DEMARCATING SCIENCE FROM NON-SCIENCE

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1 INTRODUCTION

Every field of inquiry deals with some subject matter: it studies something rather than nothing or everything. Thus it should be able to tell, at least roughly, what sort of objects it is concerned with and how its objects of study differ from those studied by other disciplines. If a discipline were unable to offer a characterization of its subject matter, we would be entitled to suspect that its representatives do not really know what they are talking about. Evidently, what holds for all fields of inquiry also holds for a particular discipline such as the philosophy of science. Therefore, it belongs to the job description, so to speak, of the philosopher of science to tell us what that "thing" called science is.

Yet whereas everyone seems to know intuitively which fields of knowledge are scientific (such as physics and biology) and which are not (such as astrology and palmistry), it has proved difficult to come up with a satisfactory demarcation criterion. Indeed, many of the demarcation criteria proposed by philosophers of science have proved to be unsatisfactory, for being either too narrow or too wide. In addition, due to the historical and sociological studies of science, many contemporary authors believe that there simply is, or even can be, no single criterion or set of criteria allowing for a clear-cut characterization of scientific vis-à-vis non-scientific areas of human inquiry. In particular, most contemporary philosophers doubt that there is a set of necessary and sufficient conditions demarcating science from non-science. It comes as no surprise therefore that, in a survey conducted with 176 members of the Philosophy of Science Association in the US, about 89% of the respondents denied that any universal demarcation criteria have been found [Alters, 1997].

Does this vindicate relativist views like Feyerabend's [1975] well-known anything-goes epistemology? Must we give up the attempt to descriptively partition the landscape of human cognition into scientific and nonscientific areas, as well as to tell genuine science from bogus science (pseudo-science)? Is, then, the philosophy of science unable to address the normative problem of why some form of human inquiry arrives at (approximately) true knowledge, whereas some other, purporting to be equally scientific, must be judged to produce only illusory knowledge?

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This situation illustrates the problems that a reasonably comprehensive analysis of the demarcation problem should address. To this end, let us restate three questions formulated by Thagard [1988, p. 157], which will guide the following analysis:

1. Why is it important to demarcate science and from what should it be distinguished?

2. What is the logical form of a demarcation criterion?

3. What are the units that are marked as scientific or nonscientific, in particular as pseudoscientific?

2 WHY DEMARCATION?

Let us begin with the second conjunct of the first question. Evidently, demarcating science means to demarcate it from nonscience. Yet, in so doing, how widely do we have to conceive of nonscience? In the broad sense, simply anything that is not science is nonscience: driving, swimming, cooking, dancing, or having sex are nonscientific activities. Now, the philosopher of science is not particularly interested in demarcating science from nonscientific activities such as these, although they involve learning and hence some cognition, leading in particular to procedural knowledge. Naturally, he will be interested first of all in cognitive activities and practices leading to propositional knowledge, i.e., explicit and clear knowledge that can be either true or false to some extent. Thus, we are primarily interested in nonscience in the sense of *nonscientific cognitive fields* involving hypotheses and systems of such (i.e., theories) as well as the procedures by means of which these are proposed, tested, and evaluated. Consequently, distinguishing science from nonscience in this narrower sense is not restricted to the classical science/metaphysics demarcation attempted by the neopositivists and Popper, but extends to all nonscientific epistemic fields.

The first reason why we should strive for a demarcation of science is theoretical: it is the simple fact stated in the beginning that every field of knowledge should be able to tell roughly what it is about, what its objects of study are. Unless the philosophy of science simply is nothing but epistemology in general, it should be able to distinguish scientific from nonscientific forms of cognition. Note that such a basic distinction between science and nonscience is not pejorative: it does not imply that nonscientific forms of cognition and knowledge are necessarily bad or inferior. Nobody doubts the legitimacy and value of the arts and humanities, for example.

The second reason, or rather set of reasons, why we ought to demarcate science from nonscience concerns in particular the normative aspect of distinguishing science from pseudoscience. Moreover, it is practical rather than theoretical, comprising aspects of mental and physical health, as well as culture and politics. Should we entrust our own as well as other peoples' health and even lives to diagnostic or therapeutic methods which have no proven effect? Should public health insurance cover magical cures? Should we even consider the possibility that clairvoyants search for missing children? Should we have dowsers search for people buried by an avalanche? Should we make sure that tax payers' money be spent only on funding scientific rather than pseudoscientific research? Should we demand that people living in a modern democratic society base their political decisions on scientific knowledge rather than superstition? Examples such as these show that the distinction of science and pseudoscience is vital not just to our physical, but also to our cultural and political life.

This aspect leads us to a third reason: the need of science education to teach what science is and how it works. To this end, the science educator needs input from the philosopher of science as to the nature of science [Alters 1997; Eflin *et al.*, 1999]. The science educator cannot just tell her students that nobody knows what science is, but that they are nonetheless supposed to learn science rather than pseudoscience. For all these reasons, we ought not to give up too readily when facing difficulties with the demarcation of science from nonscience, and in particular from bogus science.

3 HOW DEMARCATION?

In the history of the philosophy of science various demarcation criteria have been proposed (see Laudan [1983]). Let us briefly recapitulate some of the classical attempts at demarcation, starting with logical positivism. In tune with their linguistic focus the neopositivists' foremost goal was to distinguish sense from nonsense. A sentence was deemed to be (semantically) meaningful if, and only if, it was verifiable; otherwise, it was nonsense. Whereas, according to neopositivism, the statements of science are verifiable and thus meaningful, those of metaphysics and all other kinds of bad philosophy were not; they were just nonsense (see, e.g., [Wittgenstein, 1921; Carnap, 1936/37; Ayer, 1946]). To verify a sentence means to find out whether it is true, which requires that it be tested empirically. The central tenet, then, was that testability is a necessary condition of (semantic) meaning: meaning $\rightarrow$ testability.

One problem with this view is that it has things the wrong way. Indeed, to test a statement empirically, must we not know what it means, i.e., what it says, in the first place? To devise a test for a statement such as "unemployment increases crime", we must already know what that sentence means. Only thus can we handle the variables involved. Hence, meaning is in fact a necessary condition of testability rather than the other way around: testability $\rightarrow$ meaning [Mahner and Bunge, 1997]. As a consequence, nonscientific discourse can be semantically meaningful, although it may not be testable empirically. If a Christian tells us "Jesus walked on water", we know quite well what that means, although we cannot test this statement, which we may moreover regard as purely mythical. We may reject it for many reasons, but not for being nonsensical.

A logical and methodological objection against the verifiability thesis is the fact that it is rarely possible to verify a statement in the strict sense, i.e., to show that it is true. For example, we may easily verify or falsify a spatiotemporally restricted existential statement, such as “There is a pink elephant in my office”. But if we are faced with general statements such as “For all X: if A then B”, observing B does confirm A, but only inductively, never conclusively. The most cherished scientific statements, then, namely law statements, are not strictly verifiable. Hence the strong concept of conclusive verification was soon replaced by the weaker notion of confirmation [Carnap, 1936/37]. Nonetheless, having always been a critic of induction, Popper [1934/1959] suggested giving up the verifiability condition in favor of a falsifiability principle. Indeed, according to the modus tollens rule, observing not-B entails not-A. Thus, logically, falsification is conclusive, whereas verification is not. This logical asymmetry is the basis for Popper’s famous demarcation criterion of falsifiability [Popper, 1963].

Critics have soon pointed out that not all scientific statements are universal: there are also unrestricted existential statements, such as “There are positrons” [Kneale, 1974; Bunge, 1983b]. These can be verified, e.g., in this case by coming up with at least one specimen of a positron; but they cannot be falsified, because we cannot search the entire universe to conclusively show that it does not contain even a single positron. Other critics have shown that scientists do not give up a theory as being unscientific just because there are some falsifying data, unless there is a better theory at hand, concluding that Popper’s criterion does not match scientific practice [Lakatos, 1973; 1974].

In the light of such critique, Popper has later clarified his position, emphasizing that it is not practical falsifiability which is his concern, but instead logical falsifiability [Popper, 1994]. That is, a statement is *logically falsifiable* if there is at least one *conceivable* observation statement contradicting it. In other words, a statement is scientific only if it is not consistent with every possible state of affairs. In proposing falsifiability as a demarcation criterion, Popper had in mind examples such as Freudian psychoanalysis. According to psychoanalysis, the Oedipus complex is either manifest or repressed, so no possible observable state of affairs can count against it: it is unfalsifiable as a matter of principle. Again, critics were quick to point out that this does not hold for all of psychoanalysis (e.g., [Grünbaum, 1984]): while some claims are indeed unfalsifiable, many others are falsifiable and others have actually been falsified. The same holds for many other pseudosciences, such as astrology and creationism. For example, the central tenet of creationism, that a supernatural being created the world, is indeed unfalsifiable: it is compatible with every possible observation statement, for any state of affairs can be seen as exactly what the creator chose to do. Other and more specific creationist claims, however, such as that the earth is only 6000 years old, are falsifiable and falsified. Thus, the falsifiability criterion may be useful to weed out some claims as pseudoscientific, but it accepts too many falsifiable and falsified statements as scientific, although there are good reasons to regard them as pseudoscientific. For all these reasons, falsifiability has been almost unanimously rejected as *the* demarcation criterion (e.g., [Kuhn, 1970; Kitcher, 1982; Bunge, 1983b; Laudan, 1983; Siitonen, 1984; Lugg, 1987; Thagard, 1978; 1988; Rothbart, 1990; Derksen, 1993; Resnik, 2000]).

Being first of all a logical condition, falsifiability is an ahistorical criterion. The historical turn in the philosophy of science has suggested taking into account both the development of theories and their relation to rival theories. In so doing, it shifted the focus of demarcation from individual statements or hypotheses to entire theories. The classic approach is certainly Lakatos's [1970] notion of a research program. A research program is a historical sequence of theories, where each subsequent theory results form a semantical reinterpretation of its predecessor, or from adding auxiliary assumptions or other modifications. A research program is called *theoretically progressive* if each new theory has a larger content, e.g., by having greater explanatory or predictive power, than its predecessor. It is also *empirically progressive* if it is confirmed, i.e., if it actually leads to the discovery of some new fact. A research program is then called *progressive* if it is both theoretically and empirically progressive; otherwise it is called *degenerative*. Finally, Lakatos [1970] takes a research program to be *scientific* if it is at least theoretically progressive; otherwise it is *pseudoscientific*.

Critics like Laudan [1983] have objected that progress might as well occur in nonscientific fields, such as philosophy, and that some branches of science did not progress much during some periods in their history. And what if some science actually had discovered and explained everything in its domain that is to be discovered and that needs explanation; in other words, what if there were such a thing as "the end of science" as envisioned as the ultimate goal of science by Einstein [Holton, 1993; Haack, 2003], and as was more recently speculated on by Horgan [1995]? Would such a theory or discipline be no longer scientific just because it does not or rather cannot progress any more? Similarly, there is the opposite problem of radically new theories: can they be scientific without being part of an existing research program? Consequently, however useful the criterion of growth and progressiveness will be in many cases, it too cannot provide *the* decisive demarcation criterion.

Kuhn [1970] has suggested that we focus not so much on the testability of theories as on their problem-solving capacity. He illustrates his point in the case of astrology. Many predictions of astrology are testable and have failed, but astrology is not therefore a science as Popper's falsifiability criterion would allow for. According to Kuhn, this is because astrology has no puzzles to solve: even its failed predictions did not entice the astrological community to engage in problem-solving activity. At most, astrology has rules to apply, for it is essentially a craft — or rather a pseudotechnology. But if applying rules is simply a characteristic of technology rather than science, what distinguishes a scientific technique from a nonscientific one? Finally, although earlier authors like David Hilbert have already dealt with problems and pointed out that a wealth of problems is an indicator of a good science, what about the end-of-science scenario mentioned above? Would even a true theory become nonscientific if all problems surrounding it were solved?

Scientists would hardly think so.

Nevertheless, other authors too suggested focusing on problems, in particular on the permissible rules of asking questions and stating problems [Siitonen, 1984]. Obviously, the solution of a problem should enrich our knowledge and contribute to stating and solving further problems. Moreover, we may learn something about the current state of a field of inquiry by asking questions such as “What are the problems?”, “How are these problems formulated?”, and “Which efforts have been and can be used to solve these problems?” [Siitonen l.c., p. 347]. But again, all these considerations are far from providing a new demarcation criterion.

In a book on creationism, Kitcher [1982] focuses on three characteristics of science. First, the auxiliary hypotheses involved in the testing of any scientific theory are *independently testable* themselves, i.e., independently of the theory it is supposed to protect or of the particular case for which they were introduced. Second, scientific practices are *unified* wholes, not patchworks of isolated and opportunistic methods: they apply a small number of problem-solving strategies (if preferred, exemplars) to a wide range of cases and problems. Third, good scientific theories are *fertile* in the sense that they open up new areas of research. Thereby, one of the sources of fecundity is the incompleteness of scientific theories, so that some problems remain unresolved. Incompleteness and some unresolved problems are therefore not shortcomings of scientific theories but instead sources of progress.

Thagard [1988] lists five features characterizing science. As a method of inference, scientists use “correlation thinking”; that is, by means of various statistical procedures they infer causation, if any, from correlation (rather than from mere resemblance). They seek empirical confirmation and disconfirmation, and evaluate theories in relation to alternative theories, whereby these theories are consilient and simple. Finally, science progresses over time, i.e., it develops new theories explaining new facts. Thagard does not regard these features as both necessary and sufficient, but only suggests that they belong to the conceptual profile of science.

Rothbart [1990] attempts to formulate a metacriterion (or adequacy condition) for any demarcation criterion. This condition is the testworthiness of a hypothesis or theory, i.e., its plausibility to be selected for experimentation in the first place. To this end a hypothesis must fulfill certain eligibility requirements prior to testing. If it does not fulfill even one of these requirements, a hypothesis is untestworthy and hence unscientific. Actual demarcation is then obtained by specifying such eligibility requirements. One such requirement is that the proposed theory must account for all the facts that its rival background theory explains; another is that it must yield test implications that are inconsistent with those of its rival theory.

Vollmer [1993] distinguishes necessary and merely desirable features of a good scientific theory. The necessary conditions are noncircularity, internal consistency (noncontradiction), external consistency (compatibility with the bulk of well-confirmed knowledge), explanatory power, testability, and test success (confirmation). Among the desirable features are predictability and reproducibility, as well as fecundity and simplicity (parsimony). Predictability and reproducibility are not among the necessary conditions, for otherwise historical sciences, such as evolutionary biology, geology, cosmology, and of course human history, would not count as scientific because both their predictability and reproducibility are limited. However, even in such overall historical fields, not all events are unique, but repeatable at least in the sense that events *of the same kind* may reoccur on a more or less regular basis. Consequently, if the very nature of some event is of the repeatable kind, irreproducibility may still indicate that something is wrong with the given field's claim of being a science.

Reisch [1998] attempts to resuscitate the unity of science ideal of logical positivism, though not in its reductionist form. He suggests identifying the various theoretical and methodological interconnections of the sciences, which should result in what he calls a *network unification* of science and hence a *network demarcation*. An epistemic field that cannot be incorporated into the existing network of the established sciences without destroying it should be rejected as pseudoscientific. Again, such network demarcation does not draw a fixed boundary around the sciences, but allows for changes in what belongs to that network and what not. Finally, the neopositivist aspect of Reisch's approach consists in the claim that the specification of the interconnections among scientific fields is essentially a *scientific* form of demarcation rather than a philosophical one.

The result of the preceding overview is clear: neither is there a single criterion such as falsifiability to demarcate science from nonscience, nor is there a generally accepted set of necessary and sufficient criteria to do this job. However, *pace* Laudan [1983], this does not imply that no demarcation is possible. To see why, it will be useful to make a brief foray into the philosophy of biology, which faces a similar problem.

In the philosophy of biological systematics there has been a long debate concerning the ontological status and definition of biological species (see, e.g., [Mahner and Bunge, 1997]). The classical, essentialist view regards species as natural kinds defined by a set of necessary and sufficient properties. Against this view the anti-essentialists have argued that, due to the high genetic and morphological variety of organisms, there simply is no set of necessary and sufficient characters possessed by all and only the organisms of a given species, let alone higher taxonomic units (see, e.g., [Dupré, 1993]). Nevertheless, the organisms of a given species usually are both similar among each other and distinct from organisms belonging to different species.

The radical answer to this problem says that species should therefore not be conceived of as kinds at all, but rather as concrete supraorganismic individuals. Now, science too can be viewed as a concrete system, namely as a research community. In this case it is relatively easy to determine who is part of this community and who is not. But of course, science is more than that: in contrast to the sociologist of science, the philosopher of science is more interested in science as a collection of reliable knowledge items produced by following certain methodological standards. To this end, science is better regarded as a special *kind* of knowledge production, which can be demarcated from other kinds of knowledge acquisition. Now, if traditional essentialism with respect to kinds cannot be upheld at least in biology, we might try what in the philosophy of systematics could be called *moderate* species essentialism. This is the idea that biological species *can* be viewed as natural kinds, if only in a weaker sense defined by a variable *cluster* of features instead of a strict set of necessary and sufficient properties (see, e.g., [Boyd, 1999; Wilson, 1999]). Thus, whereas no single property need be present in all the members of the given species, there are always "enough" properties making these organisms belong to the given kind. (Forerunners of moderate essentialism are Wittgenstein's family resemblance concept, which was suggested for demarcation purposes by Dupré [1993], and Beckner's [1959] polythetic species definitions.)

Despite the unsolved problems concerning the formalization of such disjunctive characterizations [Mahner and Bunge, 1997], applying this approach to the demarcation of science might allow us to define science through a variable cluster of properties too, rather than through a set of necessary and sufficient conditions. For example, if we came up with ten conditions of scientificity (all of equal weight), we might require that an epistemic field fulfill at a minimum seven out of these ten conditions in order to be regarded as scientific, but it would not matter which of these ten conditions are actually met. According to the formula $N!/n!(N-n)!$, where $N = 10$ and $n = 7$, and adding the permutations for $n = 8, n = 9$, and $n = 10$, there would in this case be a total of 176 possible ways of fulfilling the conditions of scientificity.

In a similar vein, many authors have argued that, for demarcation purposes, we must do with a reasonable *profile* of any given field rather than with a clear-cut distinction (e.g., [Thagard, 1988; Derksen, 1993; Eflin *et al.*, 1999]). In other words, it will be worthwhile to attempt to come up with a whole battery of science indicators. Such a cluster of criteria should be as comprehensible as possible, and enable us to examine every possible field of knowledge by a list of marks noting the presence or absence of the relevant features, or the compliance or noncompliance with some, e.g. methodological, rule. On this basis we should be able to come to a well-reasoned (and hence rational) conclusion concerning the scientific or nonscientific status of a cognitive field.

4 CHARACTERIZING FIELDS OF KNOWLEDGE

As is obvious from the preceding section, scientificity has been ascribed to many items: individual statements, problems, methods, systems of statements (theories in the strict sense), entire practices (theories in the broad sense), historical sequences of theories and/or practices (research programs), and fields of knowledge. Given the notorious problems with the traditional demarcation criteria, it seems promising to try the most comprehensive approach, for it allows us to consider the many facets of the scientific enterprise, namely the fact that science is at the same time a body of knowledge and a system of people including their activities or practices, and hence something that did not come into existence ex nihilo, but has developed over several centuries from a mixed bag of ordinary knowledge, metaphysics and non- or at most pre-scientific inquiry. This most comprehensive approach is the one focusing on fields of knowledge (see, e.g., [Thagard, 1988]). As we shall see at the end, this approach has the advantage that, by demarcating entire fields of knowledge as scientific or nonscientific, it allows us to also evaluate individual components of such a field, like characteristic principles and methods, as being scientific or not.

Before we begin to determine whether or not a field of knowledge is scientific, we must first define what a field of knowledge is. In a chapter on pseudoscience, Thagard [1988] just refers to fields of knowledge, without, however, offering much of a characterization. In their work on "interfield theories", Darden and Maull [1977] point out that fields are characterized, for example, by a certain domain of facts as well as a number of problems, methods and theories concerning that domain. However, they do not use their characterization to demarcate between scientific and nonscientific, let alone pseudoscientific, fields. The most comprehensive characterization of epistemic fields has been proposed by Bunge [1983a; b], who has moreover explicitly used it for demarcation purposes [Bunge, 1982; 1983b; 1984]. For this reason, I shall rely heavily on his analysis, but will readily modify it whenever necessary to make it better suited to the task at hand.

4.1 Epistemic Fields

Roughly speaking, an epistemic field is a group of people and their practices, aiming at gaining knowledge of some sort. Thus, physics and theology, astronomy and astrology, psychology and parapsychology, evolutionary biology and creationism, art history and mathematics, medicine and economics, philosophy in general and epistemology in particular, as well as biology in general and genetics in particular are examples of epistemic fields. These examples show that epistemic fields, or, if preferred, cognitive disciplines can be more or less inclusive; in other words, they may be structured hierarchically. (Note that in the following we shall not distinguish between "field" and "discipline", although one might argue that the term "discipline" be reserved for denoting generally acknowledged or institutionalized fields.) They also indicate that the knowledge acquired in an epistemic field need neither be factual nor true: we may acquire knowledge about purely fictional rather than factual entities, and our knowledge may be false or illusory. (Thus, we do not adopt the classic definition of "knowledge" as "justified true belief", but rather the Popperian view that all knowledge is hypothetical, so that it can turn out to be either true or false.) Finally, it is immaterial whether the aim of our cognitive activities is either epistemic or practical, or both.

These examples of fields of knowledge just serve as a starting point for a more detailed characterization. In his characterization of epistemic fields, Bunge [1983a] considers ten aspects:

1. the group or *community* C of knowers or knowledge seekers;
2. the *society* S hosting the activities of C;

3. the *domain* or universe of discourse D of the members of C, i.e., the collection of factual or fictional objects the members of C refer to in their discourse;
4. the *philosophical background* or general outlook G, which consists of
   (a) an *ontology* or general view on the nature of things,
   (b) an *epistemology* or general view on the nature of knowledge, and
   (c) a *methodology*, *axiology* and *morality* concerning the proper ways of acquiring and handling knowledge;
5. the *formal background* F, which is a collection of logical or mathematical assumptions or theories taken for granted in the process of inquiry;
6. the *specific background* B, which is a collection of knowledge items (statements, procedures, methods, etc.) borrowed from other epistemic fields;
7. the *problematics* P, which is the collection of problems concerning the nature, value or use of the members of D, as well as problems concerning other components listed here, such as G or F;
8. the *fund of knowledge* K, which is the collection of knowledge items (propositions, theories, procedures, etc.) obtained by the previous and current members of C in the course of their cognitive activities;
9. the *aims* A, which are of course the cognitive, practical or moral goals of the members of C in the pursuit of their specific activities;
10. the *methodics* M, which is the collection of general and specific methods (or techniques) used by the members of C in their inquiry of the members of D.

Note that these aspects come in a certain logical order. For example, the method used to find out something in a given field depends on the problem to be solved, on what we already know and on our aims. Thus, Bunge analyzes an epistemic field E, for any given time, as an ordered set or, more precisely, a ten-tuple

$$\mathcal{E} = \langle C, S, D, G, F, B, P, K, A, M \rangle.$$

Since our emphasis here is on the usefulness of these coordinates for demarcation purposes, we can disregard the question of whether their order is optimal or whether an alternative order would be more adequate (e.g., exchanging P and K). Bunge calls the first three components of this ten-tuple the *material framework* of the given epistemic field, although he admits that this is a misnomer in the case of fields like mathematics and the humanities whose domains consists mostly or even exclusively of nonmaterial objects. In any case, C and S do consist of concrete objects, namely persons and systems of persons. Consisting mostly of abstract objects, the last seven components make up the *conceptual framework* of the field, which may as well be equated with Kuhn's notion of a paradigm or disciplinary matrix. This name too is a misnomer in some cases, because the methodics M need not only consist of rules and procedures as conceptual entities, but may also comprise material objects (artifacts) such as measuring instruments.

Most of the members of E will be obvious, such as D, G and M, but some remarks may nonetheless be helpful. For example, the two coordinates C and S indicate that cognition and knowledge are not self-existing, but activities of real people in a particular social environment. Only by taking these aspects into account can we do justice to the history, psychology, and sociology of knowledge. But why distinguish C from S, since C is actually a subsystem of S? Because the community C may have interesting sociological features worth examining and because it may emerge or go extinct, without necessarily having a serious effect on the entire society in which it exists or had existed. Think of L.R. Hubbard's scientology movement.

The problematics P and the aims A of an epistemic field are important characteristics, because the same domain may be studied by asking different questions, and with different aims. For example, biochemistry and molecular biology study virtually the same objects, namely certain classes of molecules, but they concern different problems: whereas biochemistry studies these molecules under purely chemical auspices, molecular biology is interested in the biological function of these molecules in living organisms. Similarly, the same object may be studied to simply learn more about it, or to control it by technical means. For example, the phylogeneticist may just be interested in the evolution of mosquitoes, whereas the applied entomologist and especially the ecotechnologist may be interested in how to control their population and restrict their geographical distribution.

4.2 Scientific epistemic fields

When speaking of science we are first of all interested in the *factual* (often called empirical) sciences, such as physics and chemistry, biology and psychology, as well as the social sciences. (Note that we prefer the expression "factual science" over "empirical science" because the advanced sciences are not just empirical, but have well-developed theoretical branches.) An epistemic field $\mathcal{S}$ is a (factually) *scientific* field if the elements of any ten-tuple $\langle C, S, D, G, F, B, P, K, A, M \rangle$ approximately satisfy the following conditions [Bunge, 1983b].

1. The community C of the field is a *research* community: it is a system of persons who share a specialized training, hold strong information links amongst each other, and initiate or continue a certain tradition of inquiry. Thus, every researcher belongs to either a local, regional, national, or international community of colleagues.

2. The society S hosting C supports or at least tolerates the activities of the persons in C. In particular, it allows for research free from authority, in that it does not proscribe which of its results have to be accepted as true, or else be rejected as false.

3. The domain D of a factual science deals exclusively with *concrete* entities (past, present and future), their properties and changes. These entities may be elementary particles, living beings, human societies, or the universe as a whole. Some of the entities hypothesized in a factual discipline may turn out not to exist really, but if they were real, they would be concrete (as opposed to abstract) entities.

4. That science rests on certain philosophical assumptions is rather uncontroversial. There is less agreement, however, as to which particular assumptions are characteristic of science. Let us therefore discuss some of the philosophical principles that are good candidates for membership in the general philosophical outlook G of any scientific field. To this end, consider a simple physiological experiment, which can be done in biology class (Fig. 1).

   Where is the hidden philosophy in this experiment? Unlike the solipsist or the follower of George Berkeley, the normal scientist does not assume that, when she is actually carrying out this experiment, it is occurring only in her mind. Nor does she suppose that a supernatural entity is producing the entire situation in her mind. We cannot prove that this is not actually the case, but it simply does not belong to the scientists' presuppositions. By contrast, the scientist takes it for granted that this experiment is occurring in an outer world existing independently of her mind, but including her as a part.

   Imagine we repeat this experiment several times under the same conditions. The first time, the gas produced would be helium, the second time oxygen, the third time no gas at all would appear. The fourth time, the entire setup would explode before even adding hydrogen peroxide, and the fifth time four of the test tubes would turn into chewing gum, whereas the fifth would fly off to the ceiling. For some reason such weird things do not happen. Instead, things remain the same under the same conditions. Moreover, the outcome of the experiment is *ceteris paribus* the same: the gas always consists of oxygen. Furthermore, its amount depends on the pH in the test tube, whereby the highest amount is produced at a pH of 8. Obviously, the properties of the things involved are constantly (i.e., lawfully) related. Imagine further that for some reason we do not get any gas out of the test tubes at all. In this case the scientist would not believe that the gas has disappeared into nothingness, but that there must be something wrong with the setup.

   Excluding effects coming out of nothingness or from some supernatural realm, the scientist further assumes that it is her adding hydrogen peroxide which causes the production of oxygen. In other words, by manipulating some part of the setup a certain effect can be produced, whereby the steps in this process are ordered: the steps in the causal chain follow each other rather than occurring capriciously. Furthermore, the scientist takes it for granted not only that no supernatural entities, like friendly fairies or evil

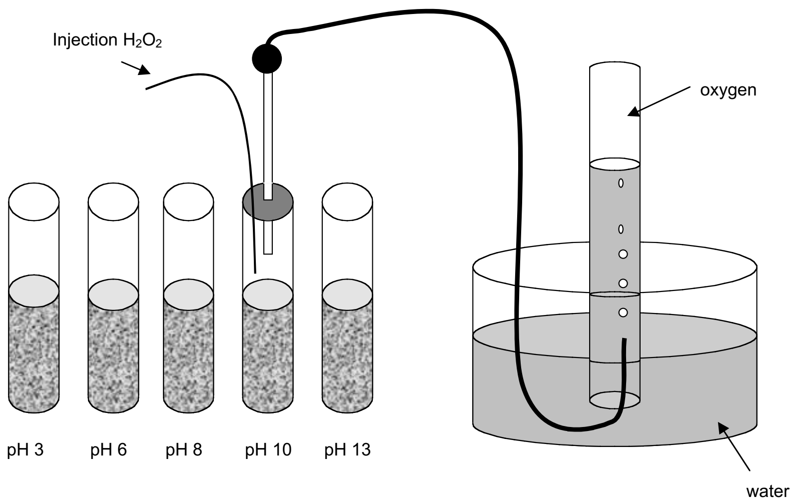

Figure 1. Take five test tubes filled with water and add a certain amount of yeast. Furthermore, by adding different amounts of hydrochloric acid (HCl) or caustic soda (NaOH) respectively, we arrange for a different acidity or alkalinity respectively in each tube, say, pH 3, pH 6, pH 8, pH 10, and pH 13. The yeast cells contain the enzyme catalase, which enables them to break down hydrogen peroxide into water and oxygen (i.e., $2H_2O_2 \rightarrow 2H_2O + O_2$). Upon adding a certain amount of hydrogen peroxide into one test tube after the other (by means of a syringe, for example), we each time close the tube and measure the amount of gas produced after 2 minutes by collecting it in a measuring tube, which is connected to the given test tube by a thin rubber hose. We do not need to specify the precise amounts and conditions here, because the basic setup of this experiment will be clear anyway (redrawn and modified from Knodel 1985, p. 39).

demons, meddle with the experiment either in the positive or in the negative, but also that they do not influence her own thinking, e.g., by making her hallucinate. And finally, she assumes that neither she herself nor anybody else can affect the setup by pure thinking or wishing alone, but only by acting; in other words, she takes it for granted that it is neither her own mind nor the mind of her colleague nor that of some little green alien on another planet which causes the outcome of the experiment.

In all this experimenting our scientist believes of course that she can get to know something about what is going on. Moreover, she also believes that the setup can be improved if necessary, and that thereby the precision of the measurement can be increased. Indeed, by varying and improving the experiment, she will find out that her earlier datum "The most oxygen is produced at a pH of 8.0" was not quite true, but that the maximum production occurs at a pH of 8.5. In other words, the initial finding was only an approximation to the real fact.

Let this sample of tacit assumptions suffice. It is time to extract some of the ontological, epistemological, and semantic *isms* or principles involved here.

a) Ontological assumptions

Despite the efforts of the positivists to denounce metaphysics as nonsense, it has long been acknowledged that science and metaphysics, though different, are related — and often even fruitfully so (see, e.g., [Agassi, 1964]). After all, one might argue that science is the emancipated daughter of metaphysics. As is indicated by the experiment described in the preceding, some minimal set of ontological tenets is presupposed even by modern science.

The first candidate is of course ontological realism, i.e., the thesis that there is a mind-independent world, whose inhabitants may become the subject matter of scientific investigation. Ontological realism is among the least controversial philosophical presuppositions of science, as is also indicated by Alters's [1997] survey mentioned earlier, in which about 90% of the interviewed philosophers agreed with the thesis that science presupposes realism. Note that ontological realism says nothing about whether this real world can be known and, if so, how and to which degree. This is a matter of epistemological realism. (It is, by the way, mostly the latter which is the target of antirealist criticism.)

The next assumption is ontological naturalism. This is the thesis that the inhabitants of the real world are exclusively natural as opposed to supernatural. Whether or not there is a transcendent world beyond our universe (if this very idea makes sense in the first place), our universe is causally closed, that is, there is no interaction with any possible other-worldly entities. Many philosophers of science would go even further and posit that there can be no interaction of concrete and spiritual as well as abstract entities either, even if the latter were natural ones — which reduces naturalism to materialism (e.g., [Armstrong, 1995; Mahner and Bunge, 1997]). Note that naturalism involves the parsimony principle (see Sect. 4.2, 4c). The least parsimonious view would be some sort of non-interventionism. This is the thesis that the universe is full of supernatural entities, but these have somehow agreed never to interfere with scientific measurements or experiments. Evidently, this view is quite arbitrary and nonparsimonious.

The third ontological ingredient of science is the principle of lawfulness. This is the hypothesis that the real world is not capricious, but behaves in a regular fashion. Indeed, if things behaved lawlessly, the world would resemble a cartoon movie in which everything can change into anything, forward and backward in time, in a completely arbitrary fashion. Presumably, there would be no living beings, no knowledge and no technology if the world were lawless. Note that the principle of lawfulness does not presuppose Laplacean determinism, because there are also stochastic processes — which follow probabilistic laws. Note further that, if laws as ontic regularities are distinguished from law statements purported to represent such laws, the various criticisms of the concept of natural law in science (e.g., by [Cartwright, 1983] and [Giere, 1999]) mostly concern the latter, i.e., the epistemological notion of a law. A too rigid traditional conception of natural law statements held by many philosophers of science, and our difficulties with idealization and approximation in representing real laws must not lead us to conclude that, as a consequence, there are no laws in the ontic sense, i.e., that the world behaves irregularly or even miraculously.

The fourth ontological presupposition is the principle of antecedence, which is often conflated with the causality principle. The antecedence principle maintains that causes precede their effects or, alternatively, that the presence is (causally or stochastically) determined by the past. By contrast, the principle of causality in the strict sense states that every event has an (external) cause producing the given event; more precisely, for every event e in some thing x, there is another event e' in some (other) thing $x' \neq x$, such that e' causes e. But since there are spontaneous (uncaused) events, such as exemplified by certain quantum events like radioactive decay, it is false as a universal principle. Nonetheless, in the case of our above experiment, we also need some version of the causality principle to account for the fact that our actions have some effect on the world.

The fifth ontological presupposition of science may be called the genetic or ex-nihilo-nihil-fit principle. Going back at least to Epicurus and Lucretius, this principle says that nothing comes out of nothing and nothing disappears into nothingness. Note that "nothing" here really means "nothing": even the curious vacuum field filling up empty space is *something* rather than nothing, for it can affect other things. (Note, incidentally, that this ontological assumption also affects physical cosmology: although one might be prepared to make exceptions for the universe as a whole, the genetic principle should encourage us to explore and prefer cosmological models, even big bang models, that do not assume a *creatio ex nihilo*, but presuppose some pre-existing state of the universe.)

Finally, there is the "no psi" principle [Broad, 1949; Bunge, 1983b], which is the postulate that minds or brain processes do not act directly on the things out there, but only through some motoric action of our body. Nobody could trust the readings of any measurement instrument or the results of any experiment if immediate mental forces and causes permeated the world.

These ontological principles must not be seen in isolation: they are a package deal. The idea that there are real and natural things, behaving lawfully and not popping out of, or into, nothing, is certainly the major metaphysical guide line of factual scientists. Note that these ontological and epistemological principles could all be false, which is why they are hypotheses or postulates, not ideological dogmas, as some critics of science tend to claim. However, both their eminent fertility and the extraordinary success of science justify that we accept them as true — for the time being. We might therefore call them the ontological default assumptions or, in some cases, metaphysical null hypotheses of factual science.

b) Epistemological assumptions

In order to do factual science, ontological realism must be combined with epistemological realism, i.e., the thesis that the real world can be known, if only approximately and imperfectly. Otherwise, scientists would just study the figments of their imagination, and technologists were unable to successfully alter real things, because this presupposes that at least some relevant properties of those things are known correctly.

Now, epistemological realism comes in different versions and strengths (see, e.g., the overview by Kuipers 2001, Ch. 2). We need not commit ourselves here to any position, although the most widely accepted version is likely to be what is often called scientific realism, which stipulates that we can know not just observables, but also unobservables. Elementary physics and evolutionary biology, for example, would make little sense without this assumption.

But what about instrumentalism, conventionalism, and other antirealist epistemological positions held on occasion by both scientists and philosophers of science? Are they not more parsimonious than realism? It is not just the claim that the majority of working scientists adopts realism in their daily work, but also the fact that, both in science and metascience, we should accept that position that has the greatest explanatory power and fecundity. In this regard realism beats instrumentalism, because the latter can explain neither the success nor, more importantly, the failure of scientific theories.

Moreover, whereas the instrumentalist cannot explain what the realist does and thinks, the realist is able to explain what the instrumentalist does. Thus realism subsumes instrumentalism [Vollmer, 1990; Kuipers, 2000]; see also [Kitcher, 1993] for an analysis of various antirealist arguments).

c) Methodological principles

A very general methodological maxim of any scientific approach is the principle of parsimony, also known as Ockham's Razor. It enjoins us not to multiply explanatory assumptions (entities, processes, causes, etc.) beyond necessity, in particular with respect to theoretical entities. It does not tell us, however, when such necessity obtains. Note that this principle is methodological, not ontological: it does not presuppose that nature is always and perhaps necessarily parsimonious, but that as inquirers we should begin with parsimonious assumptions. Note further that parsimony should not be readily equated with simplicity, such as the injunction to always prefer the simpler of two theories. After all, a theory can be simpler than another in many respects: it may be referentially simpler (having less qualitatively different referents), mathematically simpler, methodologically simpler (easier to test), or pragmatically simpler (easier to apply in a technological context). Simplicity in one such respect does not guarantee simplicity in another.

A second methodological principle is fallibilism or methodological skepticism. It is the acknowledgment of the fact that error is possible in all cognitive matters, so that our knowledge may be subject to criticism and, if possible, improvement and, if necessary, revision. We may highlight the latter by explicitly adding a "meliorist principle" [Bunge, 1983b] or a "principle of improvement of theories" [Kuipers, 2001].

d) Semantic assumptions

Most factual scientists maintain that their hypotheses, models and theories are true if they adequately represent the facts they refer to. That is, they subscribe to a correspondence theory of truth. Needless to say, the notion of truth is as tricky as many other concepts, so that there is no agreement among philosophers as to the appropriate truth concept in science [Weingartner, 2000]. Nevertheless, scientific realism is quite naturally associated with a correspondence concept of truth [Bunge, 1983b; Thagard, 1988; Devitt, 1996; Wilson, 2000]. Such a notion becomes easier to defend when we realize that the concept of correspondence truth provides just a semantic *definition* of "truth": it says nothing about how, and in particular how well, the truth of a hypothesis can be *known*. In other words, it does not provide a truth criterion. Truth criteria, such as evidential support, are not the business of semantics, but of methodology.

The concept of correspondence truth fits scientific practice even better when we realize that factual truth is in many cases not a dichotomy between true and false, but a matter of degrees. Models and theories often represent facts only in certain respects and moreover imperfectly so. Thus they correspond to facts only partially. Similarly, quantitative properties (represented by magnitudes) may be known only approximately, which is why scientists attempt to improve their measurement techniques. A realistic philosophy of science will therefore try to do justice to the idea of partial or approximate truth [Bunge, 1983b; Weston, 1992] and hence methods of truth approximation [Niiniluoto, 1987; Kuipers, 2000].

e) Axiological and moral assumptions

Most norms of science are built into its methodology. However, there are not only methodological values and norms, but also attitudinal and moral ones. Merton's [1973] expression "the ethos of science" captures this fact aptly, although his work is mostly concerned with attitudinal and moral norms that are not immediately relevant to the production of true knowledge (see below). To stress the fact that science has an internal system of values and corresponding norms, it may be useful to treat them all together. Thus, the researchers in a scientific field of knowledge are expected to accept the following values:

- *Logical values* such as the principle of noncontradiction and noncircularity. Together with the entire canon of valid reasoning, these are of course basic principles of rationality.
- *Semantical values* such as meaning definiteness, clarity, and maximal truth. Of course, a young or emerging scientific field may teem with vague and fuzzy concepts. But as it progresses and matures, in particular when it develops a theoretical branch, clarity and exactness are supposed to replace fuzziness. However heuristically fruitful vagueness may be in the beginning or in certain contexts, it may as well indicate that a field is degenerative rather than progressive.
- *Methodological values* such as testability (including the testability of the methods used in testing hypotheses, as well as the independent testability of auxiliary assumptions), explanatory power, predictability, reproducibility, and fecundity. Since these and other methodological categories are the main business of the philosophy of science, we shall not elaborate on them here.
- *Attitudinal*- and *moral values* such as critical thinking (or rationality in general), open-mindedness (but not blank-mindedness), universalism or objectivity (i.e., the requirement that ideas be evaluated independently of the personal, social or national characteristics of their proponents),

truthfulness, and acknowledgment of the work of others (e.g., by adequate citation).

As stated above, Merton's [1973] classic ethos of science concerns mostly attitudinal and moral values or norms, respectively. These are often abbreviated by the acronym CUDOS, which stands for four main norms: *c*ommunism (research results should be public property and accessible to everybody), *u*niversalism (see above), *d*isinterestedness (research should be uninfluenced by extra-scientific interests, and scientists should be emotionally detached from their subject matter), and finally *o*rganized *s*kepticism (scientists should be critical in particular towards their own work, and point out on their own weak spots or problematic parts). However, Merton's norms have been criticized for being too idealized and geared to an academic ivory tower situation (see [Ernø-Kjølhede, 2000] for an overview). Indeed, the history, psychology and sociology of science provide many examples that scientists have failed to follow one or more of these values. Like everyone else scientists are only human after all. Thus, individual scientists may be biased and jealous; they may intrigue against colleagues, or engage in nepotism; they often are emotionally attached to their subject matter in being passionate researchers, and they sometimes do not see the weak spots, if not flaws, in their own work; in particular, *pace* Popper, they are usually interested in having their hypotheses and theories confirmed, not refuted — after all, Nobel prizes are not awarded for the falsification of a theory. Moreover, the social and economic organization of scientific research has changed drastically during the past 50 years in that research institutions including universities are now run more like businesses, so that there is severe competition for funds and a strong pressure to focus on applied science and technology at the expense of basic science (see [Ziman, 1994]). For all these reasons Merton's classic ethos no longer describes realistically the behavior of scientists, however desirable his norms may still be from an ethical point of view (see also [Kuipers. 2001]). Finally, most of Merton's norms concern the professional social behavior of scientists in general, whereas the primary interest of the philosopher of science concerns those values and attitudes that are epistemologically relevant by contributing to gaining true knowledge, such as rationality, objectivity, and truthfulness.

In sum, the system of logical, semantical, methodological, and attitudinal ideals constitutes the *institutional rationality* of science [Settle, 1971], even though individual scientists may more or less often fail to behave rationally. (More on the problems of the rationality of science in [Kitcher, 1993].) And, however biased the individual scientist may be, the above values are also the basis for the *institutional objectivity* of science. As a consequence, basic science is value-free only in the sense that it does not make value judgments about its objects of study. In other words, basic science has no external value system.

This completes our extensive analysis of the philosophical outlook of a scientific field (condition 4), so that we proceed at last with our list of conditions characterizing an epistemic field as scientific.

5. The *formal background* F of a scientific field is a collection of up-to-date logical and mathematical theories used by the members of C in studying the items of D. This does not imply that scientificity is to be equated with formalization. All this criterion demands is that formal tools have to be handled correctly, and they must be adequate to tackle any given theoretical problem.

6. The *specific background knowledge* B is a collection of up-to-date and reasonably well-confirmed data, hypotheses, theories, or methods borrowed from adjacent fields. Every scientific field uses some knowledge from other scientific fields. For example, biology borrows knowledge from physics and chemistry. A science that borrows little from other fields is either very fundamental or very backward.

7. The *problematics* P is of course the collection of problems to be solved in the given field. It consists exclusively of epistemic questions on the nature and in particular on the lawful behavior of the objects in its domain D. It may also comprise problems concerning other components of its conceptual framework (e.g., the adequacy of methods, formalisms, and other background assumptions). If a discipline deals with practical problems, it is a technology, not a basic science.

8. The *fund of knowledge* K is a *growing* collection of up-to-date, testable and well-confirmed knowledge items (data, hypotheses, theories), gained by C and compatible with those in B. Even a young scientific field will possess some fund of knowledge, either taken over from ordinary knowledge or inherited from a parent science.

9. The *aims* A of the members of C of a field in basic science (as opposed to technology) are purely cognitive. They include, for example, the discovery and use of the laws of the members of D; the systematization of the knowledge in K (e.g., by constructing general theories); and the refinement of the methods in M.

10. The *methodics* M is a collection of empirical methods or techniques which may be used by the researchers in C in their study of the members of D, whereby "method" means a rule-directed procedure for collecting data or testing a theory. (Note that methods of reasoning, such as rules of inference or rules for evaluating theories, have been treated as belonging in G. Whence the distinction between methodics and methodology.) A scientific technique may be either concrete (i.e., involving instruments), such as electron microscopy, or conceptual (formal), such as the various statistical methods.

And they may be quite specific, such as Hennig's method of reconstructing phylogenies, or else more or less general, i.e., applicable in several fields, or for different purposes.

Among the methodological requirements for a technique to be scientific are the following. The functioning of these methods should be scrutable (e.g., by alternative procedures) and explainable by well-confirmed theories. (This may not be the case in a young field, but it should be achieved as the field matures. For example, when Galileo used his telescopes, optics was still too immature to fully explain their functioning.) And the techniques must be objective in the sense that every competent user is able to obtain roughly the same results.

It has been quite controversial whether there is such a thing as a scientific method in general (see, e.g., [Laudan, 1983; Haack, 2003]). If such a general method is expected to be a fool-proof procedure for delivering true and certain knowledge, then there is of course no such method. However, if we view the scientific method as an extremely general research *strategy*, then there may very well be a scientific method. For example, the sequence "problem–hypothesis–test–evaluation" reflects the general structure of any empirical scientific paper (introduction, methods, results, discussion), and may thus be seen as representing *the* scientific method [Bunge, 1983b]. However, if this definition is accepted, the scientific method is at best a necessary, but not a sufficient condition of scientificity: its application does not automatically turn one's inquiry into a scientific inquiry. Moreover, being extremely general, it is not an empirical method proper, so that it may as well be seen as belonging to the methodological rules in G.

In addition to the ten conditions of the ten-tuple $\langle C, S, D, G, F, B, P, K, A, M\rangle$ used in the preceding to characterize a scientific field, Bunge (1983b) requests that a scientific field satisfy two further conditions. These conditions take into account two aspects of science that have been emphasized by many philosophers of science: unity (consilience) and progressiveness.

11. The *systemicity condition.* There is at least one other field of research $\mathcal{S}'$ such that $\mathcal{S}$ and $\mathcal{S}'$ share some items in G, F, B, K, A and M; and either the domain D of one of the two fields $\mathcal{S}$ and $\mathcal{S}'$ 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 [Bunge, 1983b, p. 198]. In simpler words, every scientific field has connections with other fields — a fact which allows for multi- and interdisciplinary research. This is due to the fact that nature is organized into several levels of complexity — levels that scientific disciplines may approach from various perspectives and with different aims and methods. Thus, despite all the differences in our cognitive interests, scientific disciplines form a network of approaches, striving for a unified — a consilient or convergent — view of nature, which need not be a reductionist

one [Kitcher, 1982; Bunge, 1983b; Bechtel, 1986; Thagard, 1988; Vollmer, 1993; Reisch, 1998]; for a dissenting view see [Dupré, 1993]. For this reason, new theories are evaluated not only on the basis of empirical tests, but also with regard to their overall compatibility with the well-confirmed background theories (external consistency). Although a new theory cannot by definition be compatible with every other theory, in particular its rivals, because it would otherwise not be a new theory, it must somehow allow to be accommodated within the totality of our knowledge. In Kuhnian terms: even if revolutionary, a new theory will cause only local or regional revolutions, never a total revolution turning upside down all existing fields at once.

12. The *changeability* or *progressiveness condition.* The membership of the conditions 5–10 changes, however slowly and meanderingly at times, *as a result of research* in the same field or as a result of research in neighboring disciplines. In Lakatosian words, the history of a scientific discipline must be progressive, at least on the whole. Even if science were to come to an end in the distant future, the history of a scientific discipline would have to show a certain amount of progress. (How the view that science is progressive can be defended against various antirealist objections has been shown by [Kitcher, 1993].)

This concludes the characterization of scientific epistemic fields. Note, firstly, that this characterization applies first of all to contemporary science, because many of its features have developed into their current state over the past 400 years. Consequently, it may not be fully applicable to 17th century science, for example. As for its future development, I doubt that the basic features and principles discussed above will evolve in a way that leads to their replacement by completely different principles, in particular their contraries. However, future development might consist in their improvement as well as in the discovery of some as yet unknown features and principles.

Note, secondly, that this characterization comprises both descriptive and normative aspects. Whereas the descriptive conditions provide diagnostic indicators, the normative ones will be the foundation for any judgment on the scientificity, or nonscientificity respectively, of an epistemic field.

What about science as a whole? Science as a whole is of course the totality of all individual scientific disciplines. If, as in the preceding, we represent each scientific field as a ten-tuple $\mathcal{S}_1 = \langle C_1, S_1, D_1, G_1, F_1, B_1, P_1, K_1, A_1, M_1 \rangle$, $\mathcal{S}_2 = \langle C_2, S_2, D_2, G_2, F_2, B_2, P_2, K_2, A_1, M_2 \rangle, \ldots, \mathcal{S}_n = \langle C_n, S_n, D_n, G_n, F_n, B_n, P_n, K_n, A_n, M_n \rangle$, science as a whole can be conceived of as the sum of these ordered sets: $\Sigma = \mathcal{S}_1 + \mathcal{S}_2 + \ldots + \mathcal{S}_n$. Similarly, we could characterize a *multidiscipline*, consisting of two or more scientific fields, as the sum of two or more ten-tuples representing them [Bunge, 1983b, p. 219]. In the case of a two-field multidiscipline this would be represented by: $\mathcal{S}_1 + \mathcal{S}_2 = \langle C_1 \cup C_2, S_1 \cup S_2, D_1 \cup D_2, G_1 \cup G_2, F_1 \cup F_2, B_1 \cup B_2, P_1 \cup P_2, K_1 \cup K_2, A_1 \cup A_2, M_1 \cup M_2 \rangle$. (Note that we represent the concrete systems C and S by their composition, i.e., the set of their components.

Otherwise we would need an operation of physical or mereological addition rather than simply the one of set-theoretical union.)

By contrast, an *interdiscipline* does not just consist of at least two fields retaining their identity, but it is a merger of fields attempting to approach a common domain from a unified point of view rather than from different angles. Therefore, an interdiscipline may be conceived of as the *intersection* of two or more fields.

The analysis of a scientific field as a ten-tuple also allows us to elucidate the notion of a scientific research project. In section 4.1 we have defined the conceptual framework of an epistemic field as a septuple $\mathcal{S}_c = \langle G, F, B, P, K, A, M \rangle$. A *research project* π within a scientific field $\mathcal{S}$ characterized by a conceptual framework $\mathcal{S}_c = \langle G, F, B, P, K, A, M \rangle$ is then the septuple $\pi = \langle g, f, b, p, k, a, m \rangle$, where every component is a subset of the corresponding component of S_c [Bunge, 1983b, p. 176].

How does Lakatos's notion of a research program fit into this conceptualization? According to Lakatos [1970], a research program is a historical sequence of theories. Now theories surely belong to the fund of knowledge K of a scientific discipline. But we must also include the reference class of the theory belonging in D, as well as the formalism used to built the theory, which belongs in F. Further, Lakatos also counts auxiliary and other relevant assumptions as belonging to a theory. These may belong either in B or in K. Thus, a theory ϑ at any given time t might be construed at least as a quadruple $\vartheta(t) = \langle d(t), f(t), b(t), k(t) \rangle$, and a research program ρ over a period τ, where $\tau = [t_1, t_n]$, as an ordered set of such quadruples, $\rho(\tau) = \langle \langle d(t_1), f(t_1), b(t_1), k(t_1) \rangle, \langle d(t_2), f(t_2), b(t_2), k(t_2) \rangle, \ldots, \langle d(t_n), f(t_n), b(t_n), k(t_n) \rangle \rangle$. Depending on what we take to belong to a theory, we might as well regard a research program as a sequence of research projects as defined in the previous paragraph. Or, disregarding the historical focus of Lakatos's concept, we might simply redefine "research program" in the broad sense of "research project" or even "conceptual framework" or "disciplinary matrix" as explicated above (see, e.g., [Kuipers, 2001] for an even broader conception of "research program"). I take this broader approach to be more useful for demarcation purposes than Lakatos's idea of a series of theories in themselves.

So much for a possible characterization of the notion of a scientific epistemic field, which views science in the sense of basic factual science. It is now time to take a look at other research fields which, though not factual sciences, are related to them: mathematics, technology, and the humanities.

4.3 Other Research Fields

4.3.1 Mathematics

In contrast to the factual sciences, mathematics as well as formal logics and semantics are often called *formal sciences*. Although they have much in common with the factual sciences, the question is whether these commonalities justify to regard them as sciences. In other words, the question is whether we should use the label "science" in the strict sense of factual science or in a broader sense including formal science and perhaps technology.

Let us quickly analyze the status of mathematics with regard to the twelve conditions listed in Section 4.2. In so doing, we shall mention only those conditions that show significant differences.

Clearly, the domain D of mathematics shows an important difference with factual science: all the referents of mathematics are abstract objects. Although we can apply mathematical concepts and theories to concrete things, their properties and processes, we do so only by interpreting them in factual terms. In this way we represent factual properties in formal terms. Pure mathematics does not deal with concrete objects.

The philosophical background G of mathematics is also quite different. To begin with, mathematics can do without ontological realism: it would work just as well if there were no mind-independent reality. Of course, most mathematicians are *de facto* also ontological realists, but this is not a necessary assumption for doing mathematics: mathematics can be done on the basis of a Platonist, nominalist, or constructivist ontology (see, e.g., [Agazzi and Darvas, 1997]). Being just as ontologically neutral as logics [Nagel, 1956], mathematics has no use for the other ontological assumptions of factual science either, except for the principle of lawfulness. Indeed, mathematicians also assume that the referents of their discourse "behave" lawfully, whether they be found in a Platonic realm of ideas or whether they be constructed by our minds. Depending on the philosophy of mathematics adopted, the mathematical Platonist will need a form of epistemological realism, whereas the constructivist can do without it.

A major difference lies in the semantic concept of truth in mathematics: dealing with abstract objects and thus purely formal properties, mathematics is in no need of a correspondence theory of truth and hence can do with a coherence theory of truth (recall Leibniz's *verités de raison*; see also [Bunge, 1983b]). Only the mathematical Platonists and empiricists may have use for a correspondence theory of mathematical truth. Still, mathematical truth is *de facto* established by formal coherence.

The methodological, attitudinal and moral values are by and large the same as in factual science. The major difference here lies in the notion of testability, which can only mean conceptual testability, not empirical testability. Moreover, testability in mathematics is stronger than empirical testability, because it allows for conclusive proof and disproof, whereas empirical testability only provides confirming or disconfirming instances.

As a consequence of the differences mentioned so far, there is another difference in the methodics M: mathematics uses no empirical, but only conceptual methods. (Even though some proofs obtained with the help of computers, such as that of the four color problem, may imitate empirical means in certain respects, they are still virtual and hence conceptual. Likewise, thought experiments, whether in mathematics or in the factual sciences, are conceptual means.) However, being extremely general, the scientific method, as defined in Sect. 4.2, seems to be used in mathematics as well.

As is obvious from the preceding, the main differences between mathematics and the factual sciences lie in the fact that it deals exclusively with abstract objects. On the other hand, mathematics too is a rigorous and progressive research field, consisting of a set of fruitfully interacting subfields.

4.3.2 Technology

In popular thinking, science and technology are often conflated. Worse, industrial production and marketing of technical goods is often equated with technology, which is in turn equated with science. So science gets often blamed for everything negative associated rightly or wrongly with the Western capitalist way of living. However closely these areas may be related *de facto*, the philosopher of science or of technology is of course interested in the question of whether science and technology can be distinguished *de jure*.

Borrowing again from Bunge [1983b], I shall propose the following distinctions. To begin with, the investigation of cognitive problems with *possible* practical relevance will be termed *applied science*. Thus, an applied science differs from its basic science partner mostly in its problematics (P) and aims (A). Further, its domain D will be narrower. For example, in contrast to human biology, medical research studies only those properties of humans that concern, directly or indirectly, matters of health. The same holds for clinical psychology as opposed to psychology in general.

If we now add the requirement that, on top of having discovered or studied some X which may be useful to produce (or else prevent) some Y, we actually *design* an artifact or a procedure to produce or else prevent Y, we arrive at technology. More precisely, *technology* may be defined as "the design of things or processes of possible practical value to some individuals or groups with the help of knowledge gained in basic or applied science" [Bunge, 1983b, p. 214].

Note first that, by making technology dependent on science, this definition distinguishes technology from the traditional crafts or *technics*, which are based solely on ordinary knowledge. Note further that this definition is so wide that it includes not only the classic fields of physical and chemical engineering, but also biological, psychological and social technologies. Thus, medicine, psychiatry, pedagogy, law, city planning, and management "science" are all technological fields.

Let us briefly review the coordinates of the ten-tuple $\langle C, S, D, G, F, B, P, K, A, M\rangle$ as to the differences between science and technology. As in the preceding section, only those showing significant differences will be mentioned. To begin with, although C is a research community, it is not as international and universalist as in the case of basic science, because patents and industrial secrets limit the circulation of technological knowledge. The domain D is both narrower and wider than in the case of applied science: it is narrower because it is concerned only with natural things which are useful for us, and it is wider because it includes not only natural things and processes but also artificial ones. The general outlook G

shares a realist and naturalist ontology and epistemology with basic science, as well as most of the other philosophical assumptions and values. The main difference lies in the fact that technology does not test so much for truth as for efficiency. Truth is relevant only as a means for design and planning. Finally, the ethos of technology differs from that of basic science: usually, it consists not in the free and disinterested search for knowledge, but in task-oriented work, often depending on the economic interests of some employer (see also [Ziman, 1994]). Obviously, the problematics P and the aims A are among the main differences: the problems and aims are practical rather than cognitive. Moreover, the aim of technology is not to discover new laws: it suffices to make use of known ones. Finally, technology is characterized by a coordinate of its own: in contrast to basic science, technology has not only an internal value system, but also an external one (V). That is, it attributes positive or negative values to natural or artificial things or processes, be it raw material or finished product. Thus, a technology is actually characterized by an eleven-tuple $\langle C, S, D, G, F, B, P, K, A, M, V \rangle$.

4.3.3 Humanities

In contrast to the social sciences, which study social systems (composed of human individuals) and their activities by empirical means, the humanities mostly abstract from these concrete individuals and groups as well as their activities and study their intellectual (including artistic) products, i.e., ideas or concrete artifacts. Inasmuch as the humanities study the activities of groups or individuals, these are usually of an artistic nature, such as a theatrical or musical performance. Accordingly, literature and literary criticism, languages (philology) and part of linguistics, art history and criticism, musicology, the history of ideas, religious studies, and philosophy belong to the humanities. On the other hand, some fields like history and archeology, as well as the history and sociology of religion belong — or should belong — to the social sciences. Similarly, part of linguistics is a social science too. And according to our classification, the law (jurisprudence) and pedagogy are not humanities but sociotechnologies (Sect. 5.2). These examples show that quite often there is an overlap between some social sciences and the humanities. In particular, some fields starting out as humanities may develop into sciences.

Again, a quick review of the ten coordinates of an epistemic field will be in order. To begin with, the humanities are clearly research fields with a specialized research community C. As just mentioned before, their domain D consists of ideas and artifacts rather than natural things and processes. Consequently, the humanities are consistent with either a naturalist-materialist or a Platonist outlook. As for epistemology, the natural approach is most likely a constructivist one, which can be either realist or antirealist. Furthermore, the humanities are open to the influence of subjectivist philosophies like phenomenology and hermeneutics. (And of course, in the field of philosophy, which has to provide its own metaphilosophy, just anything goes.) In sum, the philosophical outlook of the humanities is much more variegated than that of the sciences, and necessary connections, if any, with particular philosophical presuppositions are much less obvious. Presumably, the more aspects of the much straighter scientific outlook are adopted, the better the chances of bridging a humanistic field with a scientific one. Think of linguistics and comparative religion (*Religionswissenschaft*), which make contact with sociology, history, evolutionary biology, psychology and, more recently, even the neurosciences.

As for methodology and semantics, since the humanities deal with ideas and artifacts, which are not to be explained by natural laws and mechanisms but instead interpreted and comprehended, it is unclear which role the parsimony principle plays in the humanities. More complex views and interpretations may be preferred to simpler ones, just as conversely. Similarly, fallibilism may not be that important because there may be different reasonable perspectives and interpretations, without implying that therefore one of them is erroneous. Consequently, the notion of truth in the humanities is often contextual or relative rather than factual. The fact that Othello killed Desdemona is (fictionally) true only in the context of Shakespeare's story. Another author could easily write an alternative play in which Desdemona kills Othello, so that in this context the opposite would be true. On the other hand, inasmuch as the humanities are descriptive of certain (e.g., historical) facts, these descriptions can be correct or not in the correspondence sense.

What about the internal value system of the humanities? Rationalist humanities will certainly respect the standard logical values. But there are also irrationalist branches, in particular in philosophy and certain postmodernist cultural studies (see Sect. 5.2). Very often the semantical values of clarity and exactness cannot be heeded. This is due to the very nature of human thought and communication, which is far from unambiguous, whence the need for interpretation arises. However, if these semantical values are not accepted even as remote ideals, and fuzziness is instead turned into method, the line to obscurantism may easily be crossed.

Evidently, the methodological values of testability and explanatory power in the scientific sense are not part of the humanities. A certain view, reading, or interpretation may be open to criticism, but since it is neither true nor false, it cannot be tested for truth. At most, it is reasonable, plausible, sensible, or apposite. Explanatory power may be replaced by "comprehensive power" if we admit the hermeneutic goal of understanding in the humanities. On the other hand, fecundity is certainly also a value in the humanities, because humanistic understanding can be increased if some approach opens up new perspectives.

Whereas some attitudinal values are of course the same as in the sciences, others are different. For example, just as there may exist competing theories in the sciences, there may be competing interpretations in the humanities. Honesty requires at least mentioning the existence of such competing approaches, even though the researcher wants to focus on her own. The same holds for the adequate citation of sources, although the standards appear to be lower than in the natural sciences. For example, it seems to be much easier to survive peer-review when disregarding the work of disliked colleagues in a philosophical article than in a science paper. Furthermore, the value of universalism plays only a minor role, if any, in the humanities. For, naturally, the humanities are more inclined towards relativism, because many cultural items cannot be evaluated independently of the personal and cultural characteristics of their creators: they must be seen and understood in context. Finally, like technology, many humanities have an external value system: they attribute, for example, aesthetic values, meanings, and purposes to the objects in their domain D, because the latter are studied in their relation to humans.

The formal background of the humanities, if any, is of course small. Exceptions occurring for example in philosophy, such as mathematical logics and formal semantics, may be classified as formal sciences. On the other hand, other branches of analytical philosophy too are formal (like ontology), which indicates that they are science-oriented, though not full-fledged sciences.

The aims of the humanities can be either cognitive or practical, or both. In contrast to the sciences, however, they usually do not seek to find laws. Indeed, the "sciences of the mind" (*Geisteswissenschaften*) have been regarded as descriptive (idiographic) rather than law-finding (nomothetic). On the other hand, we have seen before that some humanities make contact with the sciences, so that such multi- and interdisciplinary ventures may be able to find some cultural or even aesthetic laws.

Obviously, a major difference with the sciences is found in the methodics M of the humanities. Naturally, except for some observation, their methods are mostly conceptual. Among these are some general methods unique to the humanities, such as the hermeneutic and dialectic "method" [Poser, 2001], although these are not methods in the strict sense of rule-guided procedures to attain a certain goal. (Here, "hermeneutics" does not mean philosophical hermeneutics, but only the traditional concept of text interpretation, or understanding of works of art, respectively. And the dialectic method concerns first of all the discoursive triad thesis-antithesis-synthesis, without presupposing the whole of dialectic philosophy.) If not objective in the sense that every competent user will get roughly the same results, these "methods" are at least intersubjective in that their results can be communicated to, and understood by, other people. The humanistic scholar may also borrow or apply certain techniques from the factual sciences, but this does not yet turn her field into a science. For example, the art historian may have some paint chemically analyzed, or some cloth radiocarbon-dated, without thereby changing the nature of her discipline.

In sum, compared to formal science and technology, the humanities show the greatest distance from factual science. But again, we emphasize that this is not a value judgment. When saying, for example, that the arts and humanities are not scientific, nobody claims that they are therefore objectionable or bad.

4.4 Conclusion

The factual and formal sciences, the technologies, and the humanities are all research fields producing genuine knowledge, which on the whole is either (approximately) true or else useful, and contributes to the understanding of the world and its inhabitants. For this reason, one might argue that they should all be included in a broad conception of science. This is for example done in the German intellectual tradition, where the name of almost any field of knowledge is dignified by the ending "-wissenschaft" (-science), including the humanities, which are called *Geisteswissenschaften* (sciences of the mind). So there is bioscience alongside "music science", just as there is computer science alongside "literature science". Consequently, if a practitioner of a *Geisteswissenschaft* is told that what he does is not science, he will most likely be offended. It comes as no surprise that such a broad, if not inflationary, construal of "science" aggravates the problem of demarcation (see, e.g., [Poser, 2001]).

By contrast, most other traditions and languages separate the arts and humanities from the sciences already terminologically, so that no offense is given by calling the humanities nonscientific. Yet even so, the question remains of what to do with mathematics and technology. While some authors include both of them in the sciences (e.g., [Kuipers, 2001] classifies them as explicative research programs and design programs, respectively, within a broad conception of a *scientific* research program), others assert that neither mathematics [Lugg, 1987] nor technology [Bunge, 1983b] are sciences. In any case, taking into account the preceding overview, the common post-positivist picture, which admits more categories than just sense (i.e., science) and nonsense (i.e., all the rest), may look like the one given in Fig. 2. One the one hand, there is science including mathematics and technology; on the other there is nonscience including the arts and humanities as good nonscience, so to speak, for it too is viewed as producing true, reliable, or at least valuable knowledge, respectively, and finally pseudoscience as bad nonscience, for its knowledge claims are unjustified.

We may refine this picture by adding protoscience and prototechnology, as well as ordinary knowledge. These straddle the lines between pseudoscience and science. A protoscience is expected to develop into a science proper by leaving behind its nonscientific (or even pseudoscientific) roots (see Sect. 6). And ordinary knowledge is mostly nonscientific and reliable, but contains illusory items on the one hand, and some knowledge adopted from the sciences on the other (Fig. 3). It is the task of the science educator to increase the share of the latter and to decrease that of pseudoscience and superstition.

We shall further refine this picture later on to reflect the distinctions made above between factual science, mathematics, technology, and the humanities. Before, however, we need to take a closer look at that kind of knowledge which is not just nonscientific but in fact unscientific or pseudoscientific.

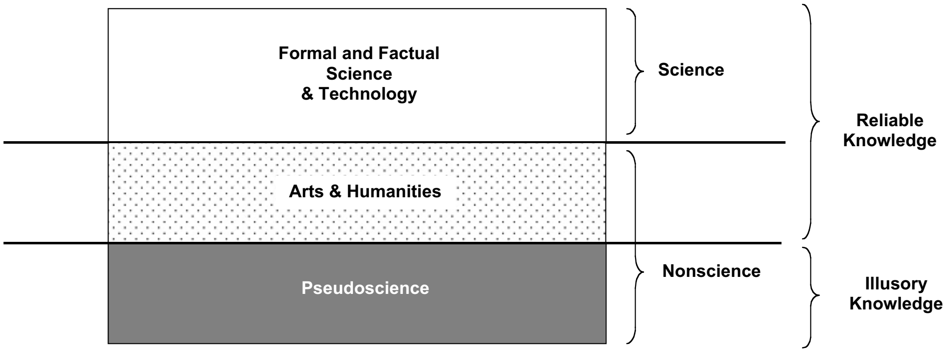

Figure 2. A common post-positivist picture of science and nonscience. As scientific research fields, mathematics, factual science (including psychology and social science), and technology are subsumed under the general label of "science". Nonscience divides into the arts and humanities (including philosophy) on the one hand, producing reliable or at least valuable knowledge, and pseudoscience on the other, offering nonreliable or illusory knowledge.

5 UNSCIENTIFIC FIELDS

As emphasized previously, calling an epistemic field *nonscientific* is not pejorative but descriptive. Calling it *unscientific*, however, is judgmental: it indicates that the given field cannot live up to its cognitive claims. Since there is no noun "unscience", an unscientific field is called a "pseudoscience". As usually defined, a pseudoscience is a particular form of nonscience, namely a nonscientific field whose practitioners, explicitly or implicitly, *pretend* to do science. Thus, to say that a field is pseudoscientific amounts to saying that it is a fake. In other words: While there is reliable or, if preferred, approximately true theoretical and practical nonscientific knowledge, the knowledge produced by pseudoscience is illusory. And since spreading bogus knowledge amounts to deception, pseudoscience has a moral dimension that other nonscientific fields lack. Therefore, a demarcation of science versus nonscience in general does not yet tell us how legitimate nonscientific fields are to be demarcated from pseudoscientific ones.

5.1 Characterizing Pseudoscience

For this reason, several authors have attempted to give not only a characterization of science as opposed to nonscience, but also of pseudoscience in particular [Thagard, 1978; 1988; Radner and Radner, 1982; Bunge, 1982; 1983b; 1984; Grove, 1985; Lugg, 1987; Derksen, 1993; 2001; Hansson, 1996; Wilson, 2000; Kuipers, 2001]. It will come as no surprise that the criticisms of such attempts parallel those leveled against any quick and clear-cut demarcation of science: though dealing with important aspects of pseudoscience, the proposed demarcation crite-

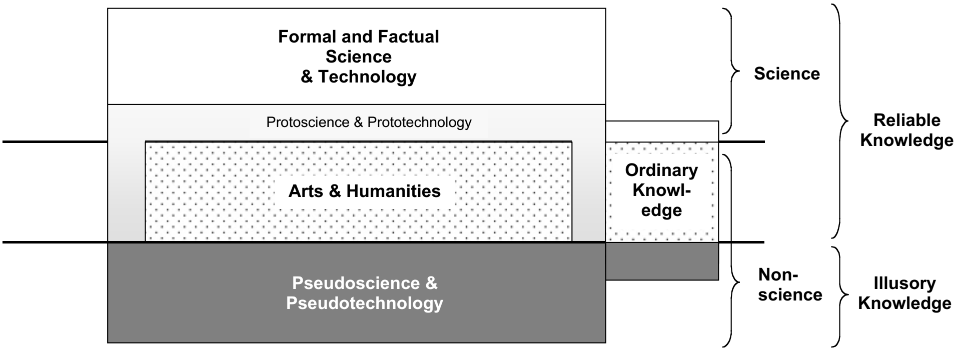

Figure 3. A refined post-positivist picture of science and nonscience, making room for ordinary knowledge as well as protoscience and prototechnology, which range from the pseudoscientific to the scientific.

ria do not combine to form a set of necessary and sufficient conditions, because they always leave some pseudosciences unscathed. Let us briefly review some such demarcation attempts.

Improving on his earlier demarcation proposal [Thagard, 1978], Thagard [1988, p. 170] contrasts his five characteristics of science mentioned in Section 3 with five features typical of pseudoscience. In pseudoscience, scientific correlation thinking is replaced by primitive resemblance thinking; empirical matters of confirmation and disconfirmation are neglected; practitioners of the field are oblivious to alternative theories; the theories are nonsimple and contain many ad hoc hypotheses; and there is no progress in doctrine and application. Thagard points out that these are indicators of pseudoscientificity, not necessary and sufficient criteria.

Grove [1985] gives four characteristics of pseudoscience. The first is the lack of an "independently testable framework of theory capable of supporting, connecting, and hence explaining their claims" (p. 237). The second is the lack of progress. Third, a pseudoscience is usually constructed in such a way that it is able to resist any possible counter-evidence; in other words, it is practically irrefutable (though it may be logically falsifiable). And fourth, according to Grove, not just irrefutability is a mark of pseudoscience but, more generally, their "total resistance to criticism".

Lugg [1987, p. 228] suggests regarding pseudosciences as "radically flawed practices, i.e., as radically flawed complexes of theories, methods and techniques". He maintains that, in the case of the pseudosciences, empirical matters are relatively unimportant, because their being conceptually flawed makes them unworthy of serious attention, whether or not their claims could actually be confirmed or disconfirmed. This is similar to Rothbart's [1990] claim that pseudoscientific theories are not testworthy. If we can already show by means of formal or informal logic that an argument or an approach is fallacious, there is no need to empirically test the hypotheses involved. Finally, according to Lugg, if pseudosciences are practices, they are social institutions, and realizing that they are such helps to explain their longevity and resilience.

Rationalistic approaches, such as Lugg's and Rothbart's, are likely to be rejected for smacking of dogmatism by those inclined towards empiricism. Can we really declare some theory untestworthy in an apriori manner? Is not empirical confirmation or disconfirmation the final arbiter of a theory? For example, Thagard [1988, p. 170] generously admits that, despite all the previous failures of astrology, future studies might find empirical support for astrology, although he takes that to be rather unlikely. By contrast, Kanitscheider [1991] maintains that there can be no such evidential support, because astrology is so defective theoretically that, even if there were strong empirical correlations between the star positions and human character and fortune, it could never explain these data by way of mechanisms that do not involve sheer magic. In other words, the empirical situation is irrelevant if the theory in question cannot even begin to explain the data at hand.

Derksen [1993] rejects the idea that it is theories, practices, or entire fields that are pseudoscientific. Instead he recommends examining the attitude or the pretensions of the individual pseudoscientist. After all, it is not a field that can have scientific pretensions, but only its practitioners, and only the latter can be blamed for not making good on these pretensions. Similarly, Kitcher [1993, p. 196] holds that "[t]he category of pseudoscientists is a psychological category. The derivative category of pseudosciences is derivatively psychological, not logical as philosophers have traditionally supposed. Pseudoscientists are those whose psychological lives are configured in a particular way. Pseudoscience is just what these people do." Whereas Kitcher has in mind the inflexible epistemic performance of American creationists, Derksen's analysis concerns the work of Freud. In his analysis Derksen [1993] lists seven attitudinal sins of the pseudoscientist. The first is the "dearth of decent evidence". Having scientific pretensions, the pseudoscientist will have to show respect for empirical evidence. But what he claims to be good evidence for his theory is in fact defective. For example, it is unclear how reliable Freud's clinical data are, because he did not ensure that they were not the result of his own suggestive questioning. (See also [Grünbaum, 1984].) The second sin consists in "unfounded immunizations", which result from selecting and tailoring the data until they fit the given theory; in other words, only particular interpretations of the data are accepted. This also happens in science but, there, immunization is based on well-confirmed theories rather than on unfounded ad hoc hypotheses.

Derksen calls the third sin the "ur-temptation of spectacular coincidence", which consists in ascribing a deeper significance to *prima facie* spectacular coincidences. The fourth sin is the application of a "magic method". That is to say, the pseudoscientist always has some magic method at hand by means of which he can generate all the data he needs. With regard to Freud, Derksen mentions the method of free association, the analysis of symbols and the interpretation of dreams, by which Freud was able to get any data he needed to support his ideas.

The fifth sin is the "insight of the initiate". This is not the claim that only the person with a specialized training can do proper research, since this holds also for science. Rather, it is the claim that the researcher has to overcome certain impediments and prejudices in order to be able to gain the knowledge and insight to be had in the given field. Thus, only the Freudian who underwent psychoanalysis himself is said to be able to practice psychoanalysis.

The sixth sin refers to the presence of an "all-explaining theory", i.e., "a theory that has ready answers to whatever happens". The seventh sin, finally, consists in "uncritical and excessive pretension". Here, "excessive" refers to the fact that, first, the pseudoscientist claims a much greater reliability of his knowledge than allowed for by the evidence (or rather the lack of evidence), and, second, that his pretensions concerning the importance of his theory are far too great. In a later paper, Derksen [2001] elaborates on these sins, offering seven further strategies typical of the "sophisticated pseudoscientist". In any case, although Derksen is right that, strictly speaking, only a person can have scientific pretensions, it seems rather unproblematic to abstract from these individual "sinful attitudes" and treat them as methodological rules, as is commonly done. The same holds in my view for Kitcher's [1993] psychologistic approach.

In a complex study of scientific research, which can be summarized only in a rather simplified way, Kuipers [2001, p. 247] defines pseudoscience as the combination of scientific pretensions and the neglect of the "principle of improvement of theories". The latter enjoins us to aim at more successful theories by eliminating the less successful ones. This improvement is supposed to occur within a research program (in the broad sense), i.e., we aim at better theories while keeping the hard core of the program intact. Only if this strategy fails should we try to adapt the hard core; and only if this strategy fails too, should we look out for a new research program. According to Kuipers, these rules may be seen as constituting *scientific* (or *methodological*) *dogmatism*. By contrast, *unscientific dogmatism* is characterized by the strict adherence to one or more central dogmas which are deemed to be in no need of improvement.

Although these authors do not quite agree on the characterization of pseudoscience, they provide important indicators of pseudoscientificity, useful for any analysis of any theory, practice, or field suspected of being pseudoscientific.

5.2 Pseudoscience or Parascience?

There is a fundamental problem, however, with the very definition of the term "pseudoscience". If it is an essential connotation of "pseudoscience" that it be a nonscientific field *with scientific pretensions*, what do we do with nonscientific fields that appear to be as defective as the classic pseudosciences, but do not claim to be scientific in the first place? As Hansson [1996] has rightly pointed out, many fields that are often subsumed under the label "pseudoscience" are not really such. Indeed, many areas in the vast realm of esoterics, occultism and New Age thinking do not pretend to be scientific at all. Some are even outright antiscientific: they reject the scientific approach to knowledge in favor of various "alternative ways of knowing". If not as completely wrong, the scientific world view is regarded at best as short-sighted and hence in dire need of "complementary" forms of cognition, such as "holistic", "spiritual" or "mystical" ones. Examples of such fields are various forms of "alternative healing" such as shamanism, or esoteric world views like anthroposophy (for further examples see [Carroll, 2003; Hines, 2003]; as well as the various articles in [Stalker and Glymour, 1989]). Obviously, the standard definition of a pseudoscience as a nonscientific field with scientific pretensions does not apply to such areas. Yet these esoteric fields do compete with science in claiming to produce, or have at their disposal, important factual knowledge that the "narrow-minded" scientific approach necessarily must overlook. Moreover, the alleged knowledge produced in these areas often collides head-on with well-confirmed scientific knowledge. For this reason, we must suspect that the "alternative knowledge" produced in such fields is just as illusory as that of the standard pseudosciences.

For these reasons it will be useful to have a different term which subsumes both the pseudosciences proper and all the other fields producing bogus knowledge. I suggest using the term *parascience* for this purpose. Note, though, that the term "parascience" is often used in a different sense, namely descriptively for a field of knowledge whose status as either a pseudoscience or a protoscience is still under debate. I shall disregard this descriptive usage here in favor of the normative one. Alternatively, we could as well give up the standard meaning of "pseudoscience" as a nonscientific field with scientific pretensions and conceive it in a broader sense to also cover all those areas dealing with bogus knowledge.

However, I shall stick here to the name "parascience", because it allows us to explore further distinctions, which are usually neglected in the demarcation literature. Thus, as a matter of principle, we can not only distinguish science from pseudoscience, but also pseudotechnology from paratechnics, and pseudohumanities from parahumanities. Recalling our earlier distinction between technology and technics, a pseudotechnology then would be a technological field based on some pseudoscience, whereas a paratechnic would just be a crackpot technic without any elaborate pseudoscientific background, or at most with a traditional magical background theory. A pseudohumanistic field would be one pretending to produce humanistic knowledge, although its business actually consists in sheer intellectual imposture or obscurantism. And a parahumanistic field, finally, would be the same, except for the fact that it does not pretend to be a field which should belong in the circle of the humanities. Finally, there is a category which contains all those fields that are neither pseudoscientific nor pseudo- or paratechnological nor pseudo- or parahumanistic. We have no choice but to call them parasciences in the narrow sense, in contradistinction to parascience in the broad sense as defined above (see Fig. 4). Having two notions of parascience is one of the disadvantages of the present analysis.

To see whether this extended typology is of any use, let us take a look at some examples. Considering these examples here does not imply that all of them

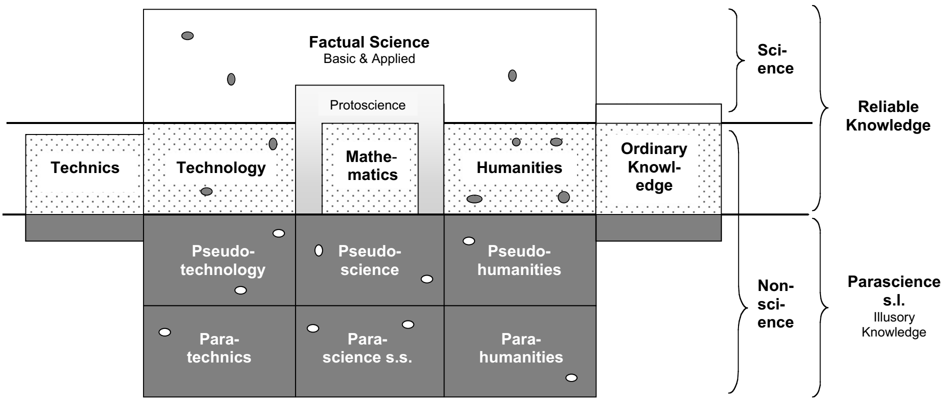

Figure 4. An extended typology of epistemic fields. In this typology only the basic and applied factual sciences are considered as strictly scientific, whereas technology, mathematics and the humanities are classed as nonscientific fields, though still close to the factual sciences. In any case they belong to the class of epistemic fields providing reliable knowledge. By contrast, the knowledge claims of the parasciences (*sensu lato*) are illusory: they do not enrich human knowledge, but pollute it. Protosciences are epistemic fields shading from the dubious into the scientific. The light gray shading indicates that by and large they are on the right track, although they are still burdened with nonscientific ideas or procedures. Ordinary (or everyday) knowledge and technics also lie in between the reliable and the mistaken. Note the gray spots on science's bright vest and the white spots on the dark attire of the parasciences. This indicates that the science/parascience distinction is not really a clear-cut black and white demarcation line, as suggested by this idealized diagram. There are pseudoscientific pockets within otherwise good sciences. These are sometimes labeled *pathological science*. And of course, some knowledge produced in science, technology, and the humanities has turned out to be false (without therefore being pseudoscientific), and not all knowledge in the parasciences need be false. Further explications in the text.

are correctly placed in the proposed category. Some of them certainly are, but the status of others is still under debate, so we may prefer to call them parascience *candidates*. Standard examples of pseudoscientific theories or fields are parapsychology, scientific creationism and intelligent design, psychoanalysis (as basic psychological theory), astrology (as a theory of human character), cryptozoology, Lyssenkoism, New Age physics, ufology, Däniken's archeology, Afrocentric history, and Sheldrake's morphogenetic fields theory (see [Shermer, 1997; 2002; Carroll, 2003; Hines, 2003]). A more recent suspect is the constructivist-relativist sociology of science [Gross and Levitt, 1994; Sokal and Bricmont, 1998; Bunge, 1999; Wilson, 2000]. All these fields pretend to be scientific, e.g., in using scientific methods.

By contrast, a parascience (in the narrow sense) does not claim to be scientific: it is just a field involving some (often traditional) theory about certain matters of facts. For example, traditional Chinese medicine involves a "biological" theory of the life energy *qi* flowing in meridians through the human body. The Indian theory of chakras asserts that the human body contains thousands of energy centers (chakras), which may be influenced by meditation (e.g., tantra). Similarly, the Western esoteric theory of reincarnation states that a personal soul really survives the body's death and can be reborn in some other body. (Note that the traditional Buddhist concept of reincarnation does not involve the survival of some spiritual substance.)

As for pseudotechnology, recall from section 4.3.2 that technology does not just consist of the classic physico-mechanical or engineering disciplines, but also of biological, psychological, and social technologies. All the fields attempting to come up with perpetua mobilia and other so-called free energy machines, with antigravitation devices and earth ray protection gizmos, count as pseudo-physicotechnologies. Likewise, sophisticated dowsing, which is based on pseudogeological assumptions, and water energizing on the basis of "quantum transformation" or other bogus concepts belong in pseudo-physicotechnology.

Examples of bio-medical pseudotechnologies are homeopathy, chiropractic, iridology, and biorhythmology. Candidates for psychological pseudotechnologies are psychoanalytical therapy, phrenological and graphological diagnosis, astrotherapy and horoscopes, neurolinguistic programming, and applied kinesiology. Finally, as pseudo-sociotechnologies have been regarded: Marxism as scientific socialism [Popper, 1959] as well as feminist technology and the so-called New Evidence Scholarship relying on subjective probabilities in jurisprudence [Bunge, 1999]. By contrast, mere paratechnics, i.e., procedures not based on some pseudoscience but at best on some parascience (in the narrow sense), are naive dowsing, faith healing, magic, voodoo, and prophetic techniques such as palmistry, Tarot, and I Ging.

What about pseudo- and parahumanities? Are there any examples at all? In section 4.3.3 we listed only some of the major differences between the humanities and the factual sciences. Since this does not constitute a positive and comprehensive characterization of the humanities, it does not enable us to demarcate genuine humanities from pseudo- and parahumanities. Thus, the following examples merely give some possible suspects, not the results of a detailed analysis. As pseudohumanities have been regarded: anthroposophy, theology, irrationalist philosophy (pseudophilosophy), and postmodernist cultural studies. Scientology may be another candidate. Parahumanities on the other hand might be hermetics, gnosticism, mysticism, and maybe traditional religions inasmuch as they make cognitive claims. These examples show the highly controversial nature of demarcating pseudo- and parahumanities. Even if this demarcation proves to be untenable or useless, it should at least provoke a detailed examination of the suspects involved before admitting them into the humanities or else refusing them entry.

Indeed, only few authors (e.g., [Kuipers, 2001]) have dared ask the question of whether, for example, theology is a pseudoscience, and whether there is such a thing as pseudophilosophy. Whereas Kuipers does not give an answer with respect to theology in his 2001 (see, however, [Kuipers, 2004]), he suggests that pseudophilosophy is the combination of philosophic pretensions with unscientific dogmatism. Philosophy reducing to nothing but exegesis, or the attempt to preserve the teachings of some master instead of developing and improving on them, would be examples of pseudophilosophy. Another example, not mentioned by Kuipers, could be irrationalist philosophy. For example, it is well known that Schopenhauer and many others accused Hegel of being a pseudophilosopher for writing utter nonsense, and the positivists, the critical rationalists and others have criticized some of the German philosophical tradition (e.g., Heidegger) for being obscurantist (see, e.g., [Albert, 1985; Edwards, 2004]). And recently, the French deconstructionists and others have been accused of being intellectual impostors [Sokal and Bricmont, 1998]. Be this as it may, if there is pseudophilosophy, it will be a pseudohumanistic field rather than a pseudoscientific one.

Theology is somewhat different, because the work of theologians ranges from the social sciences to the humanities. While working, for instance, in the field of comparative religion, text analysis, or sociology of religion, theologians do proper scientific and humanistic work — *de facto* and as individual researchers. Hence their individual work need not differ from religious studies or comparative religion (*Religionswissenschaft*), which can just as well be done by nontheologians. Presumably, the main problem with theology is institutional, because theology is by its very essence denominational: the theologian is the representative of some particular religion and is therefore expected to accept its creed as a given. The core of this belief system is not open to revision as a matter of principle, wherefore it must be regarded as a form of unscientific dogmatism. Thus, it is impossible that, as a result of internal progress in research, Christian theology will come to the conclusion that Christianity is actually false and Hinduism is true after all. For example, in the past 200 years the research of many theologians has contributed to demolishing the authority of the scriptures by putting them in a proper historical perspective, but this has not led them to abandon Christianity. Rather, it has spawned a hermeneutic industry of apologetics, attempting to save the Christian faith by reinterpreting and re-reinterpreting its tenets, often in unintelligible terms [Albert, 1985, Ch. 5]. Of course, the individual theologian may eventually change his mind and give up his belief, adopting another one or even becoming an atheist. But, unless he gets fired upon so doing, he has to leave his field if he wants to be consistent. Thus, it seems that, due to its fundamentally denominational and dogmatic nature, theology as an epistemic field is pseudoscientific or pseudohumanistic, respectively.

What about pathological science? In which category does it belong, or is it a category of its own? As mentioned in the legend of Fig. 4, pathological science concerns pockets or niches of pseudoscience still located within the sphere of science. In Fig. 4, this is indicated by the dark spots marring the field of science. Classic examples are the N-rays and polywater affairs. More recently cold fusion has been added to this list. But other theories and approaches within the sciences too have been regarded as pseudoscientific, such as steady state cosmology, the anthropic principle, the subjectivist interpretation of quantum theory, the quantum theory of measurement, evolutionary psychology, information processing psychology, and the research on race and IQ (see, e.g., [Bunge, 1982; 1983b; 1984; 1999; Shermer, 2002]). Some fields, like holocaust denial, have even somewhat branched off from academic historiography to form a specialized field of their own, which enforces the impression that they have turned into full pseudosciences [Shermer, 1997].

As for the corresponding white spots in the parascientific fields, they indicate that not every piece of knowledge in the parasciences need be false: we may find some true or useful items on occasion. An example is acupuncture. Although there is no hope for the magical theory of traditional Chinese medicine underlying the practice of acupuncture, there is some evidence that putting needles here and there has some effect on relieving certain forms of pain [Ernst *et al.*, 2001]. If this turns out to be true, acupuncture will become an area of biomedical research and explanation, which most likely will not have much in common with its parascientific origins. Finally, some parasciences, such as parapsychology, do use scientific methods for example, so that not everything occurring in an overall parascientific field need be unscientific.

So much for some possible examples illustrating the distinctions suggested in Fig. 4, and some qualifications concerning the idealizations involved. The purpose of this extensive typology is to show that in its standard definition the label "pseudoscience" fails to do justice to the wide variety of the parasciences. On the other hand, if we are only interested in distinguishing the genuine article from bunk, a simpler analysis will of course do, such as the one depicted in Fig. 3, in which, however, one might want to replace the terms "pseudoscience" and "pseudotechnology" by "parascience" and "paratechnology", respectively.

Having dealt with various parascience suspects, let us proceed at last with the characterization of parascience.

5.3 *Characterizing Parascientific Fields*

In the following analysis we shall try to develop a profile of parascience (in the broad sense) by applying the twelve criteria of scientificity listed in Sect. 4.2.

1. *Community C.* Faced with a parascience candidate, we need to examine whether there is in fact a real research community continuing a research tradition, or just a loose collection of individuals. If there really is a genuine system of persons, we need to check further whether this community engages in research, or whether it is just a group of believers.

   One of the few parasciences that does have a research community is parapsychology. Many others, by contrast, are belief communities: there is a single guru or a small number of authorities, surrounded by a more or less numerous crowd of followers, who do not engage in research, but at most in exegesis or application. Think of Immanuel Velikovsky's pseudocosmology, Erich von Däniken's pseudoarcheology and pseudohistory, Charles Berlitz's Bermuda triangle mystery, or Ron Hubbard's scientology.

2. *Society S.* The society hosting a community of researchers or else believers must at least tolerate its activities. However, political power can turn an epistemic field into a pseudoscience if it starts to proscribe what is to be accepted as true knowledge and what not, and if the people working in that field follow suit. Examples are *Deutsche Physik* (German physics) or, more generally, Aryan science in the Third Reich, and Lyssenkoism during the time of Stalinism and after. A contemporary example is creationism, which is adopted at the national level in official theocracies, or at least pushed at the regional or local level where conservative churches or fundamentalist religious groups of any color wield enough power (e.g., in Turkey, Iran, the US, and Russia). In the same vein, it is legitimate to ask whether the calls for a feminist science, based on the relativist-sociological "finding" that science is just an enterprise of white Western males, belong in the same category [Gross and Levitt, 1994; Bunge, 1999]. It may well be that women have somewhat different research interests, so that they focus on different problems. But as soon as we get to questions of method, testing, validity, and justification, there seems to be no leeway for "alternative" forms of science.

3. *Domain D.* The domains of parascientific fields often comprise dubious and ill-defined items, such as mysterious energies or vibrations, which have so far escaped detection. In other words, many parasciences still have to prove that the objects and processes they refer to in their discourse do exist really. Therefore, much of their domain is factually empty and consists mostly of speculative entities. An example is parapsychology, which has not been able to come up with a single unambiguous finding concerning the real existence of "psi" [Alcock, 2003; Hines, 2003].

   At first sight, hypothesizing unobserved or unobservable entities appears to be analogous to the theoretical entities posited in many scientific fields.

However, the difference is ontological, semantical and methodological: if not supernatural, the entities posited in many parascientific fields are by definition paranormal or, if preferred, paranatural, and they are often idle, arbitrary, or nonparsimonious, for not being embedded in some explanatory theory proper. Hence they are often ill-defined, i.e., they are so vague that it is unclear what is being tested — if there are serious tests at all. An example is the mysterious "psi" occurring in parapsychology, which is defined but negatively [Alcock, 2003]. For example, precognition is defined as seeing future events in a way that *cannot* be explained by contemporary science. Likewise, psychokinesis and telepathy involve interactions that *cannot* be accounted for by any mechanisms known to normal science. Moreover, parascientific entities are not hypothesized in a search for the best explanation (i.e., abductively, as it is often called), but they are often objects of prior beliefs, for which a justification is sought only if the belief is questioned by some skeptic. So whatever *prima facie* explanatory function they may have, the very same function could often be exerted by any other paranatural entity. In other words, paranatural entities are usually not specific enough for a satisfactory explanation (see, e.g., [Flew, 1990; Kanitscheider, 1991; Humphrey, 1999]).

4. *Philosophical background G.*

   (a) *Ontology.* The ontological aspects of parascience are often neglected in favor of its methodological problems. An early exception was the philosopher Charlie D. Broad, who was a firm believer in parapsychology. He pointed out that both science and our everyday practice presuppose various philosophical assumptions, which he called "basic limiting principles" [Broad, 1949]. He gave four main examples, three of which are ontological, one epistemological. His ontological principles were (i) the antecedence principle (effects cannot precede their causes); (ii) mind cannot directly act on matter without involving a brain event; and, conversely, (iii) the mind depends on the brain, i.e., a necessary condition of any mental event is an event in the brain of a living body. An epistemological consequence is (iv) that our ways of acquiring factual knowledge are limited to sensory experiences, i.e., a physical event does not directly act on our mind, but only through some intermediate events in our sensory organs and finally in our brain. (Note that (ii) and (iii) sound dualist — Broad was sympathetic to epiphenomenalism — but may be reformulated so as to be compatible with monistic mind-body theories.) Since he maintained that the existence of the various parapsychological phenomena like telepathy and precognition was established beyond doubt, Broad concluded that these basic limiting principles of science are refuted.

   The fact that some of the research Broad referred to was later shown to be fraudulent [Ludwig, 1978; Kurtz, 1985; Hines, 2003], and that sophisticated parapsychologists try to conceive of telepathy and precognition in a somewhat different manner, so as to retain at least *prima facie* a naturalist interpretation [Duran, 1990], does not invalidate this as a useful example of the ontological problems faced by most parasciences. Indeed, many of their claims can only be upheld by giving up basic ontological convictions, which have so far proven to be extraordinarily fruitful for scientific research.

The most radical departure from the ontological paradigm of factual science is the open supernaturalism espoused by creationism. Inasmuch as creationism stipulates a *creatio ex nihilo*, it also violates Lucretius's principle. It is unclear whether or not many other parascientific claims can be accommodated within ontological naturalism. In any case, they still violate much of what we know about the lawful behavior of things. Homeopaths, for example, claim that high dilutions that no longer contain even a single molecule of the given substance still have a potent pharmacological effect. If what we know about chemistry is roughly true, there can be no such effect. Homeopaths have learned to concede this objection, but now forward the protective hypothesis that, in the mandatory process of shaking the dilutions (called "dynamization"), somehow the relevant "information" of the given substance gets transferred to the solvent. So what produces the therapeutic effect is this "information". It goes without saying that this supposed information is ill-defined and perhaps even immaterial, because water chemistry tells us that any molecular structure formed by H_2O-clusters is too short-lived to do any informational work. Moreover, if water (or alcohol or whatever fluid) had a memory, why would it specifically remember only the information of the homeopathic substance rather than that of all the other chemicals it had contained previously?

Another example is Therapeutic Touch. By moving her hands about 10cm over the patient's body, the healer attempts to adjust the patient's "vital energy", whose "imbalance" is always among the causes of whatever disease is to be healed. Needless to say, biology has abandoned any idea of vital energies long ago.

These examples show that many of the ideas occurring in the parasciences and paratechnologies are not necessarily supernatural in the traditional sense of involving powerful personal entities like gods or demons, but nevertheless *paranatural* [Kurtz, 2000], in the sense that they are not compatible with the naturalist-materialist outlook of the factual sciences. If we enrich this standard naturalism with more and more paranatural elements, it remains unclear, when this results in destroying it altogether.

The only ontological principle that is rarely violated by the parasciences is ontological realism. Even the weirdest entities occurring in the domain of the parasciences are deemed to exist really after all. The same holds for epistemological realism, which is why we proceed with a look at the methodological principles in the following subsection.

These examples illustrate that the parasciences not only suffer from the methodological problem of lacking evidential support, but also from their incompatibility with the major metaphysical background assumptions, which belong to the general hard core — the hard hard core, so to speak — common to any scientific approach. (For an analysis of the ontological presuppositions of esoterics see [Runggaldier, 1996].)

(b) *Methodology.* It is rather obvious that both Ockham's razor and fallibilism are widely neglected in the parasciences. Indeed, many parasciences populate the universe with (often occult) entities that are not needed for a scientific explanation of the world around us. Examples are the many life or other energies and forces postulated by quack medicine and pseudophysics. Dowsers believe that there are not only earth rays, but that these also occur in certain grids, which can be measured and mapped. And occultism teems with ghosts and spirits. There is no indication that the nature or the number of such entities is restricted by considerations of parsimony in hypothetico-deductive reasoning: their only restriction seems to be due to the limits of their authors' imaginative powers. This is not to say that they serve no explanatory function: they certainly do. The point is, as mentioned earlier, that almost any other arbitrary alternative or additional entity would do just as well.

As for fallibilism, it too is evident that most parascientists are not willing to seriously consider the possibility that they may be in error. If we extend Settle's [1971, p. 185] diagnosis of magic to the parasciences in general, we might say that many parasciences are explanatorily complete and thus come with the air of certainty, whereas factual science is explanatorily incomplete and thus accompanied by corrigibility. This difference helps to explain why the former are so much more appealing to many than the latter. Obviously, an explanatorily complete field has no need for research and hence for improvement, let alone revision (see Kuiper's [2001] definition of pseudoscience mentioned above). As we shall see later on again, some parascientific fields do allow for some limited improvement, such as parapsychology and astrology. However, these changes are not due to an internal tradition of fallibilism, but they are the result of massive external criticism by mainstream scientists.

(c) *Semantics.* As a truth definition the correspondence notion of truth, being simply a companion of ontological realism, is adopted in most parascientific fields. The major difference between science and parascience lies in the question of what is acceptable as truth indicators. Now this belongs in methodology, not semantics, so it may suffice here to add that, beside the main question of what can be regarded as legitimate objective evidence, the parasciences often accept as indicators of truth also subjective "evidence", such as sheer belief or feeling, mystical vision, or other paranatural forms of experience.

(d) *Axiological and moral assumptions.* Different values manifest themselves in different behaviors of the individuals adopting these values. Thus, as mentioned in Section 5.1, Derksen [1993] has suggested analyzing the behavior and attitudes of the individual pseudoscientist, and Kitcher [1993] has recommended focusing on the psychology of the pseudoscientist. However enlightening this may be in some cases, in particular when taking a closer look at the founding father (or mother as the case may be) of some field, as Derksen did with Freud, it does not suffice to characterize the entire epistemic field. For example, it is possible for an individual to behave rationally within a magical belief system [Settle, 1971], whereas an individual scientist working in a rational tradition may on occasion behave irrationally. For this reason we better focus on the *institutional rationality*, or irrationality respectively, exhibited by the community C of some epistemic field, which is done best by examining the latter's general ethos or value system.

- *Logical values.* The canon of valid reasoning and thus the basic principles of rationality may be accepted officially, but they can be suspended whenever needed to save some claim. Lots of logical blunders occurring in the parasciences have been collected by various authors (see, e.g., [Schick and Vaughn, 1999; Wilson, 2000]). Since many of these occur in the context of justification, we shall give a sample in the subsection on methodological values.
- *Semantical values.* Meaning definiteness and clarity are rarely among the semantical values of the parasciences. Instead, vagueness and fuzziness are rampant, if not even seen as virtues by those cherishing the mysterious. We must also be prepared to encounter the meaningless, i.e., nonsense. (Note that scientists often are too quick in calling something nonsense, just because it is false. However, something that is false cannot be nonsense, because nonsense can be neither true nor false, for it has no semantic meaning in the first place.) Regrettably, since for most laymen many scientific theories are more or less incomprehensible, unintelligibility on the part of a parascientific theory may easily be mistaken for a sign of an authentic science.
- *Methodological values.* Many parasciences are characterized by methodological values and hence procedures of their own. These consist, for example, in certain rules of inference or rules of evaluating evidence which are quite often regarded as fallacious by philosophers of science. For this reason they either have been eliminated from science, or, if they occasionally reappear in some reasoning, are quickly detected and denounced as mistakes by the scientific community. Indeed, fallacious methods were described already by

19th century philosophers of science like Mill and Peirce, and many modern authors who attempted to demarcate pseudoscience by its peculiar inferential methods, have collected various fallacies as indicators of pseudoscientificity (e.g., [Radner and Radner, 1982; Giere, 1984; Thagard, 1988; Schick and Vaughn, 1999; Wilson, 2000]). Since these fallacies do constitute important parascience indicators, a quick sample will be in order.

The a priori method: Accept only those beliefs that are such that it is impossible to imagine that the contrary is true [Wilson, 2000]. In other words, a hypothesis is accepted and considered worthy of use for explanation not on the basis of empirical evidence, but because its proponents regard alternatives as inconceivable. Examples: von Däniken keeps repeating that he simply cannot imagine how some artifact could have been produced by ancient man without extraterrestrial help. The creationists (including the more recent branch of Intelligent Design) keep repeating that it is inconceivable how the natural process of evolution could have produced certain complex organs without divine design or even intervention.

The fallacy of competition: This is the claim that some parascientific theory should be admitted because it might become an alternative theory in the future. Yet, as Radner and Radner [1982] point out, competition is only among current alternatives: by referring to some unknown future science, one actually refuses to compete. Their very apt analogy is the attempt to participate in a marathon on roller skates, arguing that the marathon might be changed to a skating race in the future.

Simplistic elimination [Giere, 1984; Wilson, 2000]: Assuming there are two rival theories A and B, and they are the only possible alternatives, we may infer that A is true if B is false. Yet in reality there usually are many possible alternative theories that might explain the same fact. So if we are faced with two or more alternative theories, we must first make sure that they really are the only alternatives, and that they are not false all together. Thus, many supposed eliminations are fallacious, because they do not consider all possible alternatives. The creationists argue, for instance, that there are only two alternatives: evolutionary theory and the theory of divine creation. But if evolutionary theory, including all we know about the history of the universe, is false, then divine creation is not the only remaining alternative: it may well be then that life is coeval with an uncreated eternal universe. Ufologists argue that, since some strange sightings cannot be explained by the usual candidates such as satellites, balloons, aircraft, or bright planets, they must be due to extraterrestrial visitors. Yet there may also be unknown natural atmospheric processes causing a given UFO-sighting.

Anything-goes method [Wilson, 2000]: This is the argument that, since even a well-confirmed theory might possibly be false, we should not dismiss alternatives to it. So everything goes. If this were correct, the corollary would be that in fact nothing goes, because these supposed alternatives might likewise be false.

Method of authority [Wilson, 2000]: As pointed out earlier, many parasciences are belief systems rather than research fields. It comes as no surprise therefore that a rule "to accept as true what the relevant authority tells you" is wide-spread. Naturally, this holds in particular for religious or quasi-religious fields such as creationism, scientology, anthroposophy, or transcendental meditation.

Resemblance thinking [Thagard, 1988; Wilson, 2000]: This is the habit, already pointed out by John S. Mill, of inferring from the observation that *A resembles B*, that therefore *A causes B*. Prime examples of fields relying heavily on resemblance thinking are astrology and homeopathy. The latter's "law of similars", stating that like heals like, is even enshrined in the very name "homeo-pathy" (from the Greek *homoios*, similar).

The grab-bag approach to evidence [Radner and Radner, 1982]; see also the *blunderbuss argument* in [Wilson, 2000]): In evaluating the evidential support for some theory, we should not just look at the quantity of confirming instances, but first of all at their quality. Thus, we do not have to keep shooting canon balls in order to confirm the laws of motion. Of particular value, on the other hand, are data that were gathered after a theory had been proposed, and that were possibly even predicted by the theory; likewise with evidence that was produced under a variety of different conditions. Classical examples with regard to Newton's theory are the discovery of Uranus and Neptune, and the prediction of the return of Halley's comet. By contrast, it is typical of many parasciences that the sheer quantity of "evidence" makes up for the lack of quality of the individual data. For example, von Däniken pulls out artifact after artifact in favor of his "alien hypothesis"; the creationists keep listing complex biotic structures which impossibly could have come into existence naturally, i.e., by evolution; and the ufologists will report strange sighting after sighting. Moreover, as soon as one piece of such evidence has been rejected, either for being fallacious or forged, or for having been explained within a standard scientific context, the parascientist will simply continue to pull out data of the same kind and quality from his evidential grab bag, thereby keeping the skeptic busy for all times. Worse, the fact that scientists cannot always readily refute each and every item pulled out of the grab bag, is taken as a further reason for belief in the parascientific tenet in question.

- *Attitudinal values.* The attitudinal value system of the parasciences is as varied as the parasciences themselves. Thus, again, there are no universal features characterizing all the parasciences. Nonetheless, an attitudinal profile of parascience may include the following aspects. Parascientists pretend to be critical thinkers, but their canon of critical thinking is not the same as that of science and philosophy. In fact, many are just believers, not investigators. They also claim to be open-minded, but their open-mindedness does not extend to the possibility that the standard scientific view of nature is the correct one. Instead, it includes sympathy for the most outlandish claims, because to the parascientist open-mindedness often means "anything nonscientific goes", so that it amounts to blank-mindedness. Universalism and objectivity are not values in those fields dominated by authorities, or in which only the initiate has special access to the truth. Think of the various branches of occultism.

5. *Formal background F.* Concerning the formal background of any suspected parascience, we may ask questions such as the following: Are there any mathematical models? Is the mathematics in these models handled correctly? This is often not the case. In particular, in some pseudophysics such as the attempts at refuting the theory of relativity, the mathematics is defective, if not phoney. The same occurs in some social sciences, in particular sociology and economics, where pseudoquantitation may go unnoticed [Sorokin, 1956; Blatt, 1983; Bunge, 1999, Ch. 4]. The latter example illustrates once more that some research fields which on the whole are regarded as scientific may nonetheless exhibit some occasional pseudoscientific feature (Fig. 4).

6. *Specific background knowledge B.* In contrast to scientific fields, which borrow amply from adjacent disciplines, the parasciences are typically isolated enterprises. They presuppose some ordinary knowledge, and of course they borrow some science when needed. But note that the function of the scientific knowledge borrowed consists mostly in justifying the scientific pretensions of the given pseudoscience: it is easier to imitate science when you also use some well-accepted scientific knowledge. The scientific input is often not needed to advance the own field. Note also that the converse input does not obtain: scientific fields have hardly any use for knowledge produced in a parascientific field.

   Astrology, for example, accepts of course some basic astronomic facts, but disregards many others, in particular those that refute its own claims. Creationists rely heavily on biological knowledge, but only to prove the falsity of evolutionary theory. However, no scientific knowledge whatsoever can shed any light on the totally occult mechanism of divine creation. In other words, no scientific knowledge can advance creationist "theory".

The theory probably most often borrowed from the sciences is quantum theory, which has become an explanatory panacea for many parasciences, from New Age physics through parapsychology to holistic medicine [Grove, 1985; Stalker and Glymour, 1989]. For example, sophisticated parapsychologists have long abandoned stories of moving tables and telepathically communicating people. The naturalistically oriented part of current parapsychology claims that paranormal effects are microeffects rather than macroeffects, and that they can be accounted for by quantum theory. Telepathy, for instance, is no longer seen as a form of human communication, but at most as an instance of nonlocal correlations between some quantum events in two peoples' brains, or between a person's brain and some other object like a random number generator. It will come as no surprise then that the use of quantum theory in the parasciences often involves a serious distortion, in particular a return to long abandoned subjectivist interpretations. Moreover, one often uses the vocabulary of quantum theory but rarely its concepts [Stenger, 1995; Spector, 1999]. In sum, the motto is: if you don't know what it is and how it works, call in quantum theory to describe and explain it.

Note, incidentally, that in sophisticated parapsychology this move is due to the attempt to stay within the bounds of a naturalist ontology. At the same time, it presupposes a radically reductionist view, because it disregards the level structure of the world, i.e., the fact that macroobjects such as neural assemblages have systemic properties, so that their behavior is usually not influenced by microevents occurring at the quantum level. For example, neuroscientists know that mental processes, such as perception and thinking in general, involve millions, if not billions, of complexly interacting neurons and their coordinated activities at different organizational levels. The idea that quantum events occurring at the level of elementary particles or at most atoms should be able to influence these highly complex neuronal systems in a coordinated manner is extremely implausible [Beyerstein, 1987; Humphrey, 1999; Kirkland, 2000].

Parasciences sometimes also borrow ideas from other parasciences. A prime example is Carl G. Jung's concept of synchronicity, which is made use of both in sophisticated astrology and parapsychology. This is the idea that two events which have no causal connection are nonetheless "meaningfully" related (McGowan 1994; Carroll 2003; Hines 2003). Thus, if the quantum physical notion of nonlocal correlation cannot be called in as an ad hoc device to establish a connection between two (simultaneous) events, because what we have is just a coincidence, synchronicity will do the trick. For example, sophisticated astrologers have learned from the many scientific objections hurled at them during the past centuries: they nowadays admit frankly that the relation between humans and the various constellations of stars and planets is not a causal one. What saves the business though is the claim that the relation between the stars and humans is nonetheless a meaningful one, namely an instance of synchronicity. This neo-astrology then finds and interprets these meanings and explains them to its customers, turning the field into a form of astro-counseling. Note that this strategy is clearly ad hoc: it is not due to internal progress in astrology but a move to avert external criticism, making astrology immune against the standard astronomic objections without having to give up the "astro" in "astrology".

7. *Problematics P.* In the parasciences the collection of problems is usually small and mostly practical, for many parasciences are actually paratechnologies or paratechnics. Important questions about any parascience candidate are: Does it solve or help to solve problems other than its own? Do its problems arise from natural contexts, or are they artificial (fabricated)? Three examples might illustrate this problem concerning parascientific problems.

   Astrology mostly solves problems that would not exist without astrology in the first place. The only general and natural question that astrology tries to answer, namely the question why different people have different characters, is better answered by genetics, developmental psychology, and sociology. Moreover, the astrological answer is incompatible with the scientific one and thus does not enrich scientific knowledge. For the most part, however, astrology is a pseudotechnology, which has rules to apply, but no puzzles to solve [Kuhn, 1970]. In particular, the many failures of astrological predictions do not entice any problem-solving activity in the astrological community.

   The problems of von Däniken's pseudoarcheology too are fabricated rather than natural, because he preys on the natural problems of normal archeology and turns them into mysteries, which he claims can only be solved by his hypothesis about extraterrestrial visitors. Thus, von Däniken's hypothesis does not yield any new problems on its own: it is entirely parasitic on the pre-existing problems in other fields.

   Parapsychology started out with the natural problem of unusual human experiences, in particular at a time when spiritualism was *en vogue*. Some people sometimes do have anomalous (though nonpathological) experiences. The basic question therefore is whether all such anomalous experiences can be explained naturally (i.e., within the normal paradigm of scientific biopsychosociology), or whether we do need to enrich this paradigm with paranormal entities and processes to account for these unusual experiences. Yet, the more successful the normal sciences, including in more recent times the neurosciences, became in explaining anomalous experiences, the less needed were explanations referring to paranormal entities or processes. In this way, parapsychology practically lost its source of spontaneous or natural problems, although people keep experiencing unusual things. Not willing to give up the psi hypothesis in favor of the null hypothesis, parapsychologists started to fabricate new problems: they began studying arbitrary correlations between human subjects and virtually every possible other object, desperately looking for statistically significant deviations from chance expectation (i.e., anomalies), which can then be interpreted as evidence for psi. Since all the results from such — often quite sophisticated — studies are, if not negative, at best inconclusive, the consequence is the perpetual call for further research. Thus, parapsychology generates arbitrary problems of the sort "Could there be an anomalous correlation between x and y_1 or y_2 or $\ldots y_n$?" in order to keep itself alive. As Alcock [2003, p. 34] observes, the anomalies parapsychologists search for have never popped up in normal research. Thus, again, the contemporary problems of (sophisticated) parapsychology would not exist if it were not for the existence of parapsychology itself.

This may be the place to take a brief look at the role of anomalies in science and parascience. Normal scientists do not look for anomalies, they "hit them in the face" [Radner and Radner, 1982, p. 33; Alcock, 2003]. Indeed, every scientist who performs some measurement or experiment has certain expectations as to its outcome, in particular if the outcome is predicted by some theory. If the resulting data seriously deviate from these expectations, they constitute an anomaly. Although it takes more than just a few anomalies to initiate a scientific revolution, the importance of anomalies for theory change and hence scientific progress has been well known and discussed ever since the work of Kuhn [1962]. However, scientists are conservative in the sense that they will not give up an otherwise well-confirmed theory, let alone an entire research program, in favor of some alternative theory whose only merit is its ability to explain a certain anomaly. On top of explaining the given anomaly, the new rival theory must at least explain as much as does the standard theory.

By contrast, parascientists rejoice when they find anomalies. Their expectations are not those of an orderly and lawful world, but of a world teeming with mysteries. Therefore, they actively search for anomalies, which they can then turn into problems to be solved by their respective "alternative" theories. And these alternative theories are expected to revolutionize science. In so hoping, parascientists forget that no scientific revolution has ever been triggered from without. Nonetheless, there is even a field or rather a multi-field called *anomalistics*, which is exclusively devoted to the study of anomalies supposedly neglected by mainstream science. The main player in this field is the *Society of Scientific Exploration*.

8. *Fund of knowledge K*. The fund of knowledge of a parascience is not a growing collection of up-to-date and well-confirmed data and theories: it is usually small, it stagnates, it contains statements that are incompatible with well-confirmed scientific knowledge, and its hypotheses lack evidential support. For this reason, the knowledge in these fields is purely speculative and cannot be said to even approximate the truth, i.e., to roughly represent any real facts.

   A frequent feature of parascientific knowledge is its anachronistic character [Radner and Radner, 1982]. What many parascientists propagate as revolutionary new insights or at least as rival "scientific" theories is in fact very old news, so old indeed that they have long been discarded by science. For example, alternative medicine teems with mysterious vital energies that supposedly are out of balance when we are sick. Thus, the basic ideas of homeopathy only make sense when we go back 200 years when vitalism was still going strong in biology and medicine. Traditional Chinese medicine presupposes the existence of some vital energy (*qi* or *ch'i*), flowing in channels (meridians) unknown to biology. And the practitioners of therapeutic touch and reiki (*ki* is the Japanese equivalent of *qi*) claim that they treat the imbalances in the "human energy field", whereas the so-called prana healers refer to the Hinduist equivalent *prana*. The creationists still defend views that may have been legitimate 200 years ago. Then there are the pseudophysicists who still try to build perpetua mobilia or other so-called free energy machines as though thermodynamics were nonexistent, or who desperately strive to refute Einstein's two relativities in order to re-establish good old Newtonianism. Finally, astrology is another prime example of a world view that has been superseded for several hundred years.

9. *Aims A*. The aims of the parasciences are sometimes cognitive, but for the most part practical. That is, many parasciences are paratechnics or paratechnologies, such as astrology and alternative medicine. Yet even when the aims appear to be cognitive, the ultimate goal of many parasciences is often anthropocentric and quasi-religious (Alcock 1985), if not explicitly religious as in the case of creationism. *Prima facie* the goals of the creationists, such as the establishment of an alternative cosmology and history, appear to be cognitive rather than practical. But we may suspect that the ultimate goal is in fact personal salvation, which, in the fundamentalist world view, can only be achieved by a consistent way of life according to biblical literalism. Similarly, the spiritualist approach of esoterics wants to establish the multifarious spiritual connections of humans with the rest of the world. Often the ultimate goal is quite explicitly stated: the materialist world view of science is to be replaced with a spiritualist one. For example, one of the main figures in 20th century parapsychology, Joseph Banks Rhine, asserted that "little of the entire value system under which human society has developed would survive the establishment of a thoroughgoing philosophy of physicalism" (Rhine [1954/1978, p. 126]). This exemplifies how the aims of both science and parascience often depend on — conflicting — metaphysical outlooks.

10. *Methodics M*. The empirical methods used in the parasciences often are just as occult as the theoretical background assumptions. For example, an instrumental technique such as a pendulum used to diagnose some disease, presupposes some occult mechanism mediating between the healer and, say, the patient's "life energy". How can this method be checked? Interestingly, it can partly be checked scientifically, but it cannot be checked within the own theoretical system of the given field. In other words, in can partly be

tested externally, but not internally. For example, in a double-blind setup, someone claiming to be able to diagnose some specific disease by simply holding a pendulum over a photo of a patient, is given 25 photos of healthy persons and 25 photos of persons suffering from the given disease (i.e., neither the healer nor the experimenter knows which of the photos belongs in which group, and it is impossible to diagnose the given disease from merely looking at the peoples' faces on the photos). As yet, all experiments of such a kind have had negative results, i.e., the candidate's success rate has never been significantly above chance expectation.

Now this is of course a basic and objective scientific test which only checks whether or not the given technique works (not how it works if it did work in the first place). And it was imposed from the outside, because it does not belong to the methodics of the given parascience. So how can the functioning of the method be checked internally? Unsurprisingly, the healer herself might claim that she is able to check her diagnostic technique with alternative means. She may, for instance, use a dowsing rod, or perhaps just put her hand on the picture. In her normal environment all this will most likely combine with confirmation bias and subjective validation into the belief that her method is successful and reliable. However, as a matter of fact even within the own outlook of such a parascientific approach, the given method cannot be checked by other persons in the field, because her colleagues will not be able to reproduce her diagnosis. Indeed, every other person claiming the same ability will very likely come up with a different diagnosis, provided of course she does not know the earlier diagnosis of her colleague. There may be some overlap in the results due to chance, but by and large the success rate will not differ from mere guessing. In short, many techniques used in the parasciences are not objective in the sense that everyone applying the method will get the same results. This holds *a fortiori* for openly subjective methods like spiritual means of communication or mystical vision. The latter are not even methods in the sense of rule-guided procedures.

By contrast, in their attempt to imitate science, the pseudosciences often do use scientific methods. For example, the statistical methods used in sophisticated parapsychology are sometimes impeccable. Moreover, often even the general scientific method is followed as is obvious from the parapsychological journals. In so doing, many pseudosciences, in particular parapsychology and astrology, often exhibit a naive empiricist view of science: they believe that the application of scientific methods and techniques, including the scientific method as defined above, is sufficient to warrant the scientific status of their field. Indeed, in particular parapsychologists have learned a lot from their critics and have thus improved both their statistical sophistication and the precautions against fraud and self-deception. (Note again that these improvements are largely due to external pressure, not internal progress.) So they believe that what they do is proper science, and they reject the various methodological and other philosophical objections as sheer ideological dogmatism, failing to realize that conceptual criticism is part and parcel of science too.

11. *Systemicity.* The systemicity condition is one of the stronger indicators of parascientificity (recall Reisch's criterion of network demarcation mentioned in Sect. 3). Indeed, parasciences are isolated fields. They do not form a consilient system of knowledge; in particular, they make no contact with normal science. It is precisely because parascientific knowledge must be rejected as unfounded that it cannot enrich scientific knowledge. Moreover, parascientific knowledge often collides head-on with scientific knowledge: if parascientific theories were true, their scientific alternatives including those theories to which they are connected would be false. Thus, many parascientific theories would cause total or global revolutions: the entire edifice of scientific knowledge including the scientific paradigm as a whole would collapse. By contrast, contemporary scientific revolutions, if any, will only be local or regional revolutions, because too many things we have come to know during the past 400 years are reliable and must therefore be at least approximately true. Examples of fields calling for global revolutions are creationism and parapsychology. As for the latter, recall C. D. Broad's basic limiting principles, which underlie all modern science.

12. *Progressiveness.* According to the criterion of progressiveness, the membership of the conditions 5–10 changes, however slowly and meanderingly at times, *as a result of research* in the same field or as a result of research in neighboring disciplines. Obviously, many parascientific fields are plainly stagnant, which can be detected rather easily. This is due to the fact that many of them are not really research fields but instead belief systems.

    But of course, there are also some parasciences in which there is at least some minor change, and there are others which are actually research-oriented, such as parapsychology. Indeed, as mentioned before, research keeps parapsychology busy. However, despite its age of more than 120 years, it has not come up with a single conclusive finding [Kurtz, 1985; Hyman, 1989; Alcock, 2003]. Thus after 120 years it is still a field in search for its domain, and it desperately tries to gather hard data. Nonetheless, it has even produced some theories to explain certain supposedly paranormal events or experiences, respectively. It has also introduced plenty of ad hoc hypotheses to protect itself from criticism. An example is the idea of psi missing. If some experiment yields a score slightly above chance expectation, this is of course regarded as evidence for psi. Likewise, if some trial yields a below chance result, this too is seen as evidence for psi: in this case the subject's psi abilities somehow operate to avoid the target (psi missing). In this way any fluctuation around the exact chance expectation becomes evidence for psi. Given this situation, it seems that parapsychology is able to generate the appearance of progress, although a closer look reveals that this progress is just as illusory as the very domain of parapsychology. After all, can there be genuine progress when the given field does not even have a real domain?

5.4 Conclusion

We have now listed and examined a number of features characterizing parascientific fields. The features used in this characterization are of course of unequal weight: some are more decisive than others, so that their presence is a stronger indicator of a field's status. For example, a violation of some of the basic limiting principles in G carries more weight than some methodical flaw in M, which may be repairable more easily, provided the practitioners of the field care to. Since the above features are not jointly necessary and sufficient conditions, another open question is how many of these characteristics must at a minimum be present for a field to be parascientific. Insofar as such a condition is a necessary one, such as the logical requirement of noncontradiction, we may reject the given field as irrational on this one count. In most cases, however, a simple characterization of a parascience such as "it's all a matter of X", where X may stand for falsifiability, method, or attitude will not do. Indeed, we ought to be more careful and always attempt to prepare a comprehensive profile of the suspected field. Such a profile should allow us to come to a well-reasoned conclusion as to the scientific or parascientific status of the given field, although every such conclusion will differ in the reasons used as its premises.

The preceding analysis focused on epistemic fields as the central units of demarcation. However, a comprehensive profile of some parascientific epistemic field should also allow us to diagnose smaller units as parascientific, if they are the bearers of one or more characteristic features occurring in the profile. Such smaller units may be theories as systems of statements, which may be inconsistent or circular, or incompatible with the accepted background knowledge; individual hypotheses, which may be logically unfalsifiable; individual methods, which may have long been weeded out from the sciences for being defective; or some behavior or attitude of the representatives of the field, and so on. In this way we are justified in calling a theory, a hypothesis, a method, or a behavior unscientific. This is of particular importance when we are dealing with an epistemic field which we normally regard as scientific. For in such a case the philosopher of science may still detect some unscientific feature and denounce it as being pseudoscientific, calling for its repair or, if impossible, its elimination.

6 PROTOSCIENCE AND HETERODOXY

Calling some theory, approach or entire epistemic field parascientific is a strong and damning verdict. For this reason we must be quite careful in our judgment, which ought to be based on a diligent examination of the suspected theory or field. Now, whereas the philosopher of science may be more careful in such pursuit, scientists are sometimes less careful. Thus, many authors have warned us that the history of science should teach us sobering and humbling lessons concerning the science/pseudoscience demarcation (e.g., Toulmin 1984). First, it has always been too easy a temptation to reject a theory or approach as pseudoscientific just because it is heterodox, or maybe just because we do not like or understand it. Second, some theories that are declared pseudoscientific may actually turn out to be protoscientific, so that their possibly bright future could be endangered by an unfair judgment. Third, there is the historical problem of judging a certain field in retrospect: some field that may be clearly pseudoscientific today, may have been protoscientific at an earlier time and hence in a different scientific landscape.

A prime example is Alfred Wegener's hypothesis of contintental drift, which was initially rejected when proposed in 1915 and sometimes even derided, but eventually became the basis for the plate tectonics revolution in the 1960ies. Wegener's ideas were indeed protoscientific rather than pseudoscientific because he did not refer to untestable myths and mysteries like Velikovsky or von Däniken, but instead to geological and climatological data. And he did not behave like a pseudoscientist, for he admitted that his ideas were conjectural and that the main problem of his hypothesis was the unknown mechanism of continental drift. However, his geological colleagues also acted rationally in rejecting his hypothesis for being too implausible at that time (see [Kitcher, 1982; Radner and Radner, 1982]). Apart from the historical vindication of Wegener's protoscientific ideas, an assessment of Wegener's hypothesis in a pseudoscience profile would most likely have shown that even at their time his views were not pseudoscientific, but merely unorthodox [Edelman, 1988]. This indicates that it is not always true that we can determine the scientific status of a certain theory or field only retrospectively, e.g., by observing its historical progress or else degeneration.

A less favorable example is phrenology, which has been regarded as a protoscience leading to neuropsychology (Young 1970). Phrenology advanced the correct and fruitful idea that mental functions are localized in the brain, but was badly mistaken in the claim that these functions manifest themselves craniologically, i.e., as bulges on the skull. The latter made phrenological diagnosis a pseudotechnology, which, however, had some beneficial side-effects on the treatment of prisoners and the mentally ill [Hines, 2003]. In this case a retrospective analysis shows that a small part of phrenology led to progress, if only in a field that quickly emancipated itself from phrenology, whereas the larger part degenerated into a pseudoscience.

In the case of astrology opinions are divided. Apart from its defenders of course, even some philosophers of science are willing to grant astrology the status of a former protoscience (e.g., [Thagard, 1978]). Others maintain that astrology never was a protoscience, because even in antiquity educated people, like Strabo, Cicero and Ptolemy, clearly distinguished between astronomy and astrology, whether or not they believed in the latter [Culver and Ianna, 1988]. Moreover, it was obvious to many even back then that astrological predictions are unreliable for failing too frequently. And although some early scientists like Kepler practiced some astrology, they too kept it apart from science. Thus, it seems that despite various connections and flirtations between early astronomers and astrologers, astrology has long, if not always, been para- or even pseudoscientific, contributing nothing to astronomy or any other science.

These historical examples illustrate the need for a comprehensive analysis of any field or theory suspected of being a parascience. Even if we were wrong with our judgment at a given time, a genuine protoscience will sooner or later prove its fruitfulness and potential by developing into a full-fledged scientific field, propelled by successful research, or at least by giving rise to some scientific field.

But what exactly does "sooner or later" mean? We must ask this question because one of the most intriguing and sophisticated pseudosciences, namely parapsychology, has always claimed that it is actually a protoscience (or a pre-paradigmatic science, as some parapsychologists prefer to call it in Kuhnian terms), so that its classification as a pseudoscience would be unjustified. Now the birth of parapsychology as a field of research is usually taken to coincide with the establishment of the *Society for Psychical Research* in 1882, although earlier research in the area of spiritualism dates back to the 1850s [Kurtz, 1985]. Should a field still be regarded as a protoscience after more than 120–150 years? As mentioned several times in this chapter, parapsychology is a field still in search for a proper domain, because it has not succeeded in producing any findings that would convince its critics from mainstream psychology of the existence of some paranormal entities or processes [Hyman, 1989; Hines, 2003]. Worse, as Alcock [2003, p. 32] summarizes the situation: "...to the extent that parapsychology constitutes a 'field' of research, it is a field without a core knowledge base, a core set of constructs, a core set of methodologies, and a core set of accepted and demonstrable phenomena...". Does this not rather indicate that there is no such thing as psi (in other words, that the null hypothesis is true) and that the field is degenerative rather than protoscientific?

The same holds for astrology and creationism, which have also learned to exploit the "humbling lessons of history", claiming to be actually protosciences, which deserve to be granted their due chance of proving themselves full-fledged sciences. Yet if we are suspicious of a 120-150 years old protoscience, we are entitled to be even more skeptical of alleged protosciences that are thousands of years old.

A comprehensive profile of the epistemic field under consideration should also help to solve the problem of how to distinguish fruitful scientific heterodoxy from pseudoscientific deviation. In his foreword to the book "Scientists Confront Velikovsky" [Goldsmith, 1977], the famous science fiction author Isaac Asimov has coined the terms *endoheresy* and *exoheresy*. These terms capture nicely the gist of Section 5.3, namely the condition that a heresy must stay within the bounds of the scientific superparadigm, so to speak, if it is to be considered legitimate, even though the majority of the scientific community may reject it as mistaken or misguided. For example, in developmental biology there is a school called "developmental structuralism" [Webster and Goodwin, 1996], which takes genes to be relatively irrelevant for development, and hence seeks to explore the role of "universal laws of form" or "transformation laws" in development. Thus, it is attempted to describe the developing organism by field equations, reviving the earlier notion of a morphogenetic field. This structuralist approach is rejected or ignored by most developmental biologists, but it stays within the bounds of science, although some of the philosophical considerations of these authors seem to be in need of repair [Mahner and Bunge, 1997]. By contrast, the morphogenetic field hypothesis of the former biochemist Rupert Sheldrake is clearly an exoheresy, for it shows too many marks of pseudoscience and is irreparably esoteric [Carroll, 2003].

The preceding considerations result in the recommendation that both the sciences and the humanities ought to welcome endoheresies, because they form a valuable stock of alternative views, however implausible they may be at a given time. After all, it is too easy to be blinkered by orthodoxy which is reinforced by the routine of normal research. On the other hand, scientists must judge for themselves whether they wish to spend any time on investigating exoheresies. However, if not for scientific reasons, they should on occasion study exoheresies for educational purposes, explaining to the public why certain claims are parascientific and hence unworthy of serious attention. Although scientists may have very good reasons for rejecting exoheresies, they must keep explaining these reasons to the public in order to avoid the impression that their refusal to pay attention to parasciences is due to sheer dogmatism and arrogance. Thus, the advancement of the public understanding of science requires that we deal not only with science, but also with parascience.

7 CONCLUSION

Looking at the figures 2, 3 and 4, we notice that there are two main demarcation lines: the one between science and nonscience, and the other between reliable (approximately true) and illusory knowledge. Now some authors maintain that it is the latter which is the more important one (e.g., [Laudan, 1983; Haack, 2003]). After all, proper inquiry and proper standards of reasoning and evidence exist also outside science. For example, not only the philosopher arguing his case, but also the policeman investigating a crime knows (or at least should know) how to reason properly and how to distinguish good from bad evidence. As a consequence science would not differ in kind from other epistemic areas where common standards of rational and objective inquiry are practiced, but at most in the degree and thoroughness of their application [Haack, 2003]. Since determining when knowledge is gained in a proper way is the task of epistemology in general, it seems that the basic epistemological demarcation between knowledge and illusion is more important than that between science and nonscience.

This view usually rests on the idea that science is but an extended form of common sense, as both scientists like Thomas Huxley and Albert Einstein, and philosophers like John Dewey and Gustav Bergmann believed [Haack, 2003]. But unless the common sense of philosophers is totally different from everybody else's, this view is doubtful: there are good arguments for the contrary thesis that, in important respects, science transcends common sense and ordinary language, and therefore is quite "unnatural" [Wolpert, 1992]. The fact that so many people have serious difficulty in understanding scientific concepts, theories, and methods renders noncommonsensism more plausible than commonsensism. Yet even if scientific thinking were just extended common sense, it would still be the task of the philosopher of science to tell us how scientific cognition and knowledge differ from nonscientific cognition and knowledge.

In any case, wherever we eventually draw our lines, the important thing is to draw some line at all, so as not to surrender to relativism, arbitrariness, and irrationalism.

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