Defense of a Modest Scientific Realism

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1 Introduction

Let us begin by distinguishing *two* levels of debate about scientific knowledge: one crude, the other subtle. The crude debate pits scientific objectivists of all kinds — be they realists, pragmatists or of some other stripe — against postmodernists, relativists and radical social constructivists. The subtle debate pits scientific realists against objectivist anti-realists of various kinds (pragmatists, verificationists, instrumentalists, etc.).

This paper is intended as a (small) contribution to both debates. We want, of course, to defend the notion of science as a cognitive endeavor seeking (and sometimes finding) objective knowledge — in some sense or other — about the external world. And we want to defend a *modest realism*: one which insists that the goal of science is to find out how things really are and which asserts we are making progress in that direction, but which recognizes that this goal will always be incompletely achieved and which is aware of the principal obstacles.[1]

The crude debate would perhaps not be worth bothering with at all, were it not for the fact that relativism and radical social constructivism have become hegemonic in vast areas of the humanities, anthropology and sociology of science (among other fields). In many intellectual circles nowadays, it is simply taken for granted that all facts are "socially constructed", scientific theories are mere "myths" or "narrations", scientific debates are resolved by "rhetoric" and "enlisting allies", and truth is a synonym for intersubjective agreement. If all this seems an overstatement, consider the following assertions by prominent Science Studies practitioners:

> [T]he validity of theoretical propositions in the sciences is in no way affected by factual evidence.[2]
>
> The natural world has a small or non-existent role in the construction of scientific knowledge.[3]
>
> Since the settlement of a controversy is the *cause* of Nature's representation, not the consequence, we can never use the outcome — Nature — to explain

[1]For related arguments, see Nagel (1997), Haack (1998), Kitcher (1998), Maxwell (1998) and Brown (2001).

[2]Gergen (1988, p. 37).

[3]Collins (1981, p. 3). Two qualifications need to be made: First, this statement is offered as part of Collins' introduction to a set of studies (edited by him) employing the relativist approach, and constitutes his summary of that approach; he does not *explicitly* endorse this view, though an endorsement seems implied by the context. Second, while Collins appears to intend this assertion as an empirical claim about the history of science, it is possible that he intends it neither as an empirical claim nor as a normative principle of epistemology, but rather as a methodological injunction to sociologists of science: namely, to act *as if* "the natural world ha[d] a small or non-existent role in the construction of scientific knowledge", or in other words to *ignore* ("bracket") whatever role the natural world may in fact play in the construction of scientific knowledge. We have argued elsewhere (Bricmont and Sokal 2001) that this approach is seriously deficient *as methodology* for sociologists of science.

> how and why a controversy has been settled.[4]

> For the relativist [such as ourselves] there is no sense attached to the idea that some standards or beliefs are really rational as distinct from merely locally accepted as such.[5]

> Science legitimates itself by linking its discoveries with power, a connection which *determines* (not merely influences) what counts as reliable knowledge ...[6]

Over the last four years, we have participated in numerous debates with sociologists, anthropologists, psychologists, psychoanalysts and philosophers. Although the reactions were extremely diverse, we have repeatedly met people who think that assertions of fact about the natural world can be true "in our culture" and yet be false in some other culture.[7] We have met people who systematically confuse facts and values, truths and beliefs, the world and our knowledge of it. Moreover, when challenged, they will consistently deny that such distinctions make sense. Some will claim that witches are as real as atoms, or pretend to have no idea whether the Earth is flat, blood circulates or the Crusades really took place. Note that these people are otherwise reasonable researchers or university professors. All this indicates the existence of a radically relativist academic Zeitgeist, which is weird.[8] To be sure, these are oral statements made in seminars or private discussion, and oral statements usually tend to be more radical than written ones. But the published written assertions quoted in the preceding paragraph are already quite weird.[9]

If one inquires about the justifications for these surprising views, one is invariably led to the "usual suspects": the writings of Kuhn, Feyerabend and Rorty; the underdetermination of theories by data; the theory-ladenness of observation; some writings of (the later) Wittgenstein; the "Strong Programme" in the sociology of science.[10] Of course, the latter authors do not usually make the most radical claims that we have heard. Rather, what typically happens is that they make ambiguous or confused statements that are then interpreted by others in a radically relativist fashion.

[4] Latour (1987, pp. 99, 258), emphasis in the original. See Sokal and Bricmont (1998, chap. 4) for a detailed discussion.

[5] Barnes and Bloor (1981, p. 27), clarification added by us.

[6] Aronowitz (1988, p. 204), emphasis in the original.

[7] For an example involving the origins of Native American populations, see Sokal and Bricmont (1998, Epilogue) and Boghossian (1996).

[8] We emphasize that we have no idea how widespread these extreme positions are. But their mere existence is weird enough.

[9] For extremely weird written statements, see also the discussion by Latour of the causes of the death of the pharaoh Ramses II (Latour 1998); and for a critique, see Sokal and Bricmont (1998, note 123).

[10] In this paper we will be restricting our attention to epistemological questions; we will not be addressing the sociology of science, its tasks or its methodologies. See Bricmont and Sokal (2001) for a critique of the methodological relativism embodied in the Strong Programme.

Therefore, one of our goals here will be to disentangle various confusions caused by fashionable ideas in the contemporary philosophy of science. Roughly speaking, we will argue that those ideas contain a kernel of truth that can be understood properly when those ideas are carefully formulated; but then they give no support to radical relativism.

A far more subtle debate in the philosophy of science concerns the relative merits of realism and instrumentalism (or pragmatism).[11] Roughly speaking, realism holds that the goal of science is to find out how the world really is, while instrumentalism holds that this goal is an illusion and that science should aim at empirical adequacy. We will address this debate in detail in a moment; for now we simply want to emphasize how it is *not* relevant for the crude debate. Relativists sometimes tend to fall back on instrumentalist positions when challenged, but in reality there is a profound difference between the two attitudes.[12] Instrumentalists may want to claim either that we have no way of knowing whether "unobservable" theoretical entities really exist, or that their meaning is defined solely through measurable quantities; but this does not imply that they regard such entities as "subjective" in the sense that their meaning would be significantly influenced by extra-scientific factors (such as the personality of the individual scientist or the social characteristics of the group to which she belongs). Indeed, instrumentalists may regard our scientific theories as, quite simply, the most satisfactory way that the human mind, with its inherent biological limitations, is capable of understanding the world.

This paper is organized as follows: In Section 2 we shall examine some basic epistemological problems (notably the underdetermination of theory by evidence) and discuss the problems faced by both realism and instrumentalism. We shall also offer some brief comments on radical relativism and radical redefinitions of truth. In Section 3 we shall sketch what seems to us to be a defensible *modest realism*, and point out its relation with the picture of the world provided by the renormalization group in physics.

2 Some Basic Epistemological Problems

2.1 Solipsism and radical skepticism

Before discussing some serious issues in the philosophy of science, we need to clear out of the way some old red herrings. The first point that should be non-controversial is that solipsism (the idea that there is nothing in the world except my sensations) and radical skepticism (that no reliable knowledge of the world can ever be obtained) cannot be refuted. It is doubtful whether anyone really believes those doctrines — at least when crossing a city street — but their irrefutability is nevertheless an important philosophical observation. Since the arguments are standard and go back at least to

[11] For a variety of views, see e.g. Leplin (1984).

[12] This point is also made clearly by Brown (2001, chap. 5).

Hume, we need not repeat them here. Unfortunately, many of the arguments adduced in favor of relativist ideas are, in reality, banal reformulations of radical skepticism but applied in unjustifiably selective ways.[13]

2.2 Realism and its discontents

In the same way that nearly everyone in his or her everyday life disregards solipsism and radical skepticism and spontaneously adopts a "realist" or "objectivist" attitude toward the external world, scientists spontaneously do likewise in their professional work. Indeed, scientists rarely use the word "realist", because it is taken for granted: *of course* they want to discover (some aspects of) how the world really is! And *of course* they adhere to a "correspondence" notion of truth (again, a word that is barely used): if a biologist asserts it is true that a given disease is caused by a given virus, she means that, in actual fact, the disease is caused by the virus.[14,15] Of course, much preliminary discussion may be required, in any given case, to clarify the *meaning* of the terms used in the assertion; but once the meaning of the statement has been clarified to the point that what is being asserted is (sufficiently) unambiguous, the statement's truth value is determined solely by the extent to which the assertion does or does not correspond to reality.

Please note that by adopting this notion of truth[16], we are not yet making any claim about how one *obtains evidence* concerning the truth or falsity of a given statement, or even about whether that is possible. These are separate questions: one thing is to pose a problem clearly, the other is to solve it. Consider, for example, the statement "William Shakespeare was born on April 23, 1564". No one today knows for sure whether this statement is true or false[17], and no one has yet found a method for obtaining definitive evidence one way or the other. Nevertheless, this statement *is* either true or false (once one clarifies, for example, that it is to be interpreted

[13] Another favorite tactic employed by relativists is to conflate facts and our knowledge of them, not by giving any argument, but simply by using intentionally ambiguous terminology. See Sokal and Bricmont (1998, chap. 4) for examples in the works of Kuhn, Barnes–Bloor, Latour and Fourez.

[14] This interpretation of the word "true" is, in our view, quite simply a *precondition for the intelligibility* of people's assertions about the world.

[15] Let us stress that we are here using the term "correspondence notion of truth" in a broad sense; we do *not* intend to enter into the philosophical debate between "correspondence theories of truth" (understood in the narrow sense) and "deflationary theories of truth" (see e.g. Devitt 1997, chap. 3). Our main concerns in this paper are ontological and epistemological, not semantic; both correspondence and deflationary theories are (insofar as we can understand them) compatible with our vision of scientific realism. Our principal aim is, rather, to distinguish the notion of truth as "correspondence with reality", broadly understood, from epistemic notions (e.g. warranted assertability, verification) and pragmatic/relativistic notions (e.g. utility, intersubjective agreement).

[16] Or rather, simply *acknowledging* that this is how the word "true" is universally used by fluent speakers of the English language (except for a few philosophers to be discussed in Section 2.4).

[17] The parish register of Holy Trinity Church in Stratford-upon-Avon shows that Shakespeare was baptized there on April 26, 1564. But his exact birth date is unknown.

relative to the Julian calendar); and its truth or falsity depends only on the facts of Shakespeare's birth (and not, for example, on the beliefs or other characteristics of some individual or social group).

So, how *does* one obtain evidence concerning the truth or falsity of scientific assertions? By the same imperfect methods that we use to obtain evidence about empirical assertions generally. Modern science, in our view, is nothing more or less than the deepest (to date) refinement of the rational attitude toward investigating *any* question about the world, be it atomic spectra, the etiology of smallpox, or the Bielefeld bus routes. Historians, detectives and plumbers — indeed, all human beings — use the same basic methods of induction, deduction and assessment of evidence as do physicists or biochemists.[18] Modern science tries to carry out these operations in a more careful and systematic way, by using controls and statistical tests, insisting on replication, and so forth. Moreover, scientific measurements are often much more precise than everyday observations; they allow us to discover hitherto unknown phenomena; and scientific theories often conflict with "common sense". But the conflict is at the level of conclusions, not the basic approach. As Susan Haack lucidly observes:

> Our standards of what constitutes good, honest, thorough inquiry and what constitutes good, strong, supportive evidence are not internal to science. In judging where science has succeeded and where it has failed, in what areas and at what times it has done better and in what worse, we are appealing to the standards by which we judge the solidity of empirical beliefs, or the rigor and thoroughness of empirical inquiry, generally.[19]

Scientists' spontaneous epistemology — the one that animates their work, regardless of what they may say when philosophizing — is thus a rough-and-ready *realism*: the goal of science is to discover (some aspects of) how things really are. More precisely,

> The aim of science is to give a true (or approximately true) description of reality. This goal is realizable, because:
>
> 1. Scientific theories are either true or false. Their truth (or falsity) is literal, not metaphorical; it does not depend in any way on us, or on how we test those theories, or on the structure of our minds, or on the society within which we live, and so on.
> 2. It is possible to have evidence for the truth (or falsity) of a theory. (It remains possible, however, that all the evidence supports some theory T, yet T is false.)[20]

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[18] The allusion to historians and detectives was employed independently (and prior to us) by Haack (1993, p. 137): "there is no reason to think that [science] is in possession of a special method of inquiry unavailable to historians, detectives, and the rest of us". See also Haack (1998, pp. 96–97).

[19] Haack (1998, p. 94).

[20] This brief definition of realism is due to Brown (2001, beginning of Chapter 5, pp. 152–153 in uncorrected page proofs).

The most powerful objections to the viability of scientific realism consist in various theses showing that theories are underdetermined by data.[21] In its most common formulation, the underdetermination thesis says that, for any finite (or even infinite) set of data, there are infinitely many mutually incompatible theories that are "compatible" with those data. This thesis, if not properly understood[22], can easily lead to radical conclusions. The biologist who believes that a disease is caused by a virus presumably does so on the basis of some "evidence" or some "data". Saying that a disease is caused by a virus presumably counts as a "theory" (e.g. it involves, implicitly, many counterfactual statements). But if there are really infinitely many distinct theories that are compatible with those "data", then we may legitimately wonder on what basis one can rationally choose between those theories.

In order to clarify the situation, it is important to understand how the underdetermination thesis is established; then its meaning and its limitations become much clearer. Here are some examples of how underdetermination works; one may claim that:

– The past did not exist: the universe was created five minutes ago along with all the documents and all our memories referring to the alleged past in their present state. Alternatively, it could have been created 100 or 1000 years ago.

– The stars do not exist: instead, there are spots on a distant sky that emit exactly the same signals as those we receive.

– All criminals ever put in jail were innocent. For each alleged criminal, explain away all testimony by a deliberate desire to harm the accused; declare that all evidence was fabricated by the police and that all confessions were obtained by force.[23]

Of course, all these "theses" may have to be elaborated, but the basic idea is clear: given any set of facts, just make up a story, no matter how *ad hoc*, to "account" for the facts without running into contradictions.[24]

It is important to realize that this is all there is to the general (Quinean) underdetermination thesis. Moreover, this thesis, although it played an important role in the refutation of the most extreme versions of logical positivism, is not very different from the observation that radical skepticism or even solipsism cannot be refuted: all our knowledge about the world is based on some sort of inference from the observed to the unobserved, and no such inference can be justified by deductive logic alone. However, it is clear that, in practice, nobody ever takes seriously such "theories" as

[21] Often called the Duhem–Quine thesis. In what follows, we will refer to Quine's version (Quine 1980), which is much more radical than Duhem's.

[22] Particularly concerning the meaning of the word "compatible". See Laudan (1990) for a more detailed discussion.

[23] Of course, this latter situation, unlike the previous two, *does* occur frequently enough. But its occurrence or not depends on the particular case, while the underdetermination thesis is a *general* principle meant to apply to *all* cases.

[24] In the famous paper in which Quine sets forth the modern version of the underdetermination thesis, he even allows himself to change the meanings of words and the rules of logic, in order to show that any statement can be held true, "come what may" (Quine 1980, p. 43).

those mentioned above, any more than they take seriously solipsism or radical skepticism. Let us call these "crazy theories"[25] (of course, it is not easy to say exactly what it means for a theory to be non-crazy). Note that these theories require no work: they can be formulated entirely *a priori*. On the other hand, the difficult problem, given some set of data, is to find even one non-crazy theory that accounts for them. Consider, for example, a police enquiry about some crime: it is easy enough to invent a story that "accounts for the facts" in an *ad hoc* fashion (sometimes lawyers do just that); what is hard is to discover who really committed the crime and to obtain evidence demonstrating that beyond a reasonable doubt. Reflecting on this elementary example clarifies the meaning of the underdetermination thesis. Despite the existence of innumerable "crazy theories" concerning any given crime, it sometimes happens in practice that there is a unique theory (i.e. a unique story about who committed the crime and how) that is *plausible* and compatible with the known facts; in that case, one will say that the criminal has been discovered (with a high degree of confidence, albeit not with certainty). It may also happen that no plausible theory is found, or that we are unable to decide which one among several suspects is really guilty: in these cases, the underdetermination is real.[26]

One might next ask whether there exist more subtle forms of underdetermination than the one revealed by a Duhem–Quine type of argument. In order to analyze this question, let us consider the example of classical electromagnetism. This is a theory that describes how particles possessing a quantifiable property called "electric

[25] Or, as the physicist David Mermin calls them, "Duhem–Quine monstrosities" (Mermin 1998).

[26] Closely related to undetermination is the problem of the theory-ladenness of observation (see e.g. Sokal and Bricmont 1998, pp. 62–64 for an elementary introduction), which is often cited by relativists as providing grist for their mill. But it actually does nothing of the kind. Thomas Nagel offers an instructive example:

> Suppose I have the theory that a diet of hot fudge sundaes will enable me to lose a pound a day. If I eat only hot fudge sundaes and weigh myself every morning, my interpretation of the numbers on the scale is certainly dependent on a theory of mechanics that explains how the scale will respond when objects of different weights are placed on it. But it is not dependent on my dietary theories. If I concluded from the fact that the numbers keep getting higher that my intake of ice cream must be altering the laws of mechanics in my bathroom, it would be philosophical idiocy to defend the inference by appealing to Quine's dictum that all our statements about the external world face the tribunal of experience as a corporate body, rather than one by one. Certain revisions in response to the evidence are reasonable; others are pathological. (Nagel 1998, p. 35)

Though Quine's insistence that "any statement can be held true come what may" (Quine 1980, p. 43) can be read as an apologia for radical relativism, his discussion (pp. 43–44) suggests that this is *not* his intention, and that he agrees with Nagel that certain modifications of our belief systems in the face of "recalcitrant experiences" are much more reasonable than others. Moreover, in the foreword to the 1980 edition of his book, Quine backtracked from his earlier assertion that "the unit of empirical significance is the whole of science" (p. 42), and said (correctly in our view) that "empirical content is shared by the statements of science in clusters and cannot for the most part be sorted out among them. Practically the relevant cluster is indeed never the whole of science" (p. viii).

charge" produce "electromagnetic fields" that "propagate in vacuum" in a certain precise fashion and then "guide" the motion of charged particles when they encounter them.[27] Of course, no one ever "sees" directly an electromagnetic field or an electric charge. So, should one interpret this theory "realistically", and if so, what should it be taken to mean?

Classical electromagnetic theory is immensely well supported by precise experiments and forms the basis for a large part of modern technology. It is "confirmed" every time one of us switches on his or her computer and finds that it works as designed.[28] Does this overwhelming empirical support imply that there are "really" electric and magnetic fields propagating in vacuum? In support of the idea that there are, one could argue that electromagnetic theory postulates the existence of those fields and that there is no known non-crazy theory that accounts equally well for the same data; therefore it is reasonable to believe that electric and magnetic fields *really* exist.

But is it in fact true that there are no alternative non-crazy theories? Here is one possibility: Let us claim that there are no fields propagating "in vacuum", but that, rather, there are only "forces" acting directly between charged particles.[29] Of course, in order to preserve the empirical adequacy of the theory, one has to use exactly the same Maxwell–Lorentz system of equations as before (or a mathematically equivalent system). But one may interpret the fields as a mere "calculational device" allowing us to compute more easily the net effect of the "real" forces acting between charged particles.[30] Almost every physicist reading these lines will say that this is some kind of metaphysics or maybe even a play on words — that this "alternative theory" is really just standard electromagnetic theory in disguise. Now, although the precise meaning of "metaphysics" is hard to pin down[31], there is a vague sense in which, if we use exactly the same equations (or a mathematically equivalent set of equations) and make exactly the same predictions in the two theories, then they are really the *same* theory as far as "physics" is concerned, and the distinction between the two —

[27] We are referring here to Maxwell's equations describing how fields are produced by charges and how they propagate, and to the Lorentz force describing how the fields "guide" the particles.

[28] When it fails to work as designed, this is, as all physicists know, the fault of the engineers and computer programmers.

[29] Since electromagnetic fields propagate at a finite speed, the forces introduced here, unlike those in Newtonian mechanics, would have to act in a non-instantaneous (i.e. delayed) manner.

[30] This attitude is reminiscent of that of Galileo's adversary Cardinal Bellarmino, who was willing to accept the Copernican system as a "calculational device" for predicting the motions of the planets; he was even willing to concede — though it was not then true, and only became true 50 years later with the development of Newtonian mechanics — the superior empirical adequacy of the Copernican system over the Ptolemaic system. He merely insisted that the Earth does not *really* move around the Sun.

[31] During the 1950s, Bertrand Russell observed: "The accusation of metaphysics has become in philosophy something like being a security risk in the public service. ... The only definition I have found that fits all cases is: 'a philosophical opinion not held by the present author'." (Russell 1995 [1959], p. 164)

if any — lies outside of its scope.

The same kind of observation can be made about most physical theories: In classical mechanics, are there really forces acting on particles, or are the particles instead following trajectories defined by variational principles? In general relativity, is space-time really curved, or are there, rather, fields that cause particles to move *as if* space-time were curved?[32] Let us call this kind of underdetermination "genuine", as opposed to the "crazy" underdeterminations of the usual Duhem–Quine thesis. By "genuine", we do not mean that these underdeterminations are necessarily worth losing sleep over, but simply that there is no rational way to choose (at least on empirical grounds alone) between the alternative theories — if indeed they should be regarded as different theories.

It is important to note the difference between the ways that the two kinds of underdetermination are established: the first can be established by pure reasoning, while the second depends (at least in part) on the concrete form of specific scientific theories. In fact, it is certainly an interesting (and very difficult) problem for philosophers of science to describe as precisely as possible, for a given scientific theory, the various inequivalent but natural "metaphysics" that can be associated with it.

But this is not yet the end of the story. There is another, much more serious, alternative to classical electromagnetism: namely, quantum electromagnetism (otherwise known as quantum electrodynamics, or QED for short). Indeed, QED has superseded classical electromagnetism as a fundamental description of reality; we now think of classical electromagnetism as being some kind of approximation to QED, valid for a more-or-less well-defined class of phenomena where quantum effects are negligible. This situation leaves some hope for the realist: it could be that the more fundamental theory (here QED) allows only one "natural" set of unobservable entities, whose existence would therefore be vindicated by the empirical successes of the theory. That may actually be the case, but it is not very likely: the deeper we probe into the nature of things, the *stranger* they tend to look.[33] Even in non-relativistic quantum mechanics, the status of "unobservable" entities, such as the wave function, is far from clear; and although it is risky to predict the future, it seems unlikely that a deeper theory, even an ultimate one, would have a unique interpretation in terms of

[32] Poincaré much emphasized this type of "underdetermination": for instance, he stressed the fact that we cannot know whether the Earth "really" rotates (Poincaré 1904). Indeed, one can always choose a reference system in which the Earth is at rest and nonrotating. But it has to be realized that, if one makes such a choice, one must consider as "real" the inertial forces (e.g. the centrifugal and Coriolis forces) that "act" on distant stars and make them move faster than the speed of light. It is interesting to note that, when Poincaré made this proposition, it was interpreted by clerical forces (at the beginning of the twentieth century!) as vindicating the condemnation of Galileo by the Church (see Mawhin 1996 for a detailed historical discussion). But this attitude shows a deep misunderstanding. For the Church, the Earth was at rest in a much more absolute sense than the one suggested by Poincaré. In fact, Poincaré's viewpoint makes sense only within a framework (that of classical mechanics) created by Galileo, Newton and their successors.

[33] That is not surprising: the deeper we probe into the nature of things, the farther we stray from the intuitions about macroscopic objects (and about human psychology, etc.) that were sculpted into our brains by natural selection.

unobservable entities.

There is a further problem for realism, and that is the problem of meaning. Before asking whether electromagnetic fields really exist, one might ask: What does the term "electromagnetic field" mean? A mathematical expression? But what does it mean for such an expression to exist in the physical world? Trying to answer that question immediately raises other questions about the status of mathematical objects, and about the correspondence between mathematical objects and the physical world.

2.3 Instrumentalism

The difficulties encountered by a hard-headed realist approach to science — and in particular to fundamental physics — suggest the adoption of a more modest attitude. Perhaps we should renounce the effort to describe the world "as it really is", and be content with seeking theories that are empirically adequate (and logically consistent, simple, etc.).

One example of the pragmatic attitude taken to absurd extremes is provided in a recent posting to the discussion group Scipolicy-L. The author is happy to defend science from postmodernist "deconstructions", provided only that scientists would refrain from making unjustified "metaphysical" assertions:

> The claim that laws of physics operate anywhere except in physics experiments ... seems to me *metaphysical* in the bad sense ...
>
> [T]he non-metaphysical interpretation of the laws of physics goes something like: Whenever we, as physicists, conduct such-and-such kind of experiment, the outcome we experience is such-and-such ...
>
> What the philosopher/hermeneuticist *should* try to convince scientists (and everyone else) of is that the laws of physics apply *only* to the domain of experimentation and the activity of physicists ...[34]

But if the laws of physics, inferred from laboratory experiments, have no validity outside the laboratory, why on earth would anyone bother doing those experiments in the first place? Experiments are not, after all, an end in themselves, like football or chess; they are, rather, a means to a higher end, namely obtaining information about the *universal* properties of the natural world. It is a far-from-obvious insight — hard won over the last 400 years — that systematic and controlled experimentation can yield knowledge about the world that would be difficult or impossible to extract from passive observation. And if Maxwell's equations hold only in physicists' labs, how can one plausibly *explain* (in a way that does not merely take it for granted) the transmission of this anti-metaphysical missive from the author's keyboard to the readers' screens?

Most self-described anti-realist philosophers of science would not, of course, go so far. They do not question that physics works outside the laboratory as well as

[34]Brad McCormick, posting to Scipolicy-L@yahoogroups.com, 22 May 2001, emphasis in the original.

inside; they only insist on a more modest interpretation of the claim that physics "works". Let us abandon "metaphysical" claims, they say, and stick to empirical adequacy. In particular, given the difficulties of realism in making precise the status of "unobservable" entities such as forces, fields and curved space-time, let us forget completely about those "metaphysical" entities, and formulate our physical theories solely in terms of observable quantities, since those are the only ones we have access to anyway. Or alternatively, let us consider those entities to be mere "calculational devices" — convenient fictions — to which we must avoid attributing any physical reality. This cluster of related (but not identical) positions is often called *instrumentalism* (or *operationalism*). Various versions of this doctrine have been championed by Pierre Duhem, Ernst Mach and the logical positivists of the Vienna Circle (among others) and were widely accepted (in words if not necessarily in deeds) by physicists in the period circa 1890–1970.[35]

But this position also encounters severe difficulties. The first problem is that the notion of something being "observable" is far from clear. Some observations are indeed made with our unaided senses, but should one limit oneself to those? Can one use eyeglasses, magnifying glasses, telescopes or microscopes without feeling obliged to translate the results back into "pure" sense data? What about infrared cameras, electron microscopes and gamma-ray telescopes? Radar and sonar?[36] And even observations made with our unaided senses are more problematic than they appear at first. For example, when I "see" a glass on the table in front of me, I do not really see the glass: rather, my eye absorbs the electromagnetic waves reflected from the glass, and my brain *infers* the existence and position of a material object (along with some of its properties such as shape, size and color). This type of inference is not, in the end, so different from the more explicit inferences from "data" to "theory" made by scientists.[37]

The second, deeper problem with instrumentalism is that the meaning of the words used by scientists goes far beyond what is "observable". To take a simple example, should paleontologists be allowed to speak about dinosaurs? Presumably yes. But in what sense are dinosaurs "observable"? After all, everything we know about them is inferred from fossil data; only the fossils are "observed". These inferences are not, of course, arbitrary: they can be justified by evidence from biology (that all bones were once part of organisms) and geology (concerning the processes that transform bones into fossils). The point is, simply, that fossil evidence is evidence for the existence of *something other than itself*: namely, the fossils of dinosaur bones are evidence for the existence (at some time in the past) of dinosaurs. And the meaning of the word "dinosaur" is not easily expressible in a language that would refer only to fossils.[38]

[35]See Weinberg (1992, pp. 174–184) for an insightful discussion.

[36]Perhaps bat instrumentalists are entitled to use sonar but not optical data, while for human instrumentalists it is the reverse.

[37]This line of argument was developed by Maxwell (1962).

[38]For example, assertions about dinosaurs' eating habits would have to be rephrased as assertions

Some instrumentalist philosophers of science are prepared to classify dinosaurs as "observable" on the grounds that, though *we* cannot observe them, they *would have been* observable to human beings had the the human species existed 100 million years ago. Now, anyone is free to define the word "observable" however he wishes; but there is no guarantee that the word, so defined, has any epistemological significance. In reality, neither dinosaurs nor electrons are ever observed directly; both are inferred from other observations, and the arguments supporting these two inferences are of comparable strength. It seems to us that, either one allows such inferences and accepts the probable reality (in some sense or other) of both dinosaurs and electrons, or else one rejects all such inferences and refuses to talk about either.[39] To be sure, the *meaning* of "electron" is far murkier than that of "dinosaur": since we can form mental pictures of mid-size objects like dinosaurs, the meaning of the words referring to them is reasonably clear intuitively even if the objects are never directly observed, which is not necessarily the case for entities like electrons. That is why we are careful to assert only that electrons exist "in some sense or other", while admitting frankly our perplexity about what electrons *really* are.[40,41]

Finally, and most importantly, when a theory repeatedly makes *surprising* predictions (particularly of *novel* phenomena) that are subsequently confirmed, this is

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concerning the spatial correlation of certain types of fossils with certain other types of fossils. This seems unhelpful, to put it mildly.

[39] Jim Brown (private communication) has made the important point that even statements about "observable" phenomena are often inferred, and that

> Sometimes observation statements that are inferred are more convincing than when directly experienced. I recall reading a funny example from Clarence Darrow [the famous American populist lawyer]. He was defending a union that had been attacked by company goons. One of the goons had bitten off the ear of a striker. The union was being prosecuted in court and Darrow was hoping to use the ear incident to defend the union. The key witness was on the stand. (I'm quoting from memory.)
>
>> Prosecutor: Did you see him bite the man's ear off?
>> Witness: No, I didn't.
>
> At this point Darrow, reminiscing on the case, comments that the prosecutor had us beaten and should have dismissed the witness, but he foolishly pushed on:
>
>> Prosecutor: Then how do you know he bit the ear off?
>> Witness: I saw him spit it out.

[40] As noted by van Fraassen (1994, p. 268), realists tend to use arguments involving mid-size objects, while instrumentalists tend to argue their case by focusing on fundamental entities like forces or fields. But this is connected with the problem of meaning: if we say "X exists", we must know what "X" means, which is less obvious for fundamental entities than for mid-size objects.

[41] It is worth emphasizing, however, that we understand the *properties* of electrons far better than we understand the properties of dinosaurs. For example, we are able to predict the magnetic moment of the electron to 11 decimal places of accuracy (see below), but we don't know what color dinosaurs were, whether they were warm-blooded, how their hearts worked, etc. We thank Norm Levitt for this observation.

powerful evidence that the theory is "on the right track", i.e. that it is at least approximately correct and that its "unobservable" theoretical entities really do exist in some sense or other. For how else could one explain such "miraculous" predictions? If scientific theories were merely simple, logically coherent summaries of the existing empirical data, one could expect successful theories to give accurate predictions of the particular phenomena they were intended to summarize, as well as of phenomena strongly correlated with them — but *not* of totally unrelated phenomena. Thus, it is unsurprising that Ptolemaic astronomy was successful in predicting the motions of the known planets: for the theory was essentially a sophisticated curve-fitting to the past observations of the known planets, and the future motions of the planets are strongly correlated with their past motions.[42] The theory's empirical success does not, therefore, give any strong reason to believe that it is approximately correct or that its theoretical entities (e.g. epicycles) really exist.[43] Newtonian mechanics, by contrast, was able not only to account for planetary motions in vastly simpler terms ($\mathbf{F} = m\mathbf{a}$ and the inverse-square law) and to achieve a unified theoretical understanding of both planetary and terrestrial motions — it was also able to predict the existence of *previously unobserved* planets, such as Neptune, found in 1846 where Le Verrier and Adams predicted it should be[44], and to predict the motion of *yet-to-be-launched* satellites. These facts — when taken together with all the other empirical confirmations of Newtonian mechanics — are, in our view, extremely strong evidence that Newtonian mechanics is getting *something* right about the world (but not, of course, that it is exactly correct or that its ontology is fundamental).

Here is an even more striking example: Quantum electrodynamics predicts that the magnetic moment of the electron (expressed in a well-defined unit which is unimportant for the present discussion) has the value

$$1.001\,159\,652\,201 \;\pm\; 0.000\,000\,000\,030$$

(where the "±" denotes the uncertainties in the theoretical computation, which in-

[42] This is because (as we now know) planetary motions are non-chaotic on time scales of less than a few million years.

[43] Jim Brown (private communication) has pointed out that Ptolemaic astronomy is capable of predicting eclipses *without using, as input, any data on past eclipses* (the only data used are non-eclipse observations of the positions of the sun and the moon). Surely *this*, he argues, is a surprising prediction. We agree: it shows, in fact, that *one* aspect of Ptolemaic astronomy's theoretical framework — namely, that solar eclipses arise when the moon occults the sun — really *is* at least approximately correct; eclipses are indeed correlated with the non-eclipse motions of the sun and the moon in exactly the way that Ptolemaic theory asserts. But the Ptolemaic theory's predictions for planetary motions are unsurprising, because the theory does little more than summarize the data on planetary motions that went into its construction.

[44] For a detailed history, see, for example, Grosser (1962) or Moore (1996, chaps. 2 and 3). Please note that the validity of our observation is independent of whether Adams and Le Verrier correctly computed the Newtonian prediction for the position of Neptune or found it partly by accident (as seems to be the case). The key fact is that if one does make the correct calculations based on Newton's theory, then one indeed finds the actually observed position of Neptune.

volves several approximations), while a recent experiment gives the result

$$1.001\,159\,652\,188 \;\pm\; 0.000\,000\,000\,004$$

(where the "$\pm$" denotes the experimental uncertainties).[45] This 11-decimal-place agreement between theory and experiment — particularly when combined with thousands of other similar though less spectacular ones — would be utterly miraculous if quantum electrodynamics were not saying something at least approximately true about the world. In particular, the predictive success of quantum electrodynamics would be a miracle if electrons did not really exist in some sense or other.[46]

So, if we look critically at realism, we may be tempted to turn toward instrumentalism. But if we look critically at instrumentalism, we feel forced to return to a modest form of realism. What, then, should one do? Before coming to a possible solution, let us first consider radical alternatives.

2.4 Redefinitions of truth

When facing the problems caused by underdetermination, one may be tempted by a radical turn: What about abandoning the notion of "truth" as "correspondence with reality", and seeking instead an alternative notion of truth? There are at least two currently fashionable proposals of this kind: one is to define truth through utility or convenience, the other is to define it through intersubjective agreement. The philosopher Richard Rorty offers examples of both:

> What people like Kuhn, Derrida and I believe is that it is pointless to ask whether there really are mountains or whether it is merely convenient for us to talk about mountains.[47]
>
> Philosophers on my side of the argument answer that objectivity is not a matter of corresponding to objects but a matter of getting together with other subjects — that there is nothing to objectivity except intersubjectivity.[48]

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[45]See Kinoshita (1995) for the theory, and Van Dyck *et al.* (1987) for the experiment. Crane (1968) provides a non-technical introduction to this problem. See also Lautrup and Zinkernagel (1999) for a very careful history, which shows that the agreement between theory and experiment is real. (One might worry that the experimental number was unduly influenced by the experimenters' knowledge of the theoretical prediction, or vice versa; but careful analysis of the history shows that this is not the case.)

[46]Once again, we say "in some sense or other" in order to emphasize that electrons, quarks, etc. may not belong to the fundamental ontology of the universe, but may only be — as we now know that Dalton's "atoms" are — merely approximations objectively valid at certain scales of size and energy. See Section 3.2 below for further elaboration of this point.

[47]Rorty (1998, p. 72). See also the critiques by Nagel (1997, pp. 28–30) and Albert (1998); and see Haack (1997) for an entertaining contrast between the two radically different "pragmatist" philosophies of C.S. Peirce and of Rorty.

[48]Rorty (1998, pp. 71–72).

Similar views are expressed by some of the founders of the Strong Programme in the sociology of science:

> The relativist, like everyone else, is under the necessity to sort out beliefs, accepting some and rejecting others. He will naturally have preferences and these will typically coincide with those of others in his locality. The words 'true' and 'false' provide the idiom in which those evaluations are expressed, and the words 'rational' and 'irrational' will have a similar function.[49]

The best way to see that these redefinitions do not work is to apply them to simple concrete examples. For instance, it would certainly be useful to make people believe that if they drive drunk they will go to hell or die from cancer, but that would not make those statements true (at least on an intuitive understanding of the word "true"). Similarly, once upon a time, people agreed that the Earth was flat (or that blood was static, etc.), and we now know that they were wrong. So intersubjective agreement does not coincide with truth (again, understood intuitively).

Of course, we are using here an intuitive notion of truth, and a critic might demand a more "rigorous" definition. But the problem is that all definitions tend to be circular or else to rely on fundamental undefined terms that one either grasps intuitively or does not grasp at all. And truth falls naturally in the latter category.[50]

A more fundamental problem is that these redefinitions of "truth" do not even succeed, as they claim to, in supplanting the conventional "correspondence" notion. Take, for instance, utility: to say that something is useful (for some specified goal) is already an objective statement (it has to be *really* useful for the declared goal) that relies implicitly on the correspondence notion of truth. The same remark is even more obvious for intersubjective agreement: to say that (other) people think so and so is an objective statement describing part of the (social) world "as it is".[51]

Of course, positive arguments are sometimes given to support redefinitions of truth, as for instance the following somewhat subtle sophism:

> ... the only criterion we have for applying the word "true" is justification and justification is always relative to an audience. So it is also relative to that audience's lights — the purpose that such an audience wants served and the situation in which it finds itself.[52]

The beginning of the first sentence is correct, but it does not imply that truth is identical to justification. (One may well be rationally justified in believing something

[49] Barnes and Bloor (1981, p. 27). See Sokal and Bricmont (1998, chap. 4) for a critique.

[50] After all, people who ask what "truth" means are not really in the same position as those who wonder what an octopus is or who Xenophon was.

[51] For a discussion of similar redefinitions of "truth", see Bertrand Russell's critique of the pragmatism of William James and John Dewey (Russell 1961, chaps. 24 and 25, in particular p. 779).

[52] Rorty (1998, p. 4).

that turns out, on closer examination, to be false.[53]) Moreover, what does it mean to say that justification is always relative to the purpose that an audience wants served? This introduces a subtle confusion between knowledge and values, by implicitly assuming that all knowledge depends on some "purpose", i.e. some non-cognitive goal. But what if the "audience" wants to find out how (some part of) the world really is? Rorty might reply that this goal is unattainable, as the following statement suggests: "A goal is something you can know you are getting closer to, or farther away from. But there is no way to know our distance from the truth, not even whether we are closer to it than our ancestors were."[54] But is this really so? Some of our ancestors thought that the Earth was flat. Don't we know better? Aren't we closer to the truth, in that respect at least?

The view proposed here is so implausible that one is forced to resort to some "charitable" interpretation. Perhaps Rorty means by "truth" something like the fundamental physical laws governing the entire universe, or an "absolute" truth discovered by pure thought (as in classical metaphysics); and it does makes sense to be skeptical about our ability to discover truths of those kinds. But if this is what Rorty means, then he should say so explicitly, rather than making statements that allegedly apply to all possible knowledge. Or, alternatively, perhaps Rorty simply wants to reiterate the banal observation that all statements of fact (even about the non-flatness of the Earth) can be challenged by a consistent radical skeptic. But that is not a particularly new insight.

2.5 Cognitive relativism

Roughly speaking, we will use the term "cognitive relativism" to refer to any philosophy that claims that the truth or falsity of a statement is relative to an individual or to a social group.[55]

The first thing to notice about cognitive relativism is that this doctrine follows naturally if we accept a radical redefinition of truth. Clearly, if truth reduces to utility, then the "truth" of a proposition will depend on the individual or social group for whom the proposition is alleged to be useful. Likewise, if truth reduces to intersubjective agreement, the "truth" of a proposition will depend on the particular group whose agreement is at issue. On the other hand, if we adopt the customary ("correspondence") notion of truth, then cognitive relativism is patently *false*: since a proposition is true to the extent that it reflects (some aspects of) the way the world

[53] For example, Hume (1988 [1748], section X) gives the example of a person in India who, quite rationally, refused to believe that water can become solid during winter (water solidifies very abruptly around the freezing point, so if one lives in a warm climate, it is indeed hard to believe that water can freeze). It shows that rational inferences from the available evidence do not necessarily lead to true conclusions.

[54] Rorty (1998, pp. 3–4).

[55] We will consider only relativism about statements of fact (i.e. about what exists or is claimed to exist), and leave aside relativism about ethical or aesthetic judgments.

is, its truth or falsity depends on the way the world is and not on the beliefs or other characteristics of any individual or group.

Since we have already discussed redefinitions of truth, there is not much to add, except that it makes no sense for ordinary scientists — whether they study nature or society — to adopt, even implicitly, a cognitive relativist attitude. For cognitive relativism amounts to abandoning the goal of objective knowledge pursued by science. However, it seems that some historians and sociologists want to have it both ways: adopt a relativist attitude with respect to the natural sciences, and an objectivist (even naive realist) attitude with respect to the social sciences.[56] But that is inconsistent; after all, research in history, and in particular in the history of science, employs methods that are not radically different from those used in the natural sciences: studying documents, drawing the most rational inferences, making inductions based on the available data, and so forth. If arguments of this type in physics or biology did not allow us to arrive at reasonably reliable conclusions, what reason would there be to trust them in history or sociology? Why speak in a realist mode about historical categories, such as Kuhnian paradigms, if it is an illusion to speak in a realist mode about scientific concepts (which are in fact much more precisely defined) such as electrons or DNA?

3 Towards a Reasonable Epistemology

3.1 Epistemological opportunism

Given that instrumentalism is not defensible when it is formulated as a rigid doctrine, and since redefining truth leads us from bad to worse, what should one do? A hint of one sensible response is provided by the following comment of Einstein:

> Science without epistemology is — insofar as it is thinkable at all — primitive and muddled. However, no sooner has the epistemologist, who is seeking a clear system, fought his way through such a system, than he is inclined to interpret the thought-content of science in the sense of his system and to reject whatever does not fit into his system. The scientist, however, cannot afford to carry his striving for epistemological systematic that far. ... He therefore must appear to the systematic epistemologist as an unscrupulous opportunist.[57]

So let us try epistemological opportunism. We are, in some sense, "screened" from reality (we have no immediate access to it, radical skepticism cannot be refuted, etc.). There are no absolutely secure foundations on which to base our knowledge. Nevertheless, we all assume implicitly that we can obtain some reasonably reliable knowledge of reality, at least in everyday life. Let us try to go farther, putting to work all the resources of our fallible and finite minds: observations, experiments, reasoning.

[56] See Sokal and Bricmont (1998, chap. 4) for relevant quotes from Kuhn, Feyerabend, Barnes–Bloor and Fourez, along with a more detailed critique.

[57] Einstein (1949, p. 684).

And then let us see how far we can go. In fact, the most surprising thing, shown by the development of modern science, is how far we seem to be able to go.

Unless one is a solipsist or a radical skeptic — which nobody really is — one has to be a realist about *something*: about objects in everyday life, or about the past, dinosaurs, stars, viruses, whatever. But there is no natural border where one could somehow radically change one's basic attitude and become thoroughly instrumentalist or pragmatist (say, about atoms or quarks or whatever). There are many differences between quarks and chairs, both in the nature of the evidence supporting their existence and in the way we give meaning to those words, but they are basically differences of degree. Instrumentalists are right to point out that the meaning of statements involving unobservable entities (like "quark") is in part related to the implications of such statements for direct observations. But only in part: though it is difficult to say exactly how we give meaning to scientific expressions, it seems plausible that we do it by combining direct observations with mental pictures and mathematical formulations, and there is no good reason to restrict oneself to only one of these. Likewise, conventionalists like Poincaré are right to observe that some scientific "choices", like the preference for inertial over noninertial reference frames, are made for pragmatic rather than objective reasons. In all these senses, we have to be epistemological "opportunists". But a problem worse than the disease arises when any of these ideas are taken as rigid doctrines replacing "realism".

A friend of ours once said: "I am a naive realist. But I admit that knowledge is difficult." This is the root of the problem. Knowing how things really are is the goal of science; this goal is difficult to reach, but not impossible (at least for some parts of reality and to some degrees of approximation). If we change the goal — if, for example, we seek instead a consensus, or (less radically) aim only at empirical adequacy — then of course things become much easier; but as Bertrand Russell observed in a similar context, this has all the advantages of theft over honest toil.

It is important to remember that scientific knowledge needs no "justification" from the outside. The justification for the objective validity of scientific theories (in the sense of being at least approximate truths about the world) lies in specific theoretical and empirical arguments. Of course, philosophers, historians or sociologists may be impressed by the successes of the natural sciences (as the logical positivists were) and seek to understand how science works. But there are two frequent mistakes to avoid: One is to think that, because some particular account fails (say, the logical-positivist one or the Popperian one), then some alternative account (e.g. the socio-historical one) *must work*. But that is an obvious fallacy; perhaps *no* existing account works.[58] The second, and more fundamental, mistake is to think that our inability to account in general terms for the success of science somehow makes scientific knowledge less reliable or less objective. That confuses accounting and justifying. After all, Einstein and Darwin gave arguments for their theories, and those arguments were far

[58] See McGinn (1993, chap. 7) for the interesting suggestion that understanding our own knowledge-producing mechanisms simply lies outside the bound of what is biologically feasible for our limited minds.

from being all erroneous. Therefore, even if Carnap's and Popper's epistemologies were entirely misguided, that would not begin to cast doubt on relativity theory or evolution.

Moreover, the underdetermination thesis, far from undermining scientific objectivity, actually makes the success of science all the more remarkable. Indeed, what is difficult is not to find a story that "fits the data", but to find even one *non-crazy* such story. How does one know that it is non-crazy? A combination of factors: its predictive power, its explanatory value, its breadth and simplicity, etc. Nothing in the (Quinean) underdetermination thesis tells us how to find inequivalent theories with some or all of these properties. In fact, there are vast domains in physics, chemistry and biology where there is only one[59] known non-crazy theory that accounts for the known facts and where many alternative theories have been tried and failed because their predictions contradicted experiments. In those domains, one can reasonably think that our present-day theories are at least approximately true, in some sense or other. An important (and difficult) problem for the philosophy of science is to clarify the meaning of "approximately true" and its implications for the ontological status of unobservable theoretical entities. We do not claim to have a solution to this problem, but we would like to offer a few ideas that might prove useful.

3.2 The "renormalization-group view of the world"

The status of unobservable entities in fundamental physics can be clarified by considering the relationship between successive "levels" of theorization of the same physical object. For example, chairs appear to us in everyday life as solid objects, and water appears to us as a continuous fluid. Atomic theory, on the other hand, teaches us that both chairs and water are composed of atoms. The two levels of description thus have radically different ontologies. But atomic theory does not simply declare that our everyday intuitions are wrong. Quite the contrary: atomic theory *implies* that certain aggregations of atoms will act, on macroscopic scales, as hard solids (due to the very strong electrical repulsions between protons in the two objects) and that other aggregations of atoms will act as fluids.[60] Therefore, the non-fundamental ontology of everyday life (solids and fluids) can be seen as a kind of "coarse-grained" macroscopic approximation to the more fundamental microscopic ontology of quarks and electrons; indeed, the former should be (at least in principle) *derivable* as a logical consequence of the underlying more fundamental theory.

An analogous relation holds between successive well-confirmed physical theories in the same domain. For example, in Newtonian mechanics particles interact via forces acting instantaneously at a distance, while in general relativity particles (and fields) alter the geometry of space-time, which in turn influences the motion of other

[59] Modulo the "genuine" underdeterminations discussed in Section 2.2.

[60] Of course, the details of these implications have not yet been fully worked out — we are not yet able to predict quantitatively, directly from atomic theory, the hardness of a chair (or of steel) or the viscosity of water — but qualitatively the situation is reasonably well understood.

particles. Newtonian mechanics and general relativity make only slightly different predictions for the orbits of planets, but their fundamental ontologies are radically different. Nevertheless, Newtonian mechanics is in some sense derivable from general relativity as a low-velocity weak-field approximation, so its ontology is in some sense a "coarse-grained" version of the more fundamental general-relativistic ontology.[61]

Thoughtful philosophers and scientists have understood for centuries that all measurements have a finite accuracy, so that it is dangerous to infer from the empirical adequacy of a theory — e.g. the fact that, as of 1850, Newtonian mechanics accounted for all known planetary orbits to an extraordinary precision — that the theory is *exactly* correct. All one can reasonably assert is that the theory is probably *approximately* correct (to some specified precision) in the domain where it has been well tested, so that any subsequent theory will have to incorporate the old theory as a valid approximation in this domain. The foregoing considerations now indicate a further danger: not only may the older theory be approximate rather than exact in a quantitative sense; it may also get the fundamental ontology all wrong. But this does not mean that its ontology is *simply wrong*; rather, it means that what appears in the older theory to be a fundamental entity is, in reality, a non-fundamental entity derivable as a "coarse-grained" version of something deeper.[62]

It is reasonable to conjecture that the relationship between present-day well-confirmed theories and their future successors will be something like the relationship between past well-confirmed theories and their present-day successors. For example, all of modern atomic and elementary-particle physics is based on quantum field theory (including quantum electrodynamics and, more generally, the "standard model" of electromagnetic, weak and strong interactions); and these theories have been empirically verified in vast domains, sometimes to phenomenal accuracy.[63] Likewise, general relativity gives our best current understanding of gravitational phenomena (from baseballs to planets to the universe as a whole); and it too has been confirmed to impressive precision in wide domains. Nevertheless, we are reasonably sure that these two theories *cannot* both be *exactly* true, because their fundamental ontologies are mutually incompatible.[64] We hope that quantum field theory and general

[61] We say "in some sense" because, once again, these derivations are difficult (if one tries to fill in all the details) and not *fully* understood today.

[62] As pointed out by Weinberg in his very interesting critique of Kuhn: "If you have bought one of those T-shirts with Maxwell's equations on the front, you may have to worry about its going out of style, but not about its becoming false. We will go on teaching Maxwellian electrodynamics as long as there are scientists." (Weinberg 1998) Weinberg makes an important distinction between the "soft" and "hard" parts of scientific theories. The hard part — consisting basically in the equations themselves, their interpretation in operational terms, and the class of phenomena to which they apply — does not change when scientific revolutions occur. The soft part, on the other hand, which has to do with the basic ontology postulated by the theory, does tend to change.

[63] See e.g. the discussion of the magnetic moment of the electron at the end of Section 2.3.

[64] The fields of general relativity encode the geometry of a smooth space-time manifold, while quantum mechanics implies that all fields undergo quantum fluctuations, which become stronger at smaller scales. It follows that in a quantum theory where geometry is a dynamical field, space-time

relativity will some day be superseded by an as-yet-nonexistent theory of quantum gravity. Whether this process stops somewhere at some fundamental, "final" theory or whether there are theories "all the way down", no one knows.[65] Either way, it is reasonable to expect that the fundamental ontologies of both quantum field theory and general relativity will survive in future theories as non-fundamental "coarse-grained" ontologies valid in specific domains to specific degrees of accuracy.

These considerations can be summarized in a picture that is basic to most thinking in contemporary physics: let us call it the "renormalization-group view of the world", after the work in statistical mechanics and quantum field theory performed during the 1970s (but too technical to explain in detail here) that shows how to make rather precise the concept of one theory being a "coarse-grained" approximation of another.[66] In this view, reality is composed of a hierarchy of "scales", ranging from ???? to quarks to atoms to fluids and solids ... to stars to galaxies to ???? (with bipedal primates somewhere in-between). The theory on each scale emerges from the theory on the next-finer scale by ignoring some of the (irrelevant) details of the latter. And the ontology of the theory on each scale — in particular, its "unobservable" theoretical entities — can be understood, at least in principle, as arising from the "collective" or "emergent" effects of a more fundamental theory at a finer scale.

Since no existing theory purports to be a final theory, there is no reason to consider it as literally true or to worry too much about whether the entities it postulates "really exist". Or rather, when worrying about whether the unobservable entities of a given theory "really exist", it is important to distinguish existence *as a fundamental constituent of the universe* from existence *in some coarse-grained sense*. It is a reasonable guess that *none* of the theoretical entities in our present-day theories are truly fundamental, and that *all* of the theoretical entities in our present-day well-confirmed theories will maintain some status as derived entities in future theories.

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at very small scales *cannot* be a smooth manifold. Unfortunately, the *direct* contradiction between general relativity and quantum mechanics becomes evident only at scales of order 10^{-33} centimeters and smaller — i.e. sizes about 10^{25} times smaller than an atom — or, equivalently, at energies about 10^{16} times higher than that of the Superconducting Supercollider (R.I.P.). Clearly, this realm will have to be probed *indirectly* if it is to be probed at all.

[65] See Weinberg (1992) and Bohm (1984 [1957], chap. 5) for in-depth discussions of this issue, reaching different conclusions.

[66] For a non-technical introduction to the renormalization group, see Wilson (1979).

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