to the solution of the demarcation problem consists in his invitation to learn just how difficult it is to establish the scientific credentials of a theory. He does this, not by offering a criterion of demarcation, but by demonstrating that psychoanalysis, however brilliantly conceived, is *inadequate* science. His

arguments are designed to show, not that psychoanalysis is a pseudoscience, but that the scientific credentials of the theory are deeply flawed by mistakes in logic, epistemological and ontological sleights of hand, and failures to credit disconfirmations when they occurred. Grünbaum (1990) refers to "the sort of cognitive accountability that is featured by the natural sciences" (p. 562) (and which the hermeneuticians seek to bypass by freeing the human sciences of the constraints normally imposed on research in the natural sciences).

The Grünbaum strategy thus adheres to my suggestion that in examining any theory (scientific or pseudoscientific), the articulated aims of the theorizing must be taken seriously into account. What are the articulated aims of Freudian science (for Grünbaum, *all* science)? What is the cognitive accountability featured by the natural sciences? The answer will surely go something like this: A theory in a natural science should have testable consequences, because, before all else, an empirical theory must be confirmed or disconfirmed by observations; a theory should be logically rigorous; a theory should be simple. There are other such "virtues" of theories in the natural sciences (Hempel 1983 calls them "*desiderata*"). It may also be true that not every theory accepted by natural scientists possesses all of the virtues. How many the theory must possess, and how basically involved they are in a given theory, are perhaps variable. In general, however, cognitive accountability as it is envisaged by Grünbaum involves essentially the accessibility of theories to rigorous and detailed criticism based on canons of inductive and deductive logic.

Marks of a Good Hypothesis: The Hempel Strategy

Hempel (Hempel 1983, 87–88) lists *desiderata* of scientific hypotheses, suggesting that they are imprecise constraints on scientific theory choice, "Among them are the following: a theory should yield precise, preferably quantitative, predictions; it should be accurate in the sense that testable consequences derivable from it should be in good agreement with the results of experimental tests; it should be consistent both internally and with currently accepted theories in neighboring fields; it should have broad scope; it should predict phenomena that are novel in the sense of not having been known or taken into account when the theory was formulated; it should be simple; it should be fruitful."

Hempel concedes that these desiderata are vague, and mentions that in earlier writings he had argued that they are arational (not irrational) constraints on theory choice. However, he thinks that his analysis should lead us to conclude that there is something left over that can count as objective traits of competing theories, for example, "the considerable role that precise and rule-governed reasoning does play in the critical appraisal of competing theories, which requires among other things a rigorous derivation of experimental implications and the performance of experimental tests that have to meet appropriate standards. But even the considerations adduced in appraising the satisfaction of the vaguer desiderata are typically perceived, it seems to me, as expressing not just individual taste or preference, but objective, if only roughly specified, traits of competing theories" (ibid., 95).

Thus Hempel agrees with Grünbaum that there are standards of cognitive accountability, and provides an outline of how these standards apply. In doing so, he seems to me to be providing grounds for distinguishing between sciences and those nonsciences appearing on Laudan's list. Apart from the desiderata, Hempel perceives in science some "quite generally acknowledged norms" (ibid., 93) for example, the requirement that scientific reasoning must comply with standards of deductive logic and the interdiction against logical inconsistencies; norms governing methods of measurement and the testing of statistical hypotheses. Such relatively precise norms, together with the desiderata, in effect define scientific appraisal of theories.

Bas van Fraassen (1980, 87–89) chooses to call the Hempel desiderata "virtues" of theories, and argues that the virtues have a pragmatic, not an epistemic, status. Consistency, empirical adequacy, and empirical strength are designated as epistemic traits of scientific theories. But, argues van Fraassen, in accepting a theory, we are not just believing it, we are also committing ourselves to certain wagers: that the research program that generated the theory is worth pursuing, that all relevant phenomena will be captured by the theory, and the like. Epistemic features of a theory (Hempel's "norms") have to do with a theory's relationship to the world; pragmatic virtues of a theory have to do with what we as human knowers want the theory to achieve over and above our belief that it is empirically adequate.

What We Learn from the Four Strategies

A bit of summing up is in order. The strategy of Kant, although it may be the most desirable one, *if achievable*, must wait upon further developments in the sciences before it can be sustained. As a philosophical program it cannot establish what it sets out to conclude: that there is just one set of species-universal conditions on the basis of which we can arrive at reliable knowing, defined by methodologies appropriate to a science regarded as fixed and finished.

As I have suggested, the strategy of Rescher has some value. It invites us to direct attention away from the content of sciences and their competitors and to features of method. In addition, this strategy, as we have seen, replaces a priori demarcation criteria with a posteriori evaluations of success. However, we are now in a position to see that accuracy in prediction and control of the environment are desiderata or virtues of theories, and must be taken into account in the full appraisal of competing theories, but that they cannot by themselves distinguish between sciences and nonsciences. If we leave aside non-epistemic goals scientific or nonscientific theories might serve—for example, improvement of various aspects of human and other animal life—and direct our attention solely to epistemic goals (those whose realization employs, in the sciences, the norms Hempel mentions), and construe these by reference to empirical adequacy, empirical support, and consistency, accuracy in prediction and success in control are clearly not decisive factors in marking off science from non-science.

What the strategies of Grünbaum and Hempel (and of van Fraassen) enjoin is attention to the norms of cognitive appraisal that must be actually operative in determining a theory's adequacy as a set of claims *about the world*. Here, the constraints on theory are the epistemic ones of deductive consistency (and of compliance with norms of nonfallacious ordinary reasoning) and empirical support. Therefore, for purposes of fair comparison of theories thought to be scientific with those thought not to be scientific, we need to emphasize that, other nonepistemic things being equal, the aim of all such theories is *to tell the truth about some features of the world*. What is to be investigated, then, are the credentials a given theory has offered on behalf of its entitlement to be taken seriously as a set of empirical claims. In other words, we need to know what a given theory can

offer in the way of support for its claims to empirical knowledge. In what follows, I will show a way of making a fairly precise distinction between those theories with acceptable credentials, and those lacking them.

A New Strategy of Demarcation

MAN is the Interpreter of Nature, Science the right interpretation

—William Whewell, *Novum Organon Renovatum*

Man interprets nature in many ways, by means of science, poetry, walks in the woods, sailing on dangerous seas, telling age-old stories, seeking spirits in trees and clouds, suns and pestilences. I am interested here only in those interpretations that are cognitive claims, or texts. Texts in this sense are of course also subject to interpretation. What is to be avoided is thinking of nature as a text. Nature is interpreted, but is not a text. Symptoms, microscope slides containing slices of tissue, animal footprints, all require interpretation, but are not texts.⁴ If one thinks of all the kinds of texts there are, the problem of learning how to interpret them arises: Given a text of one sort or another, what are the permissible interpretations? Goodman has of course raised this question in a number of his writings (see Goodman and Elgin 1988, part 2, sec. 3). I think we should begin to take it seriously.

Goodman seems to me correct in thinking that texts are defined syntactically, and that interpretation of meaning is a matter of semantics. Texts are normally strings of inscriptions or vocal sounds. There are exceptions. Some texts are pictorial, and only context will distinguish text from design. What the old grammarians used to call *hypotyposis* or *subjectio sub adspectum*, referring to representational exhibition of meaning with sensuous clarity, encompassed both metaphor and pictorial image, both poetic symbol and visible sign. Thus I will want to include mandalas and international road signs as texts.⁵ We can then generate a long list of different kinds of texts, and we can begin to wonder how many good or right interpretations are possible for each kind. A sentence describing chemical and morphological features of amethyst samples appears to be correctly interpretable in only one way, or in very few permissible ways, whereas

Shakespeare's *Hamlet* may have a number of right interpretations. The Hamlet of Olivier and the Hamlet of Zeffirelli are both acceptable readings of Shakespeare's text.

These considerations lead us to the idea of a *range of permissible interpretations* of a given text, and to the equally suggestive idea that kinds of texts can be distinguished from one another by *the size of that range*. Think then of a scale of texts ranging from those for which only one interpretation is permissible to those for which any number of interpretations are permissible. At one end of that scale will be, perhaps among others, those texts stating claims in formal systems of mathematics and logic, where the texts have only one permissible interpretation, just because no semantics is involved. To have only one permissible interpretation is equivalent, I think, to requiring no interpretation at all. International road signs will be at this extreme end of the scale, as will texts stating standardized rules of a game, say, chess.⁶ At the other extreme end of the scale, what one might call the limit of interpretation, is nonsense or absurdity, that textual gibberish for which any interpretation at all will do. I take it that allowing any interpretation at all is equivalent to having no permissible interpretations. So we might say that gibberish is "beyond interpretation." At other places along the scale will be all of the other kinds of texts, each with a different range of permissible interpretations. Our question now becomes as follows: Where on this scale do we place the various texts of the natural sciences, and where, the texts of the various enterprises thought intuitively to be classed as pseudosciences? Since it is clearly in the interests of all parties to this dispute to have their range of textual interpretations as close to 1 as possible, a collateral question is as follows: How, in the cases of those texts where truth-telling is a central goal, does one limit the range of required hermeneutic insight, the range of permissible interpretations? We will see that the answers to both questions relate also to what we can reasonably mean by support for textual claims of various sorts.

We need to limit ourselves to those texts seeking to tell the truth about the world. They might seek to do other things as well, but for the purposes of the present exercise employment of rhetoric ingredient in a given text will be ignored. We are interested, then, only in those texts which, literally construed, can be either true or false. Notice that I say "*can be*" either true or false. I think it is totally counterproductive to argue for a distinction between sciences and

pseudosciences on the basis of acceptance of the form of scientific realism which teaches that to accept a scientific theory is to believe that it is true. The pseudoscientist is always in a position to counter this view with the correct observation that no texts stating views about the world have ever been conclusively established as true, hence none are ever believable because true. This is the thin edge of the wedge actually employed by many who challenge the epistemic credentials of the sciences: Since no view has ever been conclusively established as true, mine is as good as yours, and on altogether proper epistemic grounds. Because the pseudoscientist is always in a position to exploit the logical contingency of generalizations over items of experience, van Fraassen's antirealism seems to be the best philosophical standpoint from which to argue with the pseudoscientists. Van Fraassen holds that although the language of science must be literally construed, and hence that such language can be either true or false, acceptance of a theory only amounts to believing the theory is empirically adequate, and possesses certain virtues (1980, 10–12).

In thus suggesting that van Fraassen's antirealism is a better foil to pseudoscience than is realism, I am not suggesting that this constitutes a telling reason for accepting van Fraassen's constructive empiricism. What I want to emphasize is that if the difference between science and nonscience is to be made out fairly, those of us who accept the intuitive difference had better approach the problems with a very parsimonious ontology and philosophy of theory acceptance. On one point there seems to be no fundamental disagreement: what Hempel takes to be the core norms of natural science inquiry (and which supply the basis for that cognitive accountability Grünbaum thinks to be definitive of natural science) is entirely compatible with van Fraassen's idea that the central features of science have to do with empirical adequacy, empirical support, and consistency. *At least this much* would be wanted by both realists and antirealists, and constitutes enough for us to go on.

Let us then think of the literal construal of a text as accepting that what it says can be either true or false, and as accepting nothing stronger than that. We need this idea to apply both to scientific texts and to those thought to be pseudoscientific, for claims from both quarters seek at a minimum to be empirically adequate, empirically supported, and consistent. Further, we need to endorse van Fraassen's statement that "to insist on a literal construal of the language

of science is to rule out the construal of a theory as a metaphor or simile, or as intelligible only after it is 'demythologized' or subjected to some other sort of 'translation' that does not preserve logical form" (1980, 11).

It is important to note that the only form of translation allowed of sentences in organic chemistry, astrology, and creation science is translation into another natural language. For purposes of fair comparison, all contenders must state texts in one of the natural languages, which can be only translatable into any of the others.⁷ There is no hidden meaning lying behind or beyond literally construed language in the sense required here. All of this granted, where do the various literally construed texts fit on the scale of texts? What is it that gives the texts of the natural sciences and those of the alleged pseudosciences different places?

My thesis is that, all things considered, the texts of the natural sciences are closer to 1 on the scale of texts with ranges of permissible interpretations than are the texts of the pseudosciences. This is to say that the texts of the natural sciences permit only one or very few interpretations, whereas those of the pseudosciences can be fit to many more legitimate interpretations. To see why this is so it is only necessary to return attention to Hempel's norms and Grünbaum's principles of cognitive accountability and to the idea of support for literally construed texts.

Science and Stability of Meaning

Hempel's desiderata and Grünbaum's implicit principles of cognitive accountability are derived mainly from work in the physical sciences, where the literally construed claims are stated with great care, are sometimes quantitative, and can even be very dangerous in consequences if not understood and applied with precision. Such claims, furthermore, require for their support experimental consequences that require exact measurements of distances and times. The language in experimental physics, chemistry and other natural sciences is thus as decisively referential (fixed in meaning) as it is possible to be, and interpretations of that language, if they are not to be taken to be completely frivolous, approach 1.⁸ All properly formulated scientific discourse has a range of 1, or very near 1. This is so because such discourse must at some point make contact with public, shared, con-

trolled observation, and because of the stability of meaning of its technical terms. This constitutes its support.⁹

We need not go to the highly sophisticated reaches of chemistry and physics, and to their applications in practical affairs, in order to understand the phenomenon of stability of meaning in the sciences.¹⁰ Consider, for example, one of Pasteur's experiments designed to help our understanding of the impossibility of spontaneous generation. Pasteur dissolved a crystalline salt of ammonia, crystalline sugar, and phosphates from the calcination of yeast in pure distilled water, and then sowed into the liquid a few spores of *Penicillium* or some mold.¹¹ The spores germinated within two or three days, filling the surface of the liquid with flakes of mycelium, where they produced healthy vegetation. Pasteur's report continues, "The precaution of employing an acid salt of ammonia hinders the development of Infusoria, which would soon stop the progress of the little plant through absorbing the oxygen of the air, without which the mould cannot thrive. All the carbon of the plant is obtained from the sugar, which is slowly used up, all its nitrogen from the ammonia, its mineral matters from the phosphates. In this matter of the assimilation of nitrogen and phosphates there is thus a complete analogy between ferments, moulds, and plants of complicated organization" (1959, 228).

Pasteur next reports that if any of the dissolved principles are omitted, the growth of the fungi is arrested. The conclusion is that fungi do not obtain carbon in the same way as do phanerogamic plants, "They do not decompose carbonic acid; they do not set free oxygen. The absorption of oxygen and the setting free of carbonic acid; are on the contrary the necessary and permanent activities of their life. These facts give us precise ideas on the mode of nutrition of fungi . . ." (ibid.). This is precise enough to enable brewers, wine and vinegar makers and dairy farmers to introduce controls on their procedures based on scientific knowledge. But that is only part of the point. The larger part is that it is not possible to understand the elements of this simple inductive experiment on the basis of several interpretations. Too much precision and exact knowledge exists at every point.

What is being investigated depends upon well-understood aspects of chemistry and biology: the nature of distilled water, of calcinated yeast, of sugar, of tartrate of ammonia, the required quantities of these chemicals, the classification of plants with organs of reproduction as phanerogamic, the behavior of Infusoria, and the kind of nu-

trition involved in such plants. All such matters are expressed in language whose meaning is fixed; there is no room for gratuitous interpretation. There is room for disagreement, but that is another matter. Notice also that I have deliberately employed a translation of Pasteur's paper, first published in *Annales de Chimie et de Physique* in 1862. The translation does not "demythologize," it simply transfers exact language from one natural language into another. For purposes of exact science, all natural languages are one language, construed literally.

One final point about the experiment is that what Pasteur wrote can be *misunderstood*. Furthermore, Pasteur could have set out to deliberately *deceive* us. I see no insurmountable problems here. With respect to texts whose range of interpretations approaches 1, misunderstanding can be corrected, perhaps not easily, but surely in time. A misunderstanding is, in the end, a wrong interpretation, and wrong interpretations are not permissible interpretations. Any text can set out to deceive us, even in the formal sciences. Here again, the detection of deception, although in some cases it may be very difficult to realize, is always in principle realizable for those texts whose range of interpretation approaches 1. The possibility of fraud does arise in the work of natural scientists; it can never arise in the work of poets.¹²

What about astrology, creationism, and other alleged pseudosciences, can their texts be literally construed and empirically supported, establishing that the ranges of interpretation are close to 1?

Astrology and Its Support

Astrological texts are charts based on calculations of positions of heavenly bodies at various times, which relate such positions causally to events in people's lives. These results are often stated in sentences that assess trends. One can find such sentences in most newspapers published in the world today. Some condemn astrology because it does not accurately predict precisely events that will determine the course of someone's life. But careful astrologers make more cautious claims, "Astrologers are not magicians. They do not deal in specific events and must always take care that no client is injured through taking their advice too literally" (Parker and Parker 1979, 154); and:

Serious astrologers do not "predict" events; they assess trends. When mentioning potential hazards or beneficial periods, the astrologer must always

remember to put these matters carefully to his client. He should never state: "You will have an accident on the twenty-first of the month," but he can say that: "On the twenty-first of the month you are likely to be somewhat prone to risks, so take additional care when handling hot dishes or sharp knives." Of course, in this way astrologers do lay themselves open to the criticism of using generalizations. It must be made clear to a client that progressions¹³ are rather like weather forecasts, and should be used in much the same way. (Ibid.)

A host of questions arise. If astrological results cannot predict, how do we determine the success of astrological claims? If the advice given by astrologers is general, how can it avoid vagueness and loss of clarity? If the advice does not speak to specific events, are we not invited to interpret it in very many ways—that is, to what I take it to mean for my life, to what you take it to mean for yours, and so on? Are not publishing the results of theoretical and experimental work and advising clients two entirely *and importantly* different matters? Does the casting of charts have any valid relationship to the advice given a client? What, after all, is the relationship between the calculations and the general advice? That advice could, for all we know, eventuate from many other kinds of sources. Is it really possible, given these questions, to construe astrological advice literally?

Consider the following item from *The Globe and Mail* (1991):

Having a Hussein Week

Saddam Hussein's birthday is April 28. Here is some of Jeane Dixon's advice to Taurus last week:

Jan. 15 (UN's Tuesday deadline): Indulging your whims could prove costly today. Think about the image you want to present.

Jan. 16 (U.S. planes hit Baghdad, including its communications centre): Spending time alone will help you extricate yourself from a difficult situation.

Jan. 17 (Iraqi missiles strike Israel): Do what must be done at home. You can easily catch up with neglected tasks.

Jan. 18 (Jordanian parliament brands United States the Great Satan): A close relationship intensifies; two minds think as one. You sense what will happen next. Now is the time to make important connections.

I suppose *The Globe and Mail* thought this piece to be humorous; if each bit of advice is construed literally, then it would seem that Saddam has failed to heed the warnings. However, even if construed literally, each piece of advice is permissibly subject to different interpretations since the advice is general and does not speak to Saddam or any other individual. (Another question is if the advice is this

general, over what instances does it generalize? the life charts of everyone born under the sign of Taurus? the character traits mythologically associated with each sign of the Zodiac? The charts and the traits are themselves open to endless interpretation.) Saddam need not have thought that failure to meet the UN deadline was whimsical; he might have wanted to project exactly the image of one who acts in defiance of the deadline. Perhaps he thought that remaining in his bunker was exactly the way to prevent his own destruction. Perhaps he thought that sending missiles to Israel was exactly the way to look after the home front.

How, then, does the astrologer attempt to limit the range of permissible interpretations of the claims involved in the advice? One way is to insist that the kind of advice given in Jeane Dixon's newspaper columns is meant only as a very loose guide. To make the advice more precise, the astrologer must look at an individual's birth chart. We have seen that even this more personally tailored advice must remain nonspecific, so it seems that no gain in restriction of interpretations is forthcoming from individualized treatment. At another level, the astrologer seeks support, not for the generalized advice, but for the entire speculative enterprise of astrology, at which point the Rescher strategy can be invoked to show that no accuracy of prediction or success in control is forthcoming from astrology. While this may be true, it is important, I think, to look briefly at what at least some astrologers present as support for their activity.

In the absence of an account of how positions of heavenly bodies can be causally related to either specific or general advice, astrologers look for support for their speculations elsewhere. A common move amounts to an appeal to authority: Aquinas's acceptance of astrology (so long as necromancy is not included) as a science, and Kepler's casting of astrological charts. Parker and Parker (1979) appeals to Jung's interest in astrology as "the first important stage in the slow revival of scientific interest in the topic [the causal relationship between planetary movements and events in human life] which we are witnessing today" (p. 59). They continue, "What is important . . . is the fact that a man recognized throughout the world as one of the greatest and most provocative thinkers of the century should have considered the data of astrology sufficiently interesting in principle not only to experiment with it, but also to include it as an integral part of his own cosmological theory" (ibid.).

If we drop the vacuous appeal to authority, what is left to support astrology and provide warrant for limiting interpretations of its claims? I think the obvious answer is "nothing." On the question of causal influence of planetary motions on human action, some astrologers point out that various forms of action at a distance are allowed by scientists, that the biological clock is an influence akin to astrological causality, and the like (ibid., 50–57). Such appeals simply complicate the speculations, lending unsubstantiated additional claims to the already heavy baggage of interpretation involved in astrology. As if this were not enough, astrologers often seek to find support for their speculations by appealing to the long history of widespread interest in their "science," and to such seemingly unrelated and highly symbolic matters as the pyramids, Stonehenge as an ancient cosmic computer, the celestial bestiary, the fact that traditionally signs of the Zodiac have been related to various parts of the human body, the close connections between mythology and astrology, even to Culpeper's herbal remedies! (For details, see Parker and Parker.)

It thus appears that, in order to take the texts of astrology to be well supported, we are driven further and further away from considerations of empirical adequacy, and closer and closer to the need for acceptance of a complete, and thoroughly questionable, metaphysical system. That system is built up of the kinds of appeals just listed, all of which boil down to being appeals based on tradition, history, community of interests, authority, collection of apparently meaningfully connected symbols (perhaps even Jung's archetypes of the collective unconscious). The consequence is that astrology cannot have its texts restricted to 1 or a few interpretations. Indeed, astrology appears as the pseudoscience *par excellence* because it is the "science" of interpretation *par excellence*. Since the range of permissible hermeneutic insight required to understand astrological claims is so large, the kind of support it seeks cannot place it close to the sciences, whose texts have a narrow range of permissible interpretations that approaches 1.

Creationism and Its Support

The same is true of creationism, although for very different reasons. Astrology simply requires acceptance of too much nonsense in order to be considered a science. The difficulty with creationism, as

an alternative to scientific biology, especially the theory of evolution, is one of a multiplicity of permissible *translations*. What I will now put forward is not new, but I hope the new context I am endeavoring to supply will justify saying it once more. In brief, creationism accepts the account of the creation of life forms found in the book of Genesis of the Bible. God created everything in the heavens and on earth in six days, starting at a time. Creationists disagree about the year in which God began his work (as well they might; it is difficult to imagine how years could be counted *before* God created the heavens!). Because creationists believe this account is a true theory, they of course want it to be taught in biology classes, as a theory in competition with the theory of evolution.

If the book of Genesis is literally construed, how are we to understand the support for its claims? At the end of the first day of creation, there were God, and light and darkness. How long was that day? We are not told how God measured time. We cannot proceed with the attempt to confirm the claim that on the first day God made light and darkness. Furthermore, God's spirit was moving on the waters when he created light and darkness. Are the waters created or not? Presumably not. Again, an incoherent claim. Obviously, literal construal of the book of Genesis, or of any book of the Bible, is impossible. We could at this point simply conclude that the text of the Bible is literary, not literal, and hence is of course subject to differing interpretations. But this would not be fair to the creationists, who are also fundamentalists, and who want to teach Genesis alongside Darwin because they think that what Genesis says is not just to be literally construed, but is literally true.

The problem for the creationist lies elsewhere. To which book of Genesis do we appeal? the original Hebrew version? Luther's translation? Numerous versions of the Bible are available: the 1611 King James version, the 1979–1982 new King James version, the Revised Standard version, the New Revised Standard version (noted for its attribution of gender-free language to God), the New English Bible version, the Revised English Bible (in an ecumenical effort, "Let us make man in our image" becomes "Let us make human beings in our image"—creating a hermaphroditic deity, now we are getting somewhere!), the New International version (the best-selling version in the United States), the New Jewish version, the new American Bible (the most widely recognized Roman Catholic version) and more. We have

seen that in the case of scientific languages literally construed, translation from one language into another is relatively unproblematic because of the stability of meaning of technical terms and the absence of metaphor. This does not make scientific texts fun to read, but surely makes them easy to translate.

So we see that the problem for the creationist is twofold, if the desired limitation of the range of permissible interpretations is to be accomplished. First, it must be shown that the text of Genesis can be literally construed, so that it is at least in principle capable of empirical adequacy. As suggested, a literal construal of the text of Genesis turns its sentences into either falsehoods or logical incoherencies. Second, it must be shown either that one version of the Bible is the only correct one, or that all versions are intertranslatable without loss of logical form or of literal reference. How can either alternative be rationally sustained? It cannot.

There is a final move open to the creationist. For it is possible to argue that there are sciences and sciences, and that creationism is a human, not a natural, science. The support for construing any version of the Bible literally comes not from the stability of reference of technical terms and from the absence of metaphor, but from facts having to do with the actions of human beings—again, from history, community, tradition, authority, and shared faith. Restriction of the range of hermeneutic insight required for interpreting biblical texts comes not from the collection of more confirming instances (corroboration), but from the conversion of more witnesses providing sympathetic testimony (vindication). If one seeks to justify the scientific status of creationism along this line, it would appear that creationism, like the human sciences generally, needs to provide adequate justification for the new kinds of evidential appeal. It cannot make good its claims to equal treatment on natural scientific grounds, but must appeal to what until further notice must be regarded as nonscientific forms of evidence.

I hope to have shown that at least two systems of speculation thought generally to be pseudosciences are indeed pseudosciences, and that the reason for this is that their ranges of permissible interpretations are either large, or can only be restricted by appealing to kinds of support which are scientifically unavailing. On my scale of ranges of permissible interpretations, where do the other pseudosciences Laudan lists find themselves? None of them have ranges that

approach 1. Some—Rosicrucians, the-world-is-about-to-enders, probably charioteers of the gods—have ranges at the other limit; their texts are gibberish. Some may even fall off the scale and approach insanity: flat earthers, circle squarers, *perpetuum mobile* builders. Still others, literally construed, can be either confirmed or disconfirmed. Nessie might one day be found and captured, Big Foot might one day be bagged. To date, alleged sightings of both creatures seem doubtful. So-called anomalous phenomena like the two monsters and UFOs may be worthy of scientific investigation, if we think society can bear the cost. Primal screamers have only themselves to blame.

NOTES

I mention in the text some of those who have helped to shape my thinking about science. The contribution of Adolf Grünbaum is simply incomparable, and is here acknowledged with warm thanks. I have also learned about the science-pseudoscience divide from my conversations and correspondence with Andrew Lugg (see Lugg 1987, and forthcoming).

1. The employment of theories of meaning to help in drawing the line between science and nonscience now seems a lost cause. The effort was to show that the formal and empirical sciences contain sentences that are cognitively significant, whereas the sentences of nonsciences are in some respect nonsense. However, "sense" is a notoriously ambiguous word. The attempts at demarcation along such lines (whose ancestor is the Kantian strategy to be discussed in the next section) could never answer the rejoinder of Nietzsche:

It is no different with the faith with which so many materialistic scientists rest content nowadays, the faith in a world that is supposed to have its equivalent and its measure in human thought and human variations—a 'world of truth' that can be mastered completely and forever with the aid of our square little reason. What? Do we really want to permit existence to be degraded for us like this—reduced to a mere exercise for a calculator and an indoor diversion for mathematicians? Above all, one should not wish to divest existence of its *rich ambiguity* (*seines vieldeutigen Characters*): that is a dictate of good taste, gentlemen, the taste of reverence for everything that lies beyond your horizon. That the only justifiable interpretation of the world should be one in which you are justified because one can continue to work and to research scientifically in *your* sense (you really mean, mechanistically?)—an interpretation that permits counting, calculating, weighing, seeing, and touching, and nothing more—that is a crudity and naiveté, assuming that it is not a mental illness, an idiocy.

Would it not be rather probable that, conversely, precisely the most superficial and external aspect of existence—what is most apparent, its skin and sensualization—would be grasped first—and might even be the only thing that allowed itself to be grasped? A 'scientific' interpretation of the world, as you understand it, might therefore still be one of the *most stupid* of all possible interpretations of the world, meaning that it would be one of the poorest in meaning. (Nietzsche 1974, sec. 373)

2. Over the centuries, the list has not changed that much. Here is one from Reginald Scot (1665), "the Knavery of Juglers, Conjurers, Charmers, Soothsay-

ers, Figure-Casters [astrologers], Dreamers, Alchymists and Philterers [sellers of erotic love potions] . . ." (p. i).

3. In the *Opus postumum* Kant altered this assessment of the cognitive status of chemistry, and tried to give a justification of scientific status of the work of Lavoisier.

4. Anything that can be counted as a system of symbols subject to interpretation is a text. This list will be very long, but will surely contain the following: proofs of theorems, reports of results of observation or experiment, narratives (histories, news releases, novels, travelogues, stories told in psychoanalytic clinical situations), police reports, lawyers' arguments, judges' decisions, poems, clinical observations, mandalas, cryptograms, messages from another world (as in seances, contact with extraterrestrials), pictograms (cave and rock paintings), works of visual art, plays, operas, musical scores, astrological charts and advice, records of divine revelations, ceremonies (Christmas, the Festival of Lights), tribal lore transmitted verbally, the writings of philosophers, metal coins, political speeches, sermons, academic lectures, street and highway signs.

5. Although again context is important. In the neighborhood of my birthplace in Lancaster County, Pennsylvania many Amish towns have charming names. The signs read "Paradise," "Intercourse," "Blue Ball," "Bird-in-Hand." Undergraduates at the University of Pennsylvania in Philadelphia are fond of stealing these signs for use as decorations in their dormitory rooms. What were signs thus become trophies, and are not to be taken literally.

6. I am aware that one can be deceived by some road signs, especially those marking miles or kilometers. One summer, a student of mine worked for the Department of Highways; he took delight in placing mileage markers at random, rather than at precisely determined intervals. Max Black used to say that "it isn't a language unless you can lie in it." It behooves all of us to face all texts with a measure of skepticism. Even mathematical texts can be misprinted. No important epistemological point turns on these admissions.

7. I must admit that sentences in scientific reports are more readily translatable into other natural languages partly because much of the terminology is standardized, and the mathematics employed is normally semantically transparent. Such ease in moving from one language to another in science is a stroke of good fortune not also available to the pseudosciences. Translations of texts in astrology, creationism and the like might preserve logical form, but they inevitably introduce ambiguities and nuances not present in the source language. To which version of the Bible should creationists direct us? I will return to this problem later.

8. A pragmatic consideration here gives weight to Rescher's emphasis upon accurate prediction: If some experiments are not carried out with the greatest attention to all details, the experimenters are at peril. What is lost if an astrological prediction turns out to be false? Perhaps the client may murder the astrologer. That is quite different from the death of a careless experimenter.

9. It might be argued that stability of meaning of technical terms is assured by the *community* of scientists, and that similar communities of astrologers and creationists and others create their own assured stability for the meaning of their technical terms. I would not deny that fraternal conditions play some role in fix-

ing meaning. What one needs to consider here is the nature of the conditions of entry into different fraternal groups. Rigorous education and satisfaction of high intellectual standards form part of those conditions for scientists. No such training and accomplishment are required of young astrologers; all that is needed for entry into the fundamentalist community is an unwavering faith in the literal correctness of biblical claims. Indeed, further study of such conditions of entry into intellectual and quasi-intellectual communities might highlight still more ways in which the sciences and the pseudosciences differ fundamentally.

10. I am emphasizing features of the discourse of experimental and observational natural science, because I am dealing only with those texts that seek to tell truths about the world. Such an emphasis, however, should not disguise the fact that within *theoretical* natural science, interpretations of theoretical constructs are themselves part of the internal dialectic of physical science. Thus, for example, the concept of gravity in classical Newtonian physics can be interpreted as action at a distance, or as involving electromagnetic fields. So far as the mathematics and the empirical support of the classical theory are concerned, these different interpretations are equivalent, and play only heuristic roles. Like Kant's ideas of reason, they have no empirical truth-telling content. I am grateful to my colleague Robert DiSalle for calling these matters to my attention.

11. The liquid consisted of 100 parts pure water, 10 parts sugar, 0.2 to 0.5 of ammonia, and 0.1 of ash of yeast.

12. However, notice David Hume's amazing attempt to rescue some literal sense for poetry, "Poets themselves, tho' liars by profession, always endeavor to give an air of truth to their fictions; and where that is totally neglected, their performances, however ingenious, will never be able to afford much pleasure" (1951, 121).

13. Birth charts are "progressed" by, for example, taking the positions of the planets on the first day after birth to relate to the conditions in the first year after birth, the positions in the fifth day after birth, to the fifth year after birth, and so on for all days after the birth date. These progressions are employed to assess trends in the life of a person.

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