Scientific realism is dead, or so many philosophers believe. Its death was announced when philosophers became convinced that one can accept all scientific results without committing oneself to metaphysical existence claims about theoretical entities (Fine 1986, 112). In addition, the inability of self–proclaimed scientific realists, despite recurrent demands, to distinguish themselves from their rival anti–realists (Stein 1989) didn’t exactly help their cause. If realists cannot identify the key feature or features that set them apart from their opponents, then there is really no need to conduct a debate on scientific realism, is there?

Feasting on the corpse, pragmatism soon won over the philosophy of science. If the entities of our best scientific theories do not genuinely refer to any deep underlying ontology, and our acceptance thereof is not dependent on such metaphysical chimeras like “truth” and “reality”, then we could believe in any theory which is useful to us. After all, this is what we mean when we say that “truth is that which works” (James 1907, 8).

Partly for independent reasons that shall become clear towards the end of this book, and partly for philosophy’s own sake as a discipline, I strongly believe that it is time to revive scientific realism and to present an alternative to pragmatism. Such revival, however, cannot be done by repeating past mistakes. If the debate on scientific realism is to be reconstructed, it must be so on bases different than those which led to its demise. What follows is an attempt to do just that. But prior to embarking on this ambitious task, I would like to open with an abridged chronicle of scientific realism’s death. My aim is to demonstrate that this death is an artifact of the way the debate has been commonly construed. Hence the main thesis of this opening chapter: that all the definitions of scientific realism that have been entertained up until now were doomed to fail from the start, in the sense that by holding to them scientific realists were in principle unequipped with the adequate tools for distinguishing themselves from their anti–realists opponents. No wonder, then, that the debate has generated an ennui among philosophers (Blackburn 2002, 112) and non–philosophers (Gal 2002, 523) alike. The writing, as it were, was on the wall, and it is time to clean it up and to start a–fresh.

I. IN SEARCH OF A GOD–EYE VIEW

Let us begin with what the debate is not about. It is a working hypothesis of this book (and, indeed, of anybody else other than Bishop Berkeley and his followers) that the external world exists independent of mind. This sort of realism, usually referred to as metaphysical realism, or \( R_0 \) henceforth, is so general and so basic, that it is presupposed, or so I shall assume, by all participants of the debate on scientific realism, no matter which camp they belong to.

Unfortunately, it is not as easy to decide what the debate is about. While most scientific realists have some vague intuitions about what their views are, in most cases they seem to wait for an anti–realist to make these intuitions precise for them. Such is the case, for example, with the first tentative definition of scientific realism we shall explore here, given by van Fraassen (1980, 8):

\[ R_1: \text{Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true.} \]

What \( R_1 \) amounts to, basically, is a twofold claim about ontology and epistemology. First, from an ontological perspective, that scientific theories are “literally true” means that the theoretical entities they incorporate actually exist, otherwise there would be no truth–makers for the theoretical claims; no specific reality to which they correspond. Second, from an epistemological perspective, by conditionalizing the “acceptance of a theory” upon the “belief in its truth”, realists who hold to \( R_1 \) are immediately compelled to define their view within the realm of theory–choice, where justification becomes a major issue, and skepticism prevails.
Anti–realists, on the other hand, are satisfied with much less than $\mathbf{R}_1$. For them, science aims to produce theories which are only empirically adequate (van Fraassen 1980, 12). No commitment to the “truth” of the theory is enunciated, nor do anti–realists rely on such beliefs in accepting or rejecting a scientific theory. Empirical adequacy is all that matters, and consequently no God–eye view is required.

But how can one distinguish between the notion of “empirical adequacy” and the notion of “truth”? In other words, how can proponents of $\mathbf{R}_1$ justify their belief in the truth of their pet theory? Do they have an epistemically privileged vantage point they can rely on?

One might try to escape the impasse that awaits the scientific realists by pointing that, as it stands, $\mathbf{R}_1$ is too naïve. What saves $\mathbf{R}_1$, or so the story goes, is its normative flavor, and the appreciation that science is an ongoing endeavor. We are not there yet, tell the scientific realists to their impatient colleagues (Popper 1963), but just stay tuned, and we will some day arrive to the mystical Archimedean point that captures “reality” as it “really” is.

But clearly this is all wrong. No vantage point outside science exists, and the scientific realists, if they accept $\mathbf{R}_1$, saddle themselves with an unfortunate vicious circle. For in order to distinguish themselves from their anti–realist opponents they must supply additional sufficient conditions—over and above the minimal and necessary condition of empirical adequacy—for the notion of “truth” they are committed to, otherwise they will end in collapsing the two opposing views into one, and no debate can ensue:

What of then of the realist, what does he add to his core acceptance of the results of science as really true? . . . [W]hat the realist adds on is a desk–thumping, foot–stumping shout of “Really!” So, when the realist and the anti–realist agree, that there really are electrons and that they really carry a unit negative charge and really do have small mass (of about $9.1 \times 10^{-28}$ grams), what the realist wants to add is the emphasis that all this is really so. “There are really electrons, really!” (Fine 1986, 129).

Yet, as Fine makes clear in this nicely written piece, the only way they can supply such additional conditions is by achieving the in principle unachievable God–eye view. Game over.

The seeds of destruction are thus already sawed. Indeed, here is a recipe for demolishing a philosophical view. First define it in such a way that will depend on a vague, albeit intuitive and apparently common-sensical, notion (“truth”). Then deflate that notion, keeping the minimal condition your opponent will agree with (“empirical adequacy”), and turn it into your own. Now roll your eyes to the sky and tell your opponents that unless they can provide you with additional conditions, over and above the minimal one you present them with, for the definition that they prefer, then there is no point in continuing the debate, as their view, by now, has collapsed into yours. But of course, by accepting the way the debate has been construed, your opponents have already agreed from the outset to play with their hands tied. No wonder you knock them dead after the first round.

There is, of course, a long tradition that, in the absence of a privileged vantage point, seeks additional, extra–empirical and extra–logical, conditions as ‘markers’ for “truth” (e.g., Howard 1998, 147–154). It is sometimes claimed that parsimony or simplicity are such ‘markers’, and now the debate can ensue on the relative weights these ‘markers’ carry, if at all, and on the extent to which they can be rationally applied in the domain of theory–choice.

But such a way–out just shifts the source of the skepticism from the notion of “truth” to the notion of “rationality”. Yet this shift inevitably leads, and indeed has led philosophy again and again, into a dead–end (Laudan 1990). For what sort of arguments can one bring in support of one’s preference in this or that external ‘marker’? Why is choosing one over the other more rational? Or maybe, as Kuhn (1962) has argued, all choices are equally a–rational?

Taking stock, two problems has surfaced from our first attempt to define scientific realism. The first is that scientific realists must find a way to distinguish themselves from their anti–realist opponents. The second is that by defining scientific realism as an epistemological thesis about belief and theory–acceptance within the context of theory–choice, scientific realists immediately make it impossible for them to solve the former.

II. MIRACLES AND SUCCESS

Scientific realism, if defined as an epistemic stance, leads to nowhere, but at least it exemplifies an urgent need to authenticate the debate by demarcating itself from anti–realism. As this realization sunk within the philosophical community, attempts to fulfill the demarcation requirement have converged on what seemed like a brilliant idea: that rather than an epistemic stance, scientific realism should be defined as an empirical hypothesis that explains the success of science, the claim being that any philosophy of science other than $\mathbf{R}_1$ turns the success of science into a miracle.

This kind of shift in perspective resonated well with the increasing tendency to “naturalize” philosophy. Indeed, if natural science should be our guide in matters metaphysical, why can’t we apply it also to philosophical doctrines? The result, to paraphrase Putnam (1978, 21) is a definition of realism that amounts to the following:
The fact that statements about reference or about approximate truth function in the explanation the phenomenon of the progress and the success of science, establishes that the notions of "truth" and "reference" have a causal explanatory role in epistemology.

This abductive inference from the success of science (as an empirical phenomenon) to the hidden mechanism that explains it (scientific realism defined as \( R_1 \)) was baptized as "the no-miracle" argument.

Now there are certainly several immediate problems that arise from \( R_2 \), and philosophers were quick to identify them. But before we explore these problems, it is instructive to point out several drawbacks in \( R_2 \) that philosophers seem to have missed.

Recall that the lesson we have drawn from \( R_1 \) was that scientific realists were having a hard time in distinguishing themselves from their opponents. If the definition of "truth" is what they want to anchor the debate on, then they had better supply additional conditions to the minimal and necessary condition of "empirical adequacy" that is used by their anti-realist rivals. The problem with \( R_1 \) is that, as an epistemological standpoint, it blocks in principle such an attempt and makes it impossible to meet the anti-realist challenge. The problem with \( R_2 \) is that, although it changes \( R_1 \) from an epistemological stance to an empirical hypothesis, it is not at all clear that this shift helps scientific realists in meeting the anti-realist challenge any better than \( R_1 \) does.

In effect, \( R_2 \), elegant as it is, doesn't really fulfill the demarcation challenge that \( R_1 \) left unmet. For even if one buys the abductive inference it alludes to (and there are good reasons to believe that such an inference will not be generally convincing, as we shall see soon), one must still face objections of the sort Fine makes above about 'table thumping' and 'foot stomping'. So while \( R_2 \) seems to have bypassed the epistemological barrier set by its predecessor—after all, or so its proponents argue, it uses nothing but science as means to gain access to "reality"—this shift doesn't change in one bit the fact that in order to hold to a notion of "truth" distinct from "empirical adequacy" one must adopt the unattainable God–eye view.

Things, however, are much worse. For while this lacuna in \( R_2 \) could have sufficed to forestall any debate on scientific realism from ensuing, the no-miracle argument soon met with other fierce objections. These, however, just exposed the fact that, very much like his predecessor, \( R_2 \) was doomed to fail from the start.

III. PESSIMISM, AGAIN

That the progress or the success of science, if regarded as empirical phenomena, require explanation, and that \( R_1 \) can serve as the best explanation for these phenomena is the essence of \( R_2 \). Such a reformulation of scientific realism clearly invites criticism. First, has science really been successful and has it really progressed? Second, if so, why is \( R_1 \) the best explanation for these phenomena? And third, why should one apply inference to the best explanation to begin with in choosing between different alternative theories of theory-choice if one is reluctant to rely on such a method in choosing between alternative theories simpliciter?

In one sense the answer to the first question is straightforward: modern science has progressed a lot since the ancient Greeks, hasn't it? But for an answer more precise one must first define "progress", and such a generally-accepted and non-question-begging definition is hard to find. This is yet another open question in the philosophy of science (Kitcher 1993, 90–177).

For example, one can define "progress" using a notion of "approximate truth" (Rawbottom 2008), only to face the obvious question about how to measure such an approximation of truths without having access to any sort of privileged, transcendental, outlook on reality. While proponents of \( R_1 \) would find such a question unproblematic, anti-realists would be less accommodating here, and consequently less convinced of \( R_2 \). Another option is to define "progress" in terms of accumulation of scientific knowledge (Bird 2008). Since science is the institutional embodiment of the desire to know, it makes progress precisely when that desire is achieved. Yet while this definition fares better than the former on independent epistemological grounds that distinguish "knowledge" from "accidental true belief", it is still unclear whether it does any better than the former in convincing those who are not already converted.

Clearly these two options are not exhaustive, and there is much more to be said on the subject, but for now suppose that everybody does agree that, at least on some vague yet intuitive view, science has indeed progressed. The second question we need to address is how did it make such a progress.

Proponents of \( R_2 \) would like us to believe that the only way science could have progressed was by latching onto the true nature of the world, by which they mean that theoretical terms in mature scientific theories genuinely refer and consequently that these theories are true. At this point one might take seriously the claim that \( R_2 \) is an empirical hypothesis and actually test it with data from the history of science. Alas, when put to the test it turns out that this hypothesis is false! Indeed, many false theories were deemed successful at the time (Laudan 1981), so unless one defines, again, "success" in terms of "truth" (and that is, as we have just seen, something one would like to avoid if
one wishes \( R_2 \) to be *generally* convincing), then one cannot extrapolate from the history of theory–choice and make the inference from the success of our current theories to their truth.

This assault on the no–miracle argument was baptized as the “pessimistic meta–induction”. It can seen as a constraint that forces any would–be scientific realist to find both a form of scientific realism and a definition of scientific progress such that the resulting no–miracle argument is sound.

One way to mitigate this constraint (Magnus & Callender 2004) is to note that, based on inductive inference as it is, the strength of \( R_2 \) can be quantified according to the data it relies upon. While such a defense may carry some force, it is still too weak to forestall an even stronger argument against \( R_2 \) which attacks its *validity* rather its *soundness*.

Recall that the inference from the success of science to the truth of our scientific theories is, according to \( R_2 \), inference to the best explanation. But isn’t it the case that those who accept inference to the best explanation in the level of scientific theories are exactly those who will now accept it the level of methodology, and *vice-versa*?

It is little short of remarkable that realists would imagine that their critics would find [the no-miracle arguments] compelling. . . . [E]ver since antiquity critics of epistemic realism have based their scepticism upon a deep–rooted conviction that the fallacy of affirming the consequent is indeed fallacious. . . . Now enters the new breed of realist... who wants to argue that . . . realism can reasonably be presumed to be true by virtue of the fact that it has true consequences. But this is a monumental case of begging the question (Laudan 1981).

Laudan is correct, of course, but rather than pointing at a drawback of scientific realism *per se*, his argument simply demonstrates the inevitable dead–end that awaits *any* formulation of the debate on scientific realism in terms of truth, reference, and as by now has become clear, also *theory–choice*. In fact, while Laudan’s critique is well–known, what most philosophers have missed, is that the circularity it points at is just an artifact of the way the debate has been construed.

For if one accepts that scientific progress is an *empirical* phenomenon, a phenomenon that is to be explained by some hidden model of theory–choice, then it is obvious that one could choose between any arbitrary number of such possible models, inference to the best explanation being just one of these. But when the alternative models are *themselves* theories for theory–choice, or methodologies, there is simply no way, on pain of inconsistency, that one would apply one type of model in choosing between (empirical) scientific theories, and another type of model in choosing between (empirical) methodologies. After all, one has just agreed that the latter choice is to be based on empirical facts. Thus any argument against the circularity that is latent in \( R_2 \) is basically an argument against the premise that scientific progress is an *empirical* phenomenon. But since Laudan has accepted this premise at the outset, he must accept its inevitable conclusion.

In fact, the same argument that Laudan is quick to marshal against \( R_2 \) applies to *any* theory of theory–choice if scientific progress is deemed an empirical phenomenon, be that methodology \( R_1 \), Bayesianism, curve fitting, or what have you. As such, rather than exposing a weakness in \( R_2 \), the argument simply demonstrates that the debate on the revised version of scientific realism was *again* doomed to fail from the start.

**IV. IN THE BEGINNING WAS STRUCTURE**

By now one would have thought that the philosophy community would call for a drastic revision of the rules of the game, and abandon the arena of theory–choice altogether as the domain in which debates on scientific realism are to be construed. And yet, instead, a peaceful surrogate has entered this very arena, promising to supply us with the best of both worlds by accommodating *both* the pessimistic meta–induction and the no–miracle arguments (Worrall 1989).

Those who tend to be skeptic of “third way” arbitrators might waive such a suggestion as a *Non sequitur*. Yet they might want to reconsider their skepticism when they learn that, in effect, even respected figures who are commonly regarded anti–realist such as Poincaré were highly appreciative of the psychological thrust of arguments such as the no–miracle argument:

> Have we any right, for instance, to enunciate Newton’s law? No doubt numerous observations are in agreement with it, but is not that a simple fact of chance? and how do we know, besides, that this law which has been true for so many generations will not be untrue in the next? To this objection the only answer you can give is: It is very improbable (Poincaré 1905, 186).

Of course, such realistic hopes evaporate as soon as one encounters the history of science, and Poincaré realized that long before the phrase “pessimistic meta–induction” was coined (Poincaré 1905, 160), but rather than agonizing about the stalemate, Poincaré offered an optimistic alternative.
The idea seems to be the following. A reasonable way to accommodate both the pessimistic meta-induction and the no-miracle arguments can be found if one is willing to redefine $R_1$ once again, albeit still within the realm of theory-choice, but now as an epistemic stance towards mathematical structures rather than theoretical entities. After all, the thrust of the pessimistic meta-induction was that entities that once were thought to be genuinely referring had turned out to be empty as the theories they were inhabiting turned out to be false. In order to restore invariance under theory-change one must identify a theoretical feature that would be preserved, as it were, under the dynamical flow of the scientific practice. Such a feature can be found, says Poincaré, in the mathematical structure of our scientific theories.

A paradigmatic example for the invariance of the mathematical structure under theory-change is the shift from Fresnel theory of light to Maxwell’s electromagnetism, along which the view of the ether as an elastic solid was overthrown (notwithstanding Maxwell’s hopes to reduce, somehow, the electromagnetic field into a mechanical substratum). With the “field” accepted as a primitive entity, light became viewed as a periodic disturbance, not in an elastic medium, but in the disembodied electromagnetic field. Yet while the ontology wildly changed, the mathematical structure, that is, the form of the theories, remained intact.

Consequently, while Fresnel was completely wrong about the nature of light, it is no miracle that his theory was successful, as he did identify the correct structure of light, encapsulated in his equations (Warrall 1989, 117). These equations . . .

... [T]each us now, as they did then, that there is such and such a relation between this thing and that; only, the something which we then called motion, we now call electric current. But these are merely names of the images we substituted for the real objects which Nature will hide for ever from our eyes. The true relations between these real objects are the only reality we can attain, and the sole condition is that the same relations shall exist between these objects as between the images we are forced to put in their place. If the relations are known to us, what does it matter if we think it convenient to replace one image by another? (Poincaré 1905, 161).

Another example for the invariance of structure under theory-change comes from the transition from Newtonian mechanics to special relativity, theories famously regarded as “incomensurable” by Kuhn and Feyerabend. While there might be a meaning variance in the shift from the concepts of the former to the concepts of the latter (e.g., mass), this variance is anticipated by the shift in the group of transformations. The Galilean group is different than the Lorentz group, hence the objects that remain invariant under these transformation are different, but from a structural perspective these objects are still particular examples of a wider, abstract, family of invariant objects in possible geometries. In this case the structure that remains invariant under theory-change is the transformation group—a structure even more abstract than the differential equations Poincaré refers to above.

Poincaré’s insights have thus led to the following revised version of scientific realism, known as structural realism:

$R_3$: The fact that statements about mathematical structure function in the explanation the phenomenon of the progress and the success of science, establishes that the notion of mathematical structure has a causal explanatory role in epistemology

In other words, in order to accommodate both the no-miracle and the pessimistic meta-induction arguments one has only to revise the former by shifting the referential anchor in $R_1$ from content into form, that is, from genuinely referring theoretical entities to mathematical structures.

V. TROUBLE IN PARADISE

The general ideology of structural realism has born some fruitful offsprings in several specific domains within the philosophy of science, e.g., the debate on the hole argument in the philosophy of spacetime (Mundy 1986). As a thesis about science, however, it faces serious difficulties, for it was soon realized that $R_3$ can be interpreted in two different ways.

On one hand one would like to restore continuity to theory-change, and consequently one must insist that the theoretical entities in one’s theories are just place-holders. This means that all one can know, or even care to know, about the world, is encapsulated in the mathematical structure of one’s theory. Call this version epistemic structural realism, or $R_{3e}$. On the other hand one would like to be worthy of the term “realist”, hence one must commit to some ontological entity. But if theoretical entities do not refer to real objects in the world (as argued by the pessimistic meta-induction), then the only ontology structural realists can commit to is the mathematical structure of the theory. Call this version ontic structural realism, or $R_{3o}$.

And the problem is that none of these versions of $R_3$ really help us in making any progress in the debate on scientific realism!
Here is why. The idea that all one can know about the world is its structure was already entertained by Russell in *The Analysis of Matter* (1927, 227; 254; 270), and elaborated by philosophers such as Maxwell (1962) and Lewis (1970). One way to formalize this idea, apparently unbeknownst to Russell, was to eliminate (unobservable) theoretical terms from a theory using Ramsey’s (1931) method which (in a theory formulated with a first–order language) replaces those terms with a collection of existentially–quantified bound predicate variables (Ladyman 1998, 411).

What the Ramsey’s method does is that it allows a theory to assert that there are some objects, properties and relations that satisfy certain implicit definitions. Their existence is known not through direct reference (for they are unobservable) but through their description, or their logical form. On this view, which closely follows Poincaré’s intuition, the world might be composed of unobservable entities between which certain relations obtain; but all we can know is the structure of these relations (Ladyman 1998, 412).

Yet as was pointed out to Russell by Newman (1928), and to those who aspired to revive his $R_{3_E}$ by Demopoulos & Friedman (1985) and Demopoulos (2007), this view fails to meet the challenge of demarcating realism from anti–realism as soon as one realizes that it basically trivializes $R_1$ and collapses it into scientific anti–realism.

For if all one can know is that there is a relation $R$ such that the structure of the world with reference to it is $W$, then since any collections of objects can be organized as to have the structure $W$, provided there are the right number of them (Newman 1928, 144), only cardinality questions are open for discovery, and any other claim about the world that can be known at all can be known a–priori as a logical consequence of the existence of a set of objects with such and such cardinality. In other words, $R_{3_E}$ turns almost all statements about the world into pure definitions, true as a matter of logic.

It is important to be clear on the nature of the problem (Demopoulos & Friedman 1985, 628). It is not a drawback of $R_{3_E}$ that it cannot distinguish between possible abstract models that multiply realize $W$ (e.g., the inability to pick up the domain of objects on which a model for our theories of the world is to be defined—think of pure and applied geometry where we have to distinguish between different relations on different domains, i.e., real numbers vs. rigid bodies). The problem arises because after we have picked a domain, we still cannot “distinguish between systems of relations that hold among the members of a given aggregate” (Newman 1928, 147).

But the trivialization of Russell’s epistemic structural realism leads to phenomenalism since the truth of a physical theory is now reduced to the truth of its observational consequences: if every assertion about the physical world is trivially true (that is, true as a matter of logic plus an empirical assumption about cardinality), then these assertions are implied by every proposition, in particular propositions about perceptions. And since the phenomenalists’ thesis is that propositions about the external world are reducible to propositions about perceptions, $R_{3_E}$ (modulo a single non–logical assumption about cardinality) guarantees this thesis (Demopoulos & Friedman 1985, 631).

By now the impasse is clear, as we are back in square one: *all* theories with the same observational consequences will be (trivially) equally true (Demopoulos & Friedman 1985, 635). The notion of “truth” collapses again into “empirical adequacy”.

$R_{3_E}$ leads, again, to nowhere. What about $R_{3_o}$? Unfortunately, also here the prospects are dim. To begin with, it is hard to imagine what the ontological thesis that all that there is, is structure, means—at least as hard as grasping a grin of a Cheshire cat without the cat (Carroll 1865, Chapter 6). And while “unimaginability” cannot be used as an argument against metaphysical theses, additional arguments are not hard to find. The deadliest of these stems from Newman’s observation above, and focuses on the indeterminacy of structure. Here is how it goes. Proponents of $R_{1_o}$ talk about the structure, but such talk is misleading (and in fact meaningless). Since one can identify structure only up to isomorphism, and since, as Newman argues, a given domain can have any structure whatsoever (Psillos 2006, 562), then in order to make sense of the structuralist claim above, one must postulate that among the structures that can characterize a domain, some are privileged. But such a privileged structure depends on certain non–structural notions, i.e., properties and relations which are imposed on the domain, and this obviously contradicts the claim that all there is, is structure...

The upshot is that $R_{3_o}$ makes it impossible for scientific realists to distinguish themselves from their anti–realist opponents, and $R_{3_{o_E}}$ apart from its weirdness, is either meaningless, or, if meaningful, then inconsistent.

The conclusion is, unfortunately, that scientific realism, defined as it was in the context of theory–choice, simply couldn’t make a difference. It fails to distinguish itself from its rivals as an epistemic thesis; it fails as an empirical hypothesis; and the surrogate that was thought to bring remedy to this unfortunate situation simply breaks down. No wonder that philosophers are no more certain about the authenticity of this debate.

VI. CHRONICLE OF A DEATH FORETOLD

This is the sad story of philosophy gone astray. As in many other cases in the history of philosophy, first we raise dust, and then we complain that we cannot see. The chronicle presented here is indeed abridged, but even it suffices to expose the fact that the debate on scientific realism, construed as it was in the domain of theory–choice,
and relying as it was on epistemological or semantic notions of “truth”, “belief”, and “reference” was a stillborn from its very conception.

This failure, I claim, is not an intrinsic feature of scientific realism per se. Rather, it is nothing but the artifact of the way the debate has been construed. Indeed, why should one pick theory-choice as the domain to conduct methodological debates in the first place? After all, if one looks at the history of science, it is not the case that scientists “accept” or “reject” theories in such a simplistic way that was presupposed, e.g., by Kuhn (1962). And it is also quite clear that whatever criteria one may choose for the rationality behind this or that choice, it will be extremely hard, if not impossible, to convince others in the uniqueness of that criteria. For all we know, rationality might even be a cognitive, psychological, process with no absolute foundations, to the extent that the debate on its status would soon resemble the futile debate on the status of “truth” we have been trying to escape all along.

Theory-choice, as hopefully this chapter has succeeded in conveying, is thus the worst arena for defining scientific realism, and if we are still interested in this debate and in a viable alternative to pragmatism, we must reconstruct both in a completely different context. This context, as I shall argue in the rest of the book, is the context of theory-construction. As we shall see, it is here where scientific realists can make a difference by distinguishing themselves from their anti-realist opponents, notwithstanding their acceptance of the crucial insight that emerged from the current futile debate, namely, that the only epistemic vantage point we have on “reality” is the scientific one. For we shall identify a major dichotomy in the practice of theoretical physics; a genuine divide between “truth” and “empirical adequacy” which doesn’t fall prey to the traps we have exposed above, and which leads to different types of theoretical practices that, or so I shall argue, exemplify best what the debate between scientific realists and anti-realists is all about.

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