Chapter 1
Are There Good Arguments Against Scientific Realism?

Paul Hoyningen-Huene

Abstract I will first discuss a peculiarity of the realism-antirealism debate. Some authors defending antirealist positions in a philosophical discussion seem to be inconsistent with what they do when treating scientific subjects. In the latter situation, they behave as realists. This tension can be dissolved by distinguishing different discourses belonging to different levels of philosophical radicality. Depending on the respective level, certain presuppositions are either granted or questioned. I will then turn to a discussion of the miracle argument by discussing a simple example of curve fitting. In the example, multiple use-novel predictions are possible without indicating the truth of the fitting curve. Because this situation has similarities with real scientific cases, it sheds serious doubt upon the miracle argument. Next, I discuss the strategy of selective realism, especially its additional crucial component, the continuity argument. The continuity of some X in a series of theories, with X being responsible for the theories' use-novel predictions, is taken to be a reliable indicator for the reality of X. However, the continuity of X could as well be due to the similarity of the theories in the series with an empirically very successful theory embodying X, without X being real. Thus, the two main arguments for scientific realism show severe weaknesses.

Keywords Miracle argument · Use-novel predictions · Continuity argument · Selective realism · Structural realism

P. Hoyningen-Huene (✉)
Institute of Philosophy, Leibniz University of Hanover, Hanover, Germany

Department of Economics, University of Zurich, Zurich, Switzerland
e-mail: hoyningen@ww.uni-hannover.de

A. Christian et al. (eds.), *Philosophy of Science*, European Studies in Philosophy of Science 9, https://doi.org/10.1007/978-3-319-72577-2_1

1.1 Introduction

There is a plausible *prima facie* answer to the title question whether there are good arguments against scientific realism, which simply is no! The source for this answer is the ubiquitous behavior of scientists, more specifically of physicists: they are usually straightforward realists when it comes to discussing scientific results. Good physicists have a solid education, are usually diligent, rational, intelligent, and self-critical people (at least as long as they talk science, not necessarily when they talk *about* science). Here is an example from recent, very topical science (suspect of earning some of its authors a Nobel Prize in physics). The upper half of Fig. 1.1 represents data that were measured on September 14, 2015 and published on Feb 11, 2016 (Abbott et al. 2016). The interpretation of these data is summarized in the conclusion of the paper:

> VIII. CONCLUSION
> The LIGO detectors have observed gravitational waves from the merger of two stellar-mass black holes. The detected waveform matches the predictions of general relativity for the inspiral and merger of a pair of black holes and the ringdown of the resulting single black hole. These observations demonstrate the existence of binary stellar-mass black hole systems. This is the first direct detection of gravitational waves and the first observation of a binary black hole merger.

The language of this conclusion (and of the whole body of the paper) is uncompromisingly realist: they "have observed gravitational waves", "the existence of binary stellar-mass black hole systems" is demonstrated, gravitational waves have been "directly" detected, and "a binary black hole merger" has been observed for the first time. There is no talk of or any argument for the given *realist* interpretation of the data: no other possibility is mentioned, let alone explicitly discarded based on some argument. Therefore, for the physicists involved – more than 1000 figure as authors

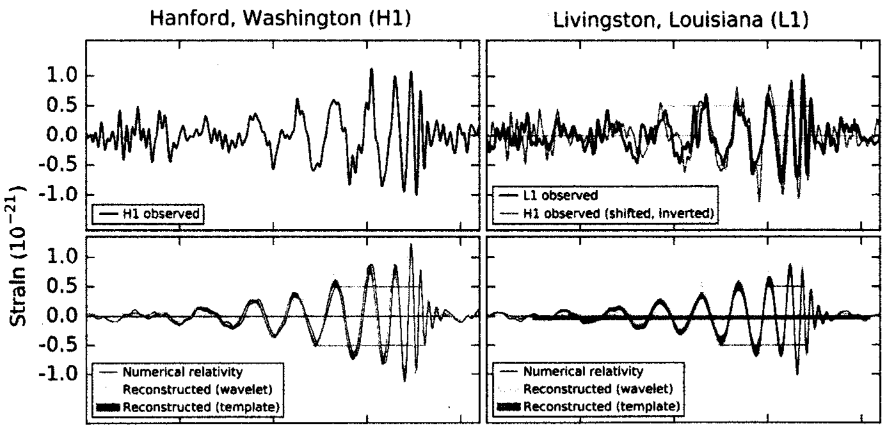

Fig. 1.1 Data for gravitational waves

of the paper – the case seems clear: they just detected really existing gravitational waves and observed the really existing merger of a pair of really existing black holes. Any argument for the evidently realist interpretation of the data is lacking. This suggests that the authors deem such an argument just totally unnecessary. If we stick to the hypothesis that this group of physicists is a bunch of fairly rational people, we must conclude that there simply are no serious arguments against the realist stance taken up in the paper, otherwise these arguments would have been confronted (and possibly disproved). Therefore, in the view of physics, as implicitly contained in the cited paper, the case seems clear: there are no serious arguments against scientific realism.

However, there seem to be serious dissenting voices: there are quite a few statements by (theoretical) physicists and chemists exactly to the contrary. A prominent example is Stephen Hawking:

> I take the positivist viewpoint that a physical theory is just a mathematical model and that it is meaningless to ask whether it corresponds to reality. All that one can ask is that its predictions should be in agreement with observation.[1]

This statement seems to be in blatant contradiction with the realist stance of the physicists who discovered gravitational waves. Is Hawking perhaps fundamentally different from these physicists? It may come as a surprise that he is not, at least as far as black hole physics and gravitational waves are concerned. In the context of the 1960s and 1970s discussion about the possible detection of gravitational waves, Hawking published a paper entitled "Gravitational radiation from colliding black holes" (Hawking 1971). Its abstract reads:

> It is shown that there is an upper bound to the energy of the gravitational radiation emitted when one collapsed object captures another. In the case of two objects with equal masses m and zero intrinsic angular momenta, this upper bound is $(2-\surd 2)\,m$.

Hawking refers to "gravitational radiation emitted" and "collapsed object[s]" (i.e., black holes), and there is no sign in the paper that these things are only calculational devices with no reality content, as one would expect from an instrumentalist. Instead, he speaks about them in the same language as one speaks about any ordinary real physical object. Hawking's stance in this paper is thus purely realist. However, what shall we make of this apparent contradiction between a purely realist and a radically instrumentalist stance occurring in the same author?

[1] Hawking (1996, 3–4). See also the very clear statement of the consequence of his positivism later in the book: "[Penrose] is worried that Schrödinger's cat is in a quantum state, where it is half alive and half dead. He feels that can't correspond to reality. But that doesn't bother me. I don't demand that a theory correspond to reality because I don't know what it is. Reality is not a quality you can test with a litmus paper. All I'm concerned with is that the theory should predict the results of measurements" (Hawking and Penrose 1996, 121). See also Hawking and Mlodinow (2010, esp. Chapter 3).

1.2 Levels of Philosophical Radicality

My suggestion is that we should distinguish, in somewhat fashionable terminology, different discourses, or: ways of reasonably discussing things (or "language games"). I shall describe the differences between these discourses as differences in the levels of their *philosophical radicality*. There is a ground level, or level zero, of philosophical radicality in which nothing is put into question for *philosophical* motives. On this level, nothing is doubted beyond what is doubted in normal scientific practice (or in everyday discourse, for that matter). For instance, in cutting edge scientific discourse about new hypothetical objects, many things are taken for granted, for instance realistically interpreted established theories and those parts of the experimental equipment that have been exhaustively tested. "Taken for granted" only means that these things are not questioned *in the given context* which does not, of course, exclude their being questioned in other contexts, be it scientific or philosophical contexts (more on the latter see below). For instance, in the recent discovery of gravitational waves and of inspiraling black holes, it was taken for granted (among many other things) that the theory on which the design of the lasers was based was correct, that the data that the two detectors produced were the result of optical interference, and that General Relativity Theory was the right theory to interpret the data (see Abbott et al. 2016).[2] The question in focus was the existence of gravitational waves and, for any particular case, their concrete sources. Clearly, this is a thoroughly realist stance: the pertinent scientific theories are interpreted realistically, and the question is whether gravitational waves really exist and what their sources are. Thus, the enterprise is a purely scientific one, devoid of any *additional* philosophical questioning.

In the given context, the first level of philosophical radicality is reached by questioning *in general* the step to a realist interpretation of scientific theories. This is what the standard philosophy of science discussion about scientific realism is all about. In this case, our knowledge of observable macroscopic objects as real objects is typically taken for granted. The question being asked is this: Are we in general justified to assume the existence and properties of those unobservable

[2]A referee of an earlier version of this paper objected to my description of level zero of philosophical radicality that "it is entirely legitimate for a scientist to question background theories in order to draw into doubt a conclusion like the detection of gravitational waves. Double checking and questioning scientific background assumptions fully plays out at the scientific level and constitutes an important element of scientific reasoning." No and yes. For instance, doubting the putative detection of gravitational waves on the basis that the use of Maxwell's equations should be questioned for the calculation of interference patterns would be far from being "entirely legitimate", as the referee has it. Although this doubt is not excluded as a matter of principle, in a series of steps of critically checking the experiment this particular step would come rather late. "Double checking and questioning scientific background assumptions" not referring to accepted fundamental theories, however, is a completely different matter. Of course, I never meant to deny the legitimacy of a critical scientific discussion of assumptions of this kind on level zero of philosophical radicality.

objects that our mature and well-confirmed theories about the pertinent domain postulate, based on our observations of macroscopic objects? A positive answer to this question is (roughly) the position of the scientific realist. Someone who denies the legitimacy of this step to a realist interpretation of well-confirmed mature theories is a scientific anti-realist, or instrumentalist. Clearly, the question about the *general* legitimacy of realist interpretations of well-confirmed mature theories is more radical than the zero level question about the legitimacy of the realist interpretation of a given *individual* theory. The former question is a philosophical question, the latter a scientific one. Clearly, on level zero, i.e., in the scientific context, the general legitimacy of realist interpretations of theories (under appropriate conditions) is taken for granted. In other words, the general *philosophical* doubt about realist interpretation articulated on level one does not come into play in the scientific practice on level zero.[3] The situation is similar to the situation we are confronted with by "the" problem of induction. Philosophers (since Hume) have asked the question of the legitimacy of inductive generalizations in general (level one). Scientists, by contrast, take the possibility of inductive generalization under appropriate conditions for granted and ask in any particular case, whether the conditions for a valid inductive generalization are met (level zero).[4]

One can push philosophical doubt even beyond level one of philosophical radicality, although this is much less fashionable in current philosophy of science. The main assumption of the first level of philosophical radicality is that we have knowledge of observable macroscopic objects. If one is a scientific anti-realist on the first level, one may extend one's doubt about the epistemic accessibility of unobservable objects to observable objects as well (this is of course but one route to this higher level of philosophical radicality). Thus, on this second level of philosophical radicality, the existence of and our epistemic access to macroscopic observable objects is questioned. Roughly, this is the level of philosophical radicality on which sense data based philosophies, Kant, perspectival realism, and model-dependent realism, among others, operate.[5] These philosophies question the

[3]I note in passing that in the history of philosophy, others have seen this difference also. For instance, Edmund Husserl denoted it as a difference between the "natural standpoint" and the "critical epistemological standpoint"; see Husserl (1967 [1922], §§ 27ff).

[4]With respect to the scientific realism debate, the above distinction between levels zero and one of philosophical radicality has been articulated somewhat differently in Magnus and Callender (2004). They distinguish "retail arguments for realism (arguments about specific kinds of things such as neutrinos, for instance) from wholesale arguments (arguments about all or most of the entities posited in our best scientific theories)" (321). Clearly, this distinction is very similar to the one proposed above. However, what is missing from my point of view in Magnus and Callender's version of the distinction is the explicit reference to the correlated difference of epistemic stances, here called different levels of philosophical radicality. Only the difference in the epistemic stances reveals the possibility to defend seemingly inconsistent positions at the different levels; see below.

[5]For perspectival realism, see, e.g., Giere (2006); for model-dependent realism, see, e.g., Hawking and Mlodinow (2010). It seems to me that these two positions are essentially identical.

givenness, or pure object-sidedness, of unitary observable macroscopic objects and propose to investigate the constitution of these objects, i.e., the contribution of subject-sided elements. Obviously, different philosophical positions may result from this questioning the apparently unquestionable pure object-sidedness of observable things. Pushing philosophical radicality even further, one may reach Cartesian skepticism (from which position it seems very difficult to move anywhere).

It should be noted that the different levels of philosophical radicality are not uniquely defined. Neither do I claim that a certain level structure of philosophical radicality in one area of philosophy can be immediately transferred to another area, say from philosophy of science to ethics. There, the levels of philosophical radicality may take on forms different from the ones in philosophy of science. The essential point is that at some level n, certain things are taken for granted, whereas at level n + 1, they are questioned. To move from one level to another, i.e. to participate in discourses situated at different levels, is not inconsistent.[6] Each level determines a certain discourse by fixing certain things as given and beyond dispute – for the sake of argument, or of conviction. A discourse determined in this way may be interesting or uninteresting, depending on one's goals and convictions. For instance, in order to understand certain every day or certain scientific practices, one should be aware of being at the zeroth level, whereas certain philosophical questions necessarily involve a move to a higher level of philosophical radicality. As I have illustrated above by the example of Steven Hawking, the same person can work at both levels – as all antirealist philosophers do when it comes to normal everyday affairs, were they typically do not doubt the existence and cognizability of observable objects.

However, individual philosophers and scientists strongly differ in their willingness to engage with the various levels of philosophical radicality. The higher the degree of philosophical radicality, the further away from common sense one moves. If one uses the adherence to common sense as an argument against one's engagement with one of the levels beyond level zero, one should be conscious about this argument's persuasive force. It may be a convincing argument for those who think that with common sense one is epistemologically on a safer ground than with any mode of philosophical questioning that a particular higher level of philosophical radicality involves. However, for those defending the practice of philosophy on a higher level, the accusation of a deviation from common sense is certainly not persuasive. Quite on the contrary: for asking philosophical questions on a certain level of philosophical radicality above level zero is nothing but questioning certain common sense presuppositions. Thus for those philosophers, the refusal of engaging with that level of philosophical radicality is nothing but a refusal of philosophy itself.[7]

[6]Some defenders of common sense realism appear to assume the inconsistency of level zero and level one. See, for example, Richard Dawkins: "Show me a cultural relativist at thirty thousand feet and I'll show you a hypocrite": Dawkins (1995, 31–32).

[7]See, e.g., Rowbottom (2011) against the scientific realist philosopher Howard Sankey.

In the following, we will move beyond level zero. I shall investigate two arguments or strategies, respectively, which are standardly used in the defense of scientific and/or structural realism: the "miracle argument" and the "selective strategy".

1.3 The Miracle Argument

One of the most important arguments for scientific realism starts from an uncontroversial observation: science has been very successful repeatedly in producing novel predictions. The cases that are relevant for the argument must be described more carefully. "Predictions" in the given context are not necessarily predictions in the temporal sense, but are statements about observable putative facts that are derived from a certain hypothesis or theory. Typically, pertinent antecedent conditions and possibly other assumptions have to be included in the premises of the derivation. The "novelty" of the prediction means in the given context that the predicted data have not been used in the construction of the theory. For clarity, sometimes the expression "use-novel predictions" is used.[8] Here are two examples. In 1916, Einstein predicted the existence of gravitational waves as a consequence of his General Relativity Theory (GRT) (Einstein (1916), with corrections in Einstein (1918)). Gravitational waves were not used in the construction of GRT and were thus a use-novel prediction. Secondly, in 1927, Heitler and London derived from the newly developed quantum theory the existence of the covalent bond between hydrogen atoms (Heitler and London 1927). The covalent bond was well known at the time but unexplained in terms of physics, and it was not used in the construction of quantum mechanics. In this sense, the existence of the covalent bond was a use-novel prediction of quantum mechanics.

The question is, how are these use-novel predictions possible? How does a theory acquire the capability of predicting novel facts that the theory was not designed for? What exactly are the resources of a theory for such predictions? In other words, how can this particular success of theories be explained? Scientific realists have a plausible answer to these questions. Theories can produce correct use-novel predictions if they are approximately true, i.e., if their theoretical terms refer to real entities and if they get the properties of these entities at least approximately right. Thus, approximate truth of theories is *sufficient* for correct use-novel predictions. However, according to the scientific realist, approximate truth is also *necessary* for correct use-novel predictions. The reasoning is that without approximate truth of a theory, it is just incomprehensible how it could be capable of producing use-novel

[8]According to Schindler (2008, 266), the term "use-novelty" has been introduced by Deborah Mayo in Mayo (1991, 524). The concept of use-novel predictions, as opposed to temporally novel predictions, was apparently introduced by Zahar and Worrall in the 1970s and 1980s: see Worrall (1989, 148–149).

prediction: it just lacks the resources to do so. So, in Hilary Putnam's famous words, realism "is the only philosophy that does not make the success of science a miracle" (Putnam 1975, 73). The putative weight of this argument is well expressed by its denotation as the "ultimate argument" for realism (van Fraassen 1980, 39; Musgrave 1988). The point of the miracle argument is that it licenses the inference from the existence of correct use-novel predictions of a theory to its approximate truth.

Despite its undeniable plausibility, the question is whether the miracle argument is really correct. There has been an intense critical discussion of the miracle argument, focusing on different aspects of it.[9] Here, I am contributing to this discussion by providing an utterly simple model that may disclose a weakness of the argument.[10] I model the situation of finding an at least approximately true theory, given some empirical evidence, by a curve fitting exercise. Imagine a situation in which there is a true function that we want to identify by a number of measurements. This function itself is determined by 15 data points in five intervals but this is unknown to us ("only God knows" that the function has 15 free parameters). So God's point of view of the situation is depicted in Fig. 1.2.

Now imagine we earthlings have measured the first six data points because we want to determine the true function. Of course, based on these six data points there are many admissible functions and not only the true function, as is illustrated in Fig. 1.3.

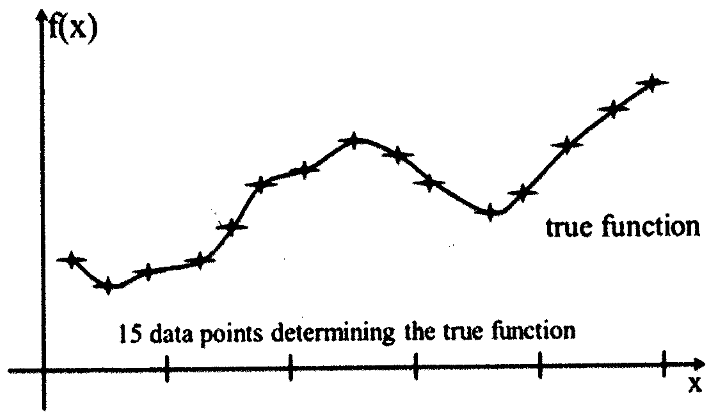

Fig. 1.2 Data points determining the true function

[9]See, e.g., Alai (2014), Hitchcock and Sober (2004), Howson (2013), Lee (2014), Lyons (2003), Lyons (2015), Magnus and Callender (2004), Menke (2014), Psillos (2006), and Saatsi and Vickers (2011).

[10]My first presentation of this model was very clumsy: see Hoyningen-Huene 2011, appendix. It was, to the best of my knowledge, ignored in the literature. The present presentation of the model got rid of much unnecessary mathematical baggage and is hopefully more palatable.

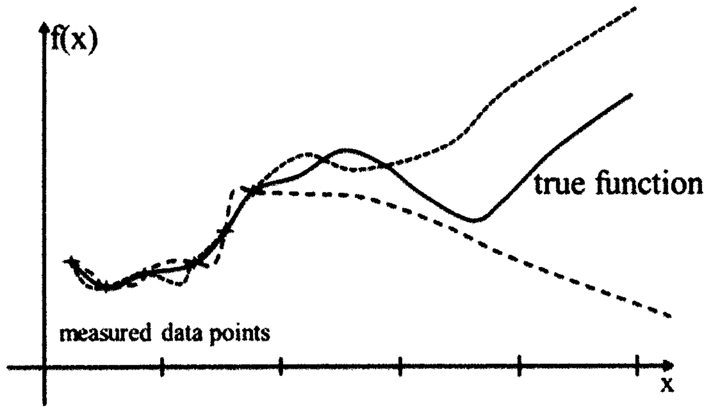

Fig. 1.3 Six measured data points with some fitting functions

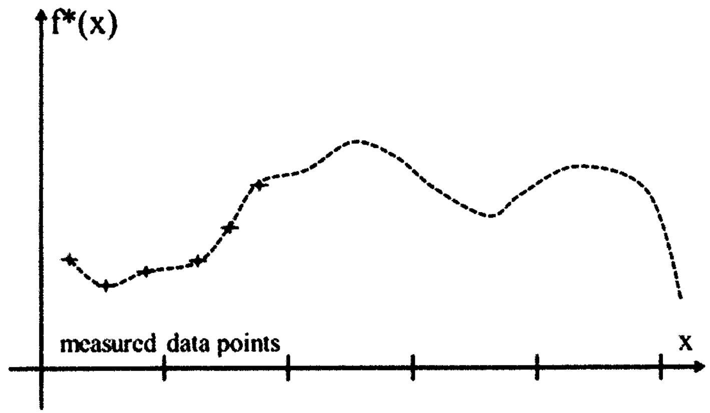

Fig. 1.4 Function f*(x) fitting six measured data points

Imagine now that in the course of our curve fitting exercise, we come up with a function f*(x) as depicted in Fig. 1.4.

As we know that there are many more functions than just the true function that fit the first six data points, we want to test the new candidate f*(x) by making predictions in the third interval of data points. Note that these predictions are strictly use-novel. The result is depicted in Fig. 1.5.

As it turns out, the predicted data points are empirically correct. In other words, we made a successful use-novel prediction based on our function f*(x). Now, overoptimistic realists will possibly exclaim that this proves that f*(x) is the true function! However, we may want to be more cautious because it could be just a lucky (though very improbable) accident that the predicted data points in the third

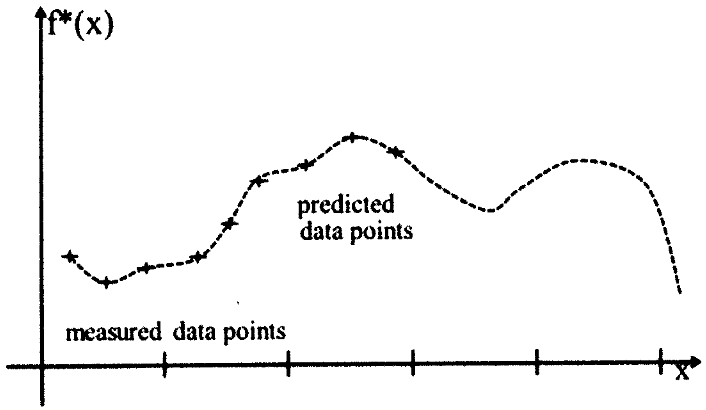

Fig. 1.5 Three data points, in the 3rd interval, correctly predicted by f*(x)

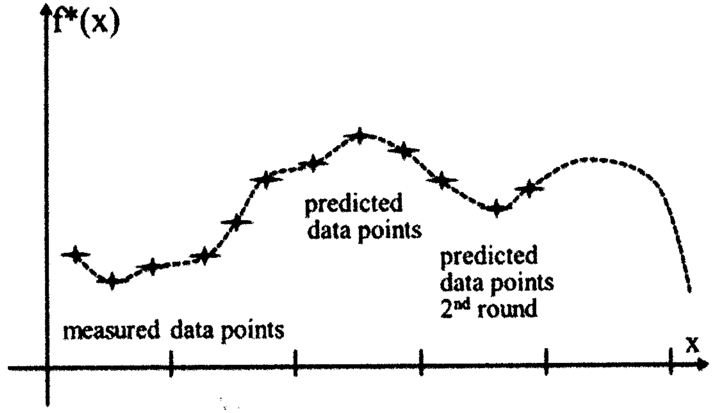

Fig. 1.6 Three more data points, in the 4th interval, correctly predicted by f*(x)

interval came out correctly. Therefore, we will make a second round of use-novel predictions with f*(x) in the fourth interval of data points: see Fig. 1.6.

As it turns out, the predicted data points are again empirically correct. In other words, we made a second round of successful use-novel predictions on the basis of our function f*(x). Now, the realist will have to say: f*(x) is the true function, everything else would be a miracle, or so the miracle argument goes! Unfortunately, in our model we cannot measure the data points in the fifth interval, but God has the full picture: see Fig. 1.7.

Unfortunately, f*(x) differs badly from the true function, in spite of its proven capability of two consecutive rounds of correct use-novel predictions.

How can the miracle argument mislead us so badly by suggesting that f*(x) must be the true function? The reason is that in the model situation, one must be very

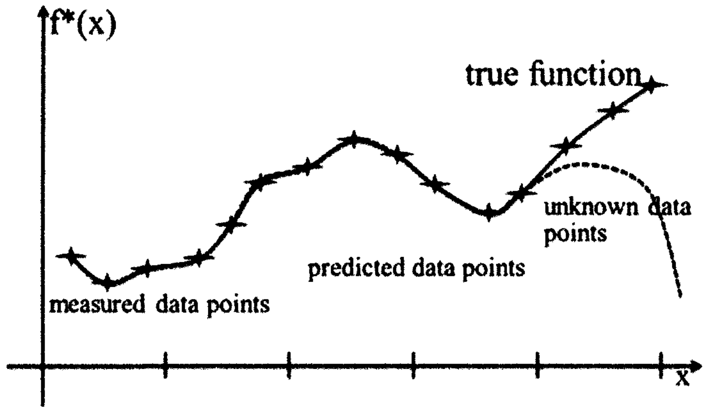

Fig. 1.7 Deviation of f*(x) from the true function

lucky to hit upon a function that produces correct use-novel predictions in the third and fourth interval. By sheer luck, this is exactly what happened when we stumbled upon f*(x). However, one must be even vastly luckier to hit, among the functions that make correct predictions in the third and fourth interval, upon the true function (or any approximately true function). There are many more fundamentally false functions producing correct use-novel predictions in the third and fourth interval than there are approximately true functions with the same predictive capability in these two intervals. In other words, predictive success of a function in the third and fourth data interval is no indicator for predictive success in the fifth interval, i.e., being the true (or an approximately true) function.

Based on this model, the essential weakness of the miracle argument can be presented as follows. The miracle argument is right in stating that it is very unlikely that a theory that has been constructed on the basis of a given set of data will be capable of making correct use-novel predictions. Because of this fact, the miracle argument concludes that there must be a reason for this unlikely possibility to be actual, and the only plausible reason seems to be – in most cases – the theory being (approximately) true. This is purely qualitative reasoning about probabilities. If one turns to comparative reasoning regarding the pertinent probabilities, the result reverts. Although the probability of finding a theory that makes correct use-novel predictions is indeed very low, the probability of finding one among them that is approximately true or even true is even much lower! The reason is that (approximately) true theories are only a small subset of the whole set of theories that are capable of making use-novel predictions. In other words, the miracle argument is fallacious: the capability of producing use-novel predictions is no reliable indicator for (approximate) truth.

However, one may object that the above reasoning is based upon a highly idealized model, and it is very questionable whether this model situation has anything to do with real science.[11] It seems to me that there are striking parallels of the model with real cases, regarding both its seductive power to realist conclusions and their failure. As is well known, classical physics was extremely successful for roughly 150 years, from the early to mid-eighteenth century (general acceptance of Newtonian physics in the 1740s) and the end of the nineteenth century (first doubts about classical physics arising). During this period, classical physics produced numerous use-novel predictions in various areas like the theory of gases, acoustics, gravitational physics (especially regarding the planetary system), electrodynamics, and so on. Up to the end of the nineteenth century, most physicists believed that Newton's theory and its extensions were the last word of physics because they were thought to be literally true.[12] This was indeed the miracle argument in action: how could the repeated success of classical physics in terms of its numerous correct use-novel predictions be explained except by classical physics being final and true? This era corresponds to the third and fourth interval in our highly idealized model. However, as is generally known the belief in the ultimate truth of classical physics has thoroughly dissolved due to the introduction of Special Relativity (1905), General Relativity (1915), and Quantum Mechanics (1925). The data these theories responded to were just not compatible with classical physics. Of course, this situation corresponds to the fifth data interval in our model in which the extrapolation of a hitherto extremely successful theory or function, respectively, proves an utter failure. I conclude that the analogy between the above model and historical reality is close enough to fundamentally doubt the validity of the miracle argument.

However, at this point I have to deal with an objection. One reviewer was not sure whether the paper was, as I had declared, "really dealing with a "level 1"-consideration only [...], but rather (at least elements of) radical level 3-skepticism. And to protect scientific realism from the threat of skepticism is not part of the

[11] For instance, a perceptive reviewer of an earlier version of this paper objected that the question of scientific realism is concerned with the introduction of scientific objects whereas the curve-fitting example is not; therefore, the curve fitting example cannot be of direct relevance to the issue of scientific realism. Yes, it is a fundamental presupposition of any analogy that there is a substantive difference between the things that are stipulated to be analogous. However, in the present case the point of comparison is the presumed truth of the function and the (approximate) truth of the theory in question. In both cases, truth is erroneously inferred from a limited amount of data that were in part even produced by correct use-novel predictions. In the curve fitting case, truth does not imply the existence of certain objects, whereas in the scientific case it does, but that does not invalidate the analogy. – For further illustration of the analogy, see the example of classical physics in the main text.

[12] Remember the well-known story by Max Planck about his Munich teacher Philipp von Jolly who in 1874 advised him not to study physics because Jolly portrayed "physics as a highly developed, almost fully matured science... Possibly in one or another nook there would perhaps be a dust particle or a small bubble to be examined and classified, but the system as a whole stood there fairly secured, and theoretical physics approached visibly that degree of perfection which, for example, geometry has had already for centuries" (Max Planck in 1924, printed in Planck 1933, cited after the translation in *Scientific American*, Feb. 1996, 10).

scientific realist's project." Let me take up the issue of skepticism. I take a skeptical doubt to consider and affirm a logical possibility that radically contradicts deeply engrained convictions (typically about reality) and that cannot be compellingly rejected. Take, for example, Descartes' "evil demon" who is supposedly systematically deceiving us about reality: it is a logical possibility that cannot be compellingly rejected. The weakness of such skeptical arguments is that we do not have the slightest positive hint that the logical possibility in question is indeed realized, and that there are infinitely many such logical possibilities. Surely, my objection to the miracle argument based on the curve-fitting example appears, at first sight, to articulate a logical possibility only. However, what distinguishes this objection from a purely skeptical one are the historical facts that I mentioned above. The possibility that even repeated use-novel predictions can falsely lead us to realistic conclusions, is not just a logical possibility, but was a (bitter) reality with respect to classical physics, as it turned out in the twentieth century. Unlike any skeptical doubt that is purely hypothetical, I presented empirical evidence that the miracle argument can be misleading. Thus, my objection does not, like a skeptical argument, articulate a purely logical possibility only (that can be eventually dismissed) but shows, by empirical example, that this possibility can indeed be real and has therefore to be taken seriously.

Do all these considerations answer the title question, "Are there good arguments against scientific realism?" Of course, they do not. They only show that what has perhaps imprudently been called "the *ultimate* argument for scientific realism" fails badly because it is fallacious. However, there may be other arguments for scientific realism besides the "ultimate" one, and there may be other forms of realism, relatives of scientific realism, that are supported by different arguments. This is indeed the case. I shall now investigate something that is not really a single argument in support of a particular kind of realism, but rather an argumentative strategy that singles out particular kinds of realism together with arguments supporting them. So we are dealing with a family of different realist positions that are united by a common type of supportive argument. The strategy is called "selective realism".

1.4 Selective Realism

Here is a rough sketch of the strategy of selective realism.[13] The starting point is a presupposition that is nowadays widely shared by philosophers of science of different persuasions (and by some fraction of scientists). This presupposition claims that most, if not all, of our accepted scientific theories, even the very best ones, are strictly speaking false. This implies that there are (at least) some parts of these theories that do not correspond to reality. Therefore, a realist interpretation

[13] For a very clear summary, see Chakravartty (2014), Sections 1.3 and 2.3. An alternative denomination of selective realism is "deployment realism"; see, e.g., Lyons (2016).

of these theories as wholes is inappropriate. However, some of these theories are remarkably successful, e.g., with regard to use-novel predictions. Now the first step in the strategy of selective realism is this: identify and select those elements of the theories in questions, which are responsible for the pertinent success. It is those elements that are potentially worthy of a realist commitment. Thus, the first step of the argument identifies, in the spirit of the miracle argument, the *candidates* for a realist commitment.

Why does this step only identify *candidates* for a realist commitment, why is an additional second step necessary? The reason is that it is known from the history of science that also theories relying on theoretical entities that were later shown not to exist, may have been persuasively successful, for instance with respect to use-novel predictions. This historical fact has been used as an argument against cruder forms of scientific realism: theoretical entities responsible for the empirical success of a theory may still be abandoned during the next scientific revolution and be replaced by something substantially different. Thus, for a robust realist commitment to some theoretical element it is not sufficient that this element is responsible for persuasive empirical success (whatever kind the favored persuasive empirical success is). In addition, this theoretical element must also survive serious theory change, i.e., revolutionary developments. This second step of the strategy of selective realism is also called "the continuity argument".

This is then the general strategy of selective realism: identify those elements of empirically successful scientific theories, which are (a) responsible for the particular empirical success and which (b) have survived revolutionary developments. Each of these conditions is necessary, and together they are seen as sufficient for the realist commitment to these elements of scientific theories. Sophisticated realists, however, do not claim that fulfilment of these two conditions *guarantees* the correctness of the realist commitment, because they are fallibilists. They only claim that under the two conditions it is reasonable to interpret these elements realistically.

Different versions of selective realism can now be distinguished according to what they take to be the most promising candidates for their realist commitments. *Scientific realism* claims that it is theoretical entities that are responsible for a theory's given empirical success. However, one has to show that in addition, these entities survive revolutionary breaks. Typically, this has been done by deflecting and containing the (undisputed) meaning change of key concepts involved in revolutionary breaks, i.e. incommensurability, to the intensions of the concepts in question, and thereby keeping their referents, i.e. their extensions, stable.[14] *Explanationism* leaves first open what the candidates for a realist commitment might be. It tries to identify them functionally by their role in producing the success of the respective theory, i.e., novel predictions. These functionally important parts of theories are called their "working posits", to which the realist should commit, in contrast to the "idle parts" of the theories, which do not contribute to the

[14]This presupposes a theory of meaning and of reference that supports this move. See among many others, e.g., Sankey (1994), especially Chapters 2 and 5.

pertinent empirical success. The working posits then explain the empirical success of these theories, which gives the position its name. As successor theories should be capable of reproducing the empirical success of their predecessors, the working posits of the predecessors should be present in them, too. Finally, *structural realism* singles out as the locus of realist commitments structural aspects of the theories in question, typically mathematical aspects of the theories.[15] The basic idea is that it is correspondence of the formalism of our best theories with physical structures that is responsible for the empirical successes of such theories. These structures may be preserved through scientific revolutions although the ontologies and the explanatory mechanisms of the theories divided by the revolutionary break may differ greatly. Again, not everything in these structures may be involved in the empirical success of a theory, so only those parts are singled out that represent "operative" relations.[16]

Clearly, in the first step of this strategy the miracle argument is involved, at least as a heuristic device, otherwise this step would not be plausible. However, for the sake of argument I will at this point not doubt the legitimacy of this first step, because only on the basis of its acceptance can the strength of the second step be assessed. Let us now turn to this second step of the strategy of selective realism, the continuity argument.

The precise formulation of the continuity argument merits careful discussion. Ioannis Votsis has devoted a full paper to this issue. His result is the following: "Preservation [through theory change] is a reliable guide to (approximate) truth"[17]. Votsis stated this in the context of his discussion of structural realism, but his statement can be generalized to the other positions we are also discussing here. This is because not only structures with a claim to reality must survive theory change, but also entities or working posits (or whatever) with a claim to reality. We can thus formulate the second step of the strategy of selective realism as follows.

Let a sequence of empirically progressive theories be given with "entity continuity", or "working posits continuity", or "structural continuity". At any given time, the respective theoretical entities, or working posits, or structures, are identified as responsible for the specific empirical success of those theories. Typically, this empirical success is spelled out as the capability of correct use-novel predictions, but it may also be another form of particularly persuasive empirical success. For the following, however, neither what is taken to be responsible for the empirical success (entities, structures ...), nor what is taken the persuasive empirical success to consist in (use-novel predictions ...), will play a role, so we can abstract from them. The (abstract) situation we are dealing with can thus be described as follows. Let a sequence theories be given with X continuity, and X is seen as at least partly responsible for their increasing, particularly persuasive empirical success. Then, so

[15]As is well-known, structural realism has gained much renewed attention in the last decades, mainly through John Worrall's work: Worrall (1996 [1989]). For a comprehensive reference to the current status of structural realism, see, e.g., French 2014.

[16]See Votsis (2011, 107–108).

[17]Votsis (2011, 116).

the continuity argument goes, X can very reliably be interpreted realistically. In this way, the continuity of X, i.e. its stability through theory change, is taken to be a (fallible) indicator of its reality.

Against the continuity argument, the following objection can be brought forward; I call it the "dead end objection".[18] For the sake of argument, let us make the (temporary) assumption that the given X continuity in the series of empirically increasingly successful theories is due to their convergence to a certain limit theory that will also display X, like the elements of the series. Thus, the elements in the series of empirically increasingly successful theories are successive approximations to the limit theory.

Under these suppositions, what do we know about this limit theory? First, the limit theory is empirically more powerful than any of the elements in the series. By "empirically more powerful", I mean that its predictions are more accurate than those of any of the elements of the series, and it may make additional correct predictions in comparison to the elements in the series. Imagine, for instance, that the predictions of the limit theory attain a relative accuracy of at least 10^{-30} for all observed quantities. However, from the given suppositions about the limit theory we cannot infer that the limit theory is also capable of producing correct use-novel predictions (or whatever the measure of the pertinent empirical success is). For instance, the potential to make use-novel predictions by means of the presence of X may have already been exhausted by the elements in the series, such that the limit theory cannot contribute additional use-novel predictions based on X.

Second, by construction, the empirical power of the limit theory is at least partly due to the presence of X. The decisive question is, are we allowed to interpret X in the limit theory realistically? The continuity argument would clearly give a positive answer to this question: the continual presence of X in all the elements of the series of theories and X's capability of producing persuasive empirical success licenses a realist interpretation of X in the elements of the series. If X can be interpreted realistically in the elements of the series, clearly the same X contained in the limit theory can also be realistically interpreted. This would be the answer of the continuity argument.

However, we should now put the continuity argument on hold because we want to investigate its validity. In order to do so, we have to investigate the properties of the limit theory more carefully. We will do that in two steps. First, we investigate the limit theory in isolation of the elements of the series of theories whose limit it is. Second, we put the limit theory back in its proper context, namely, as being the limit of the series of empirically increasingly successful theories.

[18]Following a paper that I gave in 2012 at the GAP conference in Constance, Ron Giere suggested to me the name "dead end objection", instead of "impasse objection" that I had used in Hoyningen-Huene (2013). I am grateful for this suggestion. Furthermore, the presentation of the objection in the present paper is much more careful than in the older paper. Specifically, I am now avoiding the objection that my argument is just skeptical and thus void of any argumentative force for the particular case in question.

When we consider the limit theory separately, i.e., independently of the series of theories whose limit it is, we can ask whether we have reasons to interpret its component X realistically. The situation is this: We have a theory that is empirically extremely accurate, and we can ascribe its predictive accuracy to some component X of it. However, we do not know whether the limit theory has those specific empirical virtues that we have identified earlier as the necessary criteria for a realist interpretation of X, like the capacity to produce correct use-novel predictions (or whatever). In other words, we cannot run the miracle argument on the limit theory because it does not fulfill its necessary empirical suppositions (capability to produce use-novel predictions, or whatever). Thus, when applied to the limit theory in isolation, the first step of the strategy of selective realism fails for *empirical* reasons: we just do not know whether the limit theory fulfills the necessary empirical suppositions for the application of the miracle argument. However, also the second step of the strategy of selective realism, the continuity argument, fails for the limit theory, this time for *conceptual* reason. As we are considering the limit theory in isolation, there simply are no other theories with which any continuity with the limit theory could exist, thus the continuity argument is inapplicable. In other words, the two arguments that are otherwise seen as supporting realist interpretations of (parts of) theories are not applicable to the limit theory. So, despite its undoubted empirical virtues (e.g., a relative empirical accuracy of at least 10^{-30}), the limit theory's epistemic status with respect to a realist interpretation is open at this point. Of course, there is the *possibility* of X deserving a realistic interpretation, thus explaining the fantastic predictive success of the limit theory. However, there is also the possibility that the limit theory is a model, whose component X that is responsible for immensely accurate predictions is nevertheless entirely unrealistic. So, considered independently of the series of theories whose limit it is, we have no reason to interpret the X contained in the limit theory realistically: the arguments usually adduced to support a realist interpretation of an X contained in a theory do not apply to the limit theory. Considered in separation of the series of theories whose limit it is, the epistemic status of X in the limit theory with regard to realism is open: we just do not know and have no arguments to assess it; it may be (approximately) true, or it may be radically false.[19]

How does the picture change when the limit theory is now put back in its proper context, namely, being the limit of a series of empirically increasingly successful theories? Any single theory in the series is capable of making use-novel predictions (or whatever the standard of persuasive empirical success is) due to the presence of X, and X can also be found in its successor theory. This is a fact that, according to

[19]A reviewer of an earlier version of this paper opined that "it is assumed that the limit theory is not the true theory or indeed an approximately true theory. But that's begging the question against the realist." Had I had made this assumption, the reviewer would of course be right. However, I am *not* assuming that the limit theory is not true. I leave it entirely open what the epistemic status of the limit theory is and ask what arguments we have to determine its epistemic status. Then I claim that *we have no arguments* to the effect that the X in the limit theory can be interpreted realistically, so its epistemic status *remains* open.

the continuity argument, is evidence for the legitimacy of a realist interpretation of X. Therefore, focusing on the theories in the series, we appear to have as many independent pieces of evidence for the realist interpretation of X as we have theory pairs. However, taking into account that there is a limit theory, the theories in the series are empirically successful because they participate (at least approximately) in the component X of the limit theory. Therefore, the X-continuity in the series of theories becomes a consequence of their convergence to the limit theory. Furthermore, also the fact that the theories in the series can produce correct novel predictions (or some other persuasive empirical success), is just a consequence of their (at least approximate) participation in the component X of the limit theory. It is the component X of the limit theory that the theories in the series increasingly exploit, and that increasing exploitation makes use-novel predictions (or some other persuasive empirical success) possible. In other words, interpreting the theories' capacity to produce use-novel predictions as indicators of the reality of X is a mistake; it is a consequence of the existence of a limit theory that contains X. Moreover, as there is no good reason to interpret the limit theory realistically with respect to X, there is no reason to interpret the presence of X realistically in any of the theories in the series either. Thus, instead of having as many pieces of evidence for the (approximate) reality of X as there are theory pairs in the series, we have no evidence for the (approximate) reality of X at all. Because the continuity argument suggests the opposite, it is fallacious.

In the above argument, I have used the assumption that the series of theories converges to a limit theory. This is, of course, a problematic assumption because to explicate what the convergence of theories exactly means is a highly nontrivial affair. However, it is easy to relax this condition. The series of theories does not have to converge to an empirically much more accurate theory. It is enough that there is such a theory in the background such that all the theories in the series derive their empirical potential from the kinship with regard to X to this theory. Again, the fact that this theory in the background fits many empirical facts to a very high accuracy is not by itself a sign for its verisimilitude with respect to aspect X. As the theories in the series do not in any sense go beyond the background theory, their continuously containing X cannot be interpreted realistically, as the background theory's containing X cannot be either.

1.5 Conclusion

If the given analysis is correct, I think that at the bottom level, both the continuity argument and the miracle argument share the same fundamental weakness. In the case of the miracle argument, from a theory's (possibly even repeated) use-novel predictions realist conclusions are drawn. In the case of the continuity argument, from continuity of some X through (possibly repeated) theory change realist conclusions are drawn. However, the realist conclusions are overhasty because the observed properties of the theory or theories, respectively, could equally well be due

to an underlying fundamentally false theory, i.e., a theory that cannot be realistically interpreted in the pertinent respect. The highly idealized model that I presented suggests the following hypothesis. Certain astonishing properties of theories that prompt their realist interpretation are in fact due to a theory in the background that may be fundamentally false in the critical respects. Though in general, it is indeed very unlikely that a series of given theories has a false theory in the background that is responsible for their realism-suggesting properties, it is *even more unlikely* that these properties are due to a (approximately) true theory in the background.

Let me sum up. My title question was, are there good arguments *against* scientific realism? The answer is, perhaps there are. What I have argued in this paper is that the miracle argument and the continuity argument, which are supposed to be the best arguments *for* scientific and other forms of selective realism, are fallacious.

References

Abbott, B.P., et al. 2016. Observation of gravitational waves from a binary black hole merger. *Physical Review Letters* 116 (6): 061102.

Alai, M. 2014. Novel predictions and the no miracle argument. *Erkenntnis* 79 (2): 297–326.

Chakravartty, Anjan. 2014. Scientific realism. In *The Stanford encyclopedia of philosophy*, ed. E.N. Zalta. Spring 2014 ed. http://plato.stanford.edu/archives/spr2014/entries/scientific-realism/

Dawkins, R. 1995. *River out of Eden: A Darwinian view of life*. New York: Basic Books.

Einstein, A. 1916, Juni 29. Näherungsweise Integration der Feldgleichungen der Gravitation. *Königlich Preußische Akademie der Wissenschaften (Berlin). Sitzungsberichte*, 688–696.

———. 1918, February 21. Über Gravitationswellen. *Königlich Preußische Akademie der Wissenschaften (Berlin). Sitzungsberichte*, 154–167.

French, S. 2014. *The structure of the world: Metaphysics and representation*. Oxford: Oxford University Press.

Giere, R.N. 2006. *Scientific perspectivism*. Chicago: University of Chicago Press.

Hawking, S.W. 1971. Gravitational radiation from colliding black holes. *Physical Review Letters* 26 (21): 1344–1346.

Hawking, S.W. 1996. Classical theory. In *The nature of space and time*, ed. S.W. Hawking and R. Penrose. Princeton: Princeton University Press.

Hawking, S.W., and L. Mlodinow. 2010. *The grand design*. London: Bantam.

Hawking, S.W., and R. Penrose. 1996. *The nature of space and time*. Princeton: Princeton University Press.

Heitler, W., and F. London. 1927. Wechselwirkung neutraler Atome und homöopolare Bindung nach der Quantenmechanik. *Zeitschrift für Physik* 44: 455–472.

Hitchcock, C.H., and E. Sober. 2004. Prediction versus accommodation and the risk of overfitting. *The British Journal for the Philosophy of Science* 55 (1): 1–34.

Howson, C. 2013. Exhuming the no-miracles argument. *Analysis* 73 (2): 205–211.

Hoyningen-Huene, P. 2011. Reconsidering the miracle argument on the supposition of transient underdetermination. *Synthese* 180 (2): 173–187.

———. 2013. The ultimate argument against convergent realism and structural realism: The impasse objection. In *EPSA11 perspectives and foundational problems in philosophy of science*, ed. V. Karakostas and D. Dieks, 131–139. Cham: Springer.

Husserl, E. 1967 [1922]. *Ideas: General introduction to pure phenomenology*. Trans. W.R.G. Gibson. New York: Humanities Press.

Lee, W.-Y. 2014. Should the no-mircacle argument add to scientific evidence. *Philosophia* 42 (4): 999–1004.
Lyons, T.D. 2003. Explaining the success of a scientific theory. *Philosophy of Science* 70: 891–901.
———. 2015. Scientific Realism. In *The Oxford handbook of philosophy of science*, ed. P. Humphreys. New York: Oxford University Press.
———. 2016. Structural realism versus deployment realism: A comparative evaluation. *Studies in History and Philosophy of Science Part A* 59: 95–105.
Magnus, P.D., and C. Callender. 2004. Realist ennui and the base rate fallacy. *Philosophy of Science* 71 (3): 320–338.
Mayo, D.G. 1991. Novel evidence and severe tests. *Philosoophy of Science* 58 (4): 523–552.
Menke, C. 2014. Does the miracle argument embody a base rate fallacy? *Studies in History and Philosophy of Science* 45 (3): 103–108.
Musgrave, A. 1988. The ultimate argument for scientific realism. In *Relativism and realism in science*, ed. R. Nola. Dordrecht: Kluwer Academic.
Planck, M. 1933. *Wege zur physikalischen Erkenntnis: Reden und Vorträge*. Leipzig: S. Hirzel.
Psillos, S. 2006. Thinking about the ultimate argument for realism. In *Rationality and reality: Conversations with Alan Musgrave*, ed. C. Cheyne and J. Worrall, 133–156. Berlin: Springer.
Putnam, H. 1975. What is mathematical truth? In *Mathematics, matter and method. Philosophical papers*, vol. 1, 60–78. Cambridge: Cambridge University Press.
Rowbottom, D.P. 2011. What's at the bottom of scientific realism? *Studies in History and Philosophy of Science Part A* 42 (4): 625–628.
Saatsi, J.T., and P. Vickers. 2011. Miraculous success? Inconsistency and untruth in Kirchhoff's diffraction theorie. *British Journal for the Philosophy of Science* 62 (1): 29–46.
Sankey, H. 1994. *The incommensurability thesis*. Aldershot: Avebury.
Schindler, S. 2008. Use-novel predictions and Mendeleev's periodic table: Response to Scerri and Worrall (2001). *Studies in History and Philosophy of Science Part A* 39 (2): 265–269.
van Fraassen, B.C. 1980. *The scientific image*. Oxford: Clarendon.
Votsis, I. 2011. Structural realism: Continuity and its limits. In *Scientific structuralism*, ed. P. Bokulich and A. Bokulich, 105–117. Dordrecht: Springer. Available at: http://philsci-archive.pitt.edu/5233/1/VotsisStructuralRealismContinuityanditsLimits.pdf
Worrall, J. 1989. Fresnel, poisson, and the white spot: The role of successful predictions in the acceptance of scientific theories. In *The use of experiment. Studies in the natural sciences*, ed. D. Gooding, T. Pinch, and S. Schaffer, 135–157. Cambridge: Cambridge University Press.
———. 1996 [1989]. Structural realism: The best of both worlds?. In *The philosophy of science*, ed. D. Papineau, 139–165. Oxford: Oxford university press (originally in *Dialectica* 43, 99–124 (1989)).

Chapter 2
Quantum Gravity: A Dogma of Unification?

Kian Salimkhani

Abstract The quest for a theory of quantum gravity is usually understood to be driven by philosophical assumptions external to physics proper. It is suspected that specifically approaches in the context of particle physics are rather based on metaphysical premises than experimental data or physical arguments. I disagree. In this paper, I argue that the quest for a theory of quantum gravity sets an important example of physics' *internal* unificatory practice. It is exactly Weinberg's and others' particle physics stance that reveals the issue of quantum gravity as a genuine physical problem arising within the framework of quantum field theory.

Keywords Principle of equivalence · Unification · Quantum field theory · Quantum gravity · General relativity · Graviton/spin-2 particle · Lorentz-invariance

2.1 Introduction

To 'combine general relativity and quantum mechanics'—as the issue of quantum gravity (QG) is frequently summarized—is typically understood to be the central challenge for fundamental physics. The common conviction is that this quest for QG is not only fuelled, but generated by external principles (cf. Mattingly 2005; Wüthrich 2006, 2012). Accordingly, the research program of QG is believed to be driven, first and foremost, by reasoning involving philosophical assumptions.

I thank Andreas Bartels, Cord Friebe, Stefan Heidl, Niels Linnemann, James Read, Matthias Rolffs, Thorsten Schimannek, and Christian Wüthrich for helpful discussions and remarks. Furthermore, I thank the anonymous referee for pressing me to clarify some paragraphs, especially in the opening sections.

K. Salimkhani (✉)
Institute for Philosophy, University of Bonn, Am Hof 1, 53113 Bonn, Germany
e-mail: ksalimkhani@uni-bonn.de