**A Less Than Direct Connection Indeed: Reply to Jakowljewitsch**

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The paper by Dragan Jakowljewitsch is a significant contribution to the discussion of scientific realism which has followed the publication of my ‘What is Scientific Realism?’ in an earlier issue of this journal (Sankey, 2000). The paper makes a number of important points and advances the discussion considerably. In this comment, I will respond to a number of critical points raised by Jakowljewitsch, and correct some minor misconceptions.

**1. Scientific realism vs scientific-theoretical realism**

Jakowljewitsch chooses to employ the expression ‘scientific-theoretical realism’ rather than the standard label ‘scientific realism’ (see footnote 1). He does so because of the risk that talk of scientific realism may convey a value judgement which suggests that scientific realism is a form of realism that is superior to other, non-scientific, unscientific or less scientific, forms of realism.

 As a point about the ambiguity of the English expression, ‘scientific realism’, the point is well taken. It is possible to read the expression ‘scientific realism’ as referring to a scientific form of realism, which is thought to be better than other, non-scientific forms of the doctrine. But, of course, this is not the intended significance of the label ‘scientific realism’. As the expression is customarily employed in the philosophy of science, it is the name of a realist philosophy of science of the kind that I sought to describe in my earlier paper, according to which science aims to discover the truth about both observable and unobservable features of the objective world. There is no suggestion in this doctrine that other forms of realism need be any less scientific than scientific realism. The relevant contrast is between scientific realism and anti-realism in the philosophy of science, rather than between scientific and non-scientific forms of realism.

 In ‘What is Scientific Realism?’, I contrasted scientific realism with the doctrine of scientism, which is the doctrine that science is the sole legitimate source of empirical knowledge. As I there argued, none of the essential principles which characterize the position of scientific realism imply that science is the only means of genuine knowledge-acquisition. There is no implication that knowledge is the unique possession of empirical science. Jakowljewitsch’s concern that use of the term ‘scientific’ might be taken to imply the inferiority of other forms of realism is a nice illustration of the point that a connection exists in some people’s minds between scientific realism and scientistic modes of thought.

 It should be said, though, that the expression ‘scientific-theoretical realism’ does mark a useful contrast with a closely related form of realism. This is the position of scientific entity realism associated with such philosophers as Brian Ellis, Nancy Cartwright and Ian Hacking. Such entity realists downplay the role of truth and representation in relation to theories. They emphasize instead the reality of the unobservable entities dealt with in science, especially in experimental practice. In a context in which there is a danger of conflating the standard truth-orientated form of scientific realism with the entity realist alternative, Jakowljewitsch’s preferred expression is entirely apposite.

**2. Correspondence and reference**

Jakowljewitsch remarks that “By definition, incorrect assumptions do not correspond to reality, they do not have a reference to reality” (p. 8). This is only half right. False statements do not correspond to reality. But their terms may still refer. Jakowljewitsch’s remark overlooks an important feature of the relationship between correspondence and reference.

 As indicated in ‘What is Scientific Realism?’, I take the idea that truth consists in correspondence between statement and reality to be one of the core elements of scientific realism. The notion of correspondence may be understand in a number of different ways that are consistent with realism. But while it is not necessary for scientific realism to adopt any particular conception of correspondence, my own preference is to understand correspondence in terms of reference. Truth depends on reference. A statement about the world is true if and only if its constituent terms refer to items in the world which possess the properties, or enter into the relations, described in the statement. If any of the terms fail to refer, or if the items referred to fail to possess the properties, or enter the relations, described by the statement, then the statement fails to correspond to reality, and is therefore false.

 Because truth is related to reference in the manner just described, it is possible for a statement to “have a reference to reality” even though it “does not correspond to reality”. For while the constituent terms of a false statement may fail to refer to existing items, it is also possible for a false statement to refer to existing items but to falsely attribute properties or relationships to those things. In the latter case, the statement’s constituent terms refer to existing items even though the statement is in fact false. Hence, it is possible for a statement to be false in the sense of failing to correspond to the way things are in the world, and yet it may “have a reference to reality” in the sense that its constituent terms are genuinely referring expressions.

**3. Commonsense realism and scientific realism**

Jakowljewitsch claims that commonsense realism has been “largely discredited ... in current discussions among philosophers of science” (p. 3). This is a surprising claim. It is certainly true that the uncritical acceptance of the dictates of science has been widely rejected as naive. But it is not clear that the position of commonsense realism has been discredited by any development in the philosophy of science. Nor indeed is it clear that commonsense realism has been widely rejected by philosophers of science. It is perhaps the case that, from the broadly neo-Kantian perspective of continental philosophy of science, commonsense realism tends to be rejected as naive. But such rejection is less widespread outside of continental philosophy of science.

 Commonsense realism is the view that there is a mind-independent, objective reality, with which we contend in our ordinary everyday practical activities, and of which we obtain knowledge by way of sense perception. Such an objective reality contains ordinary middle-sized objects such as tables and chairs, rocks and trees, people and animals. It is a world in which cause leads to effect, the past is a guide to the future, and in which mind interacts with reality by way of human action and behaviour. With its acceptance of the ordinary world of everyday things, commonsense realism is the kind of pre-philosophical view of the world that is brought into question by traditional epistemological scepticism of a Cartesian, and to a certain extent, Humean variety. But for a great many contemporary philosophers, especially those of a naturalistic (not to say, Moorean) cast of mind, commonsense realism emerges from its engagement with the sceptics largely unscathed.

 There is an interesting relation between commonsense realism and scientific realism. Because, as has often been said, science may lead to the correction and even overthrow of the dictates of common sense, some claim that there is a tension between science and common sense. This gives rise to two alternative strands within the scientific realist tradition. On the one hand, a number of realist philosophers have argued that commonsense realism is to be rejected in favour of scientific realism. On the other hand, some realist philosophers hold that commonsense realism and scientific realism may be combined to form a single, unified realist position. My own preference is to support this latter version of realism, according to which commonsense realism and scientific realism form part of the same realist position. This commits me to the view that, while science may correct common sense, it does not overturn it. Science may explain common sense. It does not eliminate it.

 It is possible that the preceding remarks about commonsense realism may run somewhat at cross purposes with Jakowljewitsch, despite his use of the expression ‘common-sense realism’. This is because Jakowljewitsch may not be employing the expression ‘common-sense realism’ in the sense in which it is ordinarily used in discussion of realism. He may mean something closer to what is sometimes called ‘naive realism’. After all, he does employ the expression “naively realist” at one point.

 This interpretation of what Jakowljewitsch means by ‘common-sense realism’ is suggested by the following. He claims that commonsense realism has been “discredited” because of a number of “considerations” which derive from contemporary philosophy of science. Among these “considerations”, he includes the underdetermination of theory by data, the theory-dependence of observation and the role of subjective value judgements in scientific theory choice. But, so far as I can see, such “considerations” have no implication whatsoever for commonsense realism, understood as realism about the objects of ordinary everyday experience. What they do challenge is something quite different. If correct, the “considerations” would lead to the rejection of a naive and uncritical acceptance of the claims of theoretical science as conclusively proven truths, which are directly established by observation.[[1]](#footnote-1) Such “naive realism” is, of course, to be rejected. But it is not to be identified with either commonsense or scientific realism, however the two latter may be related.

**4. Fallibilism and ‘proof for realism’**

At a number of points in his paper, Jakowljewitsch takes note of the inconclusive character of the argument for scientific realism, which I had also noted in my paper. He also suggests that realists mistakenly seek a “proof for realism”. The issue of whether or not there may be a conclusive proof of realism is an important one. It is worthwhile, therefore, to offer some explanation of why scientific realism must tread carefully around the notion of conclusive proof in science.

 Most, if not all, contemporary philosophers of science endorse a position of epistemic fallibilism. Scientific realists are no exception. According to fallibilism, certainty is not required for knowledge. Nor is certainty to be expected in scientific inquiry. There are a variety of reasons to adopt a fallibilist position. On the one hand, arguments from the sceptical tradition may be understood to show, not that knowledge is impossible, but that the quest for certainty is a forlorn quest. On the other hand, the history of science reveals science itself to be a process of ongoing change and revision, in which previously established theories are corrected or overthrown in light of later evidence or developments elsewhere in science.

 While it would be possible to formulate an infallibilist version of scientific realism, according to which science yields certain and incorrigible knowledge, few contemporary philosophers of science would embrace such a doctrine. Instead, scientific realism is usually understood in a fallibilist manner, according to which scientific knowledge is fallible knowledge, and scientific inquiry is a process of correction and change in which theories are subject to revision and established theories may be overthrown in light of recent advances.

 It is because scientific realism is to be understood in terms of fallibilism in the philosophy of science that realists must refrain from claims to be able to provide a conclusive “proof” of realism. Here it may be helpful to distinguish between two levels at which one might seek proof: one might seek proof in science or one might seek proof in the philosophy of science. Given the evolving and revisionary nature of science, realists should adopt a fallibilist attitude toward science itself. They should resist the demand for conclusive proof within the sciences.

 But the question remains of whether it may be possible to provide a conclusive proof of scientific realism as a philosophy of science. Here my own attitude does not differ much from that of Jakowljewitsch, who argues that scientific realism should be understood as one of a number of competing philosophical explanations of science, which should be subjected to comparative evaluation and accepted only if it is shown to a better explanation than the alternatives (p. 9). This issue brings me to the question of the success of science, and Jakowljewitsch’s discussion of the success argument for realism.[[2]](#footnote-2)

**5. The success argument**

I concluded ‘What is Scientific Realism?’ with a brief sketch of the main positive argument for scientific realism, the so-called success argument. According to this argument, realism should be accepted because it provides the only non-miraculous explanation of the success of science. Positions which deny the truth and reference of scientific theories render the success of science an inexplicable miracle. Scientific realism provides a natural explanation of the success of science in terms of the truth and reference of theories. It should therefore be accepted, since it is the best explanation we have of the success of science.[[3]](#footnote-3)

 Jakowljewitsch takes me to task on a number of points in relation to the success argument. He says that my construal of the success argument involves “an assumption that is actually much disputed and in need of justification, *viz*., that the truth of scientific theories and the reality of scientific entities is a fact” (pp. 3-4). But this is not an assumption that I make in the context of the success argument. Instead, the truth (or approximate truth) of theories and the reality of scientific entities is precisely what it is the purpose of the success argument to show. So far from being an assumption on which the argument is based, it is what the realist is attempting to establish by means of the success argument.

 The realist’s point is that the success of science is readily explained on the basis of the truth of theories and the reference of theoretical terms to real entities. The purpose of the success argument is to show that the realist explanation should be accepted because it constitutes a superior explanation to other explanations that might be proposed which reject realism. Thus, I understand the success argument to have a form closer to that of Jakowljewitsch’s proposed restatement of the argument (p. 4), in which the claim that scientific realism is the best explanation of the success of science occurs as premise (2).

 But, of course, the claim that scientific realism is the best explanation of the success of science is itself open to question, as Jakowljewitsch goes on to show. In the first place, even if scientific realism were the best available explanation of success, this would not show that scientific realism is true. As Jakowljewitsch notes, “the insufficiency of alternative explanations is not a positive reason for the correctness of the only remaining alternative” (p. 5). This objection, which is sometimes called “the problem of the bad lot”, since realism might just be the best of a bad lot, raises an important issue that must be addressed by any plausible version of scientific realism. The usual response is to insist that the range of alternative explanations under consideration cannot simply be an unrestricted set of available explanations (e.g., Musgrave 1988, pp. 238-9). Rather, the explanations under consideration must satisfy certain conditions of explanatory adequacy. Realism is only to be accepted as the best explanation of the success of science if, besides being the best available explanation, it also satisfies conditions of explanatory adequacy. Otherwise, the argument for realism would immediately succumb to the objection that it fallaciously infers from being the best available explanation to being the best explanation. It is only if the realist explanation is in fact the best explanation, as measured by criteria of explanatory adequacy, that the realist’s success argument has any hope of success.

 As a possible alternative explanation to realism, Jakowljewitsch mentions the notion of empirical adequacy that is the central notion of Bas van Fraassen’s constructive empiricist philosophy of science (see van Fraassen, 1980).[[4]](#footnote-4) According to constructive empiricism, the aim of science is to obtain theories which are empirically adequate, by which is meant that all of a theory’s observational consequences, past, present and future, are true. Jakowljewitsch suggests that the notion of empirical adequacy can be employed to explain the success of science, so that an anti-realist position such as constructive empiricism might provide a suitable alternative explanation to realism (p. 5). But it is quite unclear that this is the case. For how does the empirical adequacy of a theory explain its success? To be empirically adequate just is for all of a theory’s observational consequences to be true. But the success of a scientific theory consists in the truth of the observational consequences of the theory that have so far been empirically confirmed. The success of an empirically adequate theory therefore follows trivially from its being empirically adequate, since the set of its observational consequences that have so far been observed to obtain is just a subset of its true observational consequences. As Musgrave remarks, “this is like explaining why some crows are black by saying that they all are” (1988, p. 242). Such an “explanation” is either a poor explanation or it is no explanation at all. Call it what you will. Either way, the constructive empiricist alternative described by Jakowljewitsch does not provide a viable alternative explanation of success to the realist one.

 Jakowljewitsch also expresses concern about the notion of success that is employed in the realist’s success argument. Following a suggestion of Paul Feyerabend, he claims that “success and failure are ‘culturally determined terms’ so that something may be viewed as a success from the point of view of ‘one cultural practice or another’” (p. 6). But while this may be true it is irrelevant to the issue under discussion. It may well be the case that what counts as a success or a failure, or what value is placed on success or failure, varies from one cultural context to another. But this is simply not the point at issue in the debate about scientific realism. The realist points to a very specific phenomenon, namely, the success of science, and provides an explanation of that phenomenon. The success of science consists of its predictive accuracy and reliability, explanatory breadth, and ability to support technological control over the environment. Such impressive success in the empirical arena, and not any other form of success, is precisely what the realist proposes to explain by means of the appeal to the truth and reference of theories. It is just beside the point that, in other cultural contexts, one might take other sorts of phenomena to be instances of success.

 Jakowljewitsch frames his discussion of the notion of success in terms of what he calls the “success criterion”. On his interpretation of the realist’s success argument, the notion of success functions as a criterion which may be employed to determine whether a theory is true or false. In this context, he raises the question of:

... the potential criteriological efficiency of the success criterion which – like all criteria – ought to enable us to establish effective evaluations of the phenomena that are to be measured. (p. 6)

He then claims that the “success criterion” should be “capable of separating correct scientific theories from incorrect ones, and of correctly assessing in how far the correct ones are true to reality” (p. 7). He goes on to raise a number of objections designed to show that success is unable to serve as a criterion for the correctness of theories.

 While I agree with some of his specific points, Jakowljewitsch’s objections to the “success criterion” seem to me to fall wide of the mark. They are directed against a straw man. The idea of a “success criterion” plays no part in scientific realism.[[5]](#footnote-5) Nor does it figure in the realist’s success argument. The realist’s position is not that success is criterial for truth, so that a successful theory is one that is thereby shown to be true, and an unsuccessful theory is thereby shown to be false. The relation between success and truth is not that of a criterion whose satisfaction entails the truth of a theory. Success is not a necessary and sufficient condition for truth. The connection is much less direct than this. False theories may entail true consequences, and true theories (with false auxiliary hypotheses) may entail false consequences. So, success does not entail truth, and truth need not entail success. Far from serving as a criterion for truth, the success of science is a phenomenon which requires explanation. The realist’s position is not that a successful theory is thereby shown to be true. Rather, the realist’s view is that the best explanation of scientific success is that a successful theory is true, since that would explain the success.

 But this bold statement of the realist position requires immediate qualification, as Jakowljewitsch’s discussion of successful but incorrect theories makes clear (pp. 6-8). His critical analysis of the “success criterion” illustrates that there is no direct entailment from success to truth. In the history of science, there have been theories that are now regarded as true which initially met with little success. Equally, there are theories now regarded as false which once met with considerable success. Such points, which were persuasively presented in an influential article by Larry Laudan (1981), have been the focus of much attention in the literature on scientific realism. These points are extremely important points in the debate about realism because they illustrate the fact that scientific realism must employ a more refined view of the relation between success and truth than sometimes seems to be the case, especially in more streamlined formulations of the position.

 There are two sides to this coin. First, scientific realism must be formulated in such a way that there is no suggestion that in every case of scientific success the theory responsible for the success is thereby shown to be true. Some constraints on the relevant form of success must be introduced. Here two main approaches have been explored in the realist literature. One approach is to suggest that the relevant form of success must be narrowed down even further, so that the success argument is only applied to cases in which novel predictive success occurs (e.g., Musgrave 1988). The second approach, which is currently receiving much attention in the literature, is that the import of the success argument must focus upon only those elements of a theory which are responsible for the success of the theory (e.g., Psillos, 1999, p. 108). I have myself offered the suggestion that these two approaches might be combined, so that the success argument only applies to those constituent parts of a theory that are responsible for novel predictive success (see my forthcoming, chapter 1).

 Secondly, scientific realism must recognize that there may be cases in which empirical success is due to something other than the truth of a theory. Here a number of alternative explanations of success may be mentioned (though this is not meant to be exhaustive). First, and most importantly, it is crucial to emphasize that it is not truth, but approximate truth, that must play the primary role in the explanation of scientific success.[[6]](#footnote-6) Second, there may be cases in which the success of a theory is due to the truth of a low-level empirical law which has been detected by observational means, rather than to the theoretical description of that empirical law within the theory. Third, experimental manipulation of scientific entities may provide scientists with knowledge that enables them to predict and control the behaviour of those entities, even though no satisfactory theoretical explanation of their behaviour may yet exist.

 The remarks of the last two paragraphs fall well short of a fully worked out version of scientific realism. But they will serve to suggest the sort of more sophisticated position that is emerging from the literature on the topic of scientific realism. At the same time, they will serve to illustrate both why Jakowljewitsch is right to criticize what he calls the “success criterion”, and wrong to ascribe commitment to such a criterion to the scientific realist. Realists emphasize and seek to explain the success of science. But they do not treat success as criterial for truth.

**6. Understanding and disagreement**

My aim in ‘What is Scientific Realism?’ was to broaden appeal of the doctrine of scientific realism by providing a clear presentation which would improve understanding of the position. In this response to the paper by Dragan Jakowljewitsch I have again sought to remove some apparent misunderstanding of the realist position. But at the same time there have been substantive points of disagreement which I have sought to address. It is interesting how defence of a position requires both that one resolve points of misunderstanding and that one confront matters of substantive dispute. I will deem this exercise worthwhile if I have partly succeeded on either front.

**References**

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1. I will not, in the present context, challenge the “considerations” mentioned by Jakowljewitsch, which are all well-known doctrines of post-positivist philosophy of science. Suffice to say that all of the “considerations” are controversial ideas which have been subjected to thorough-going critical analysis within the literature of the philosophy of science. While they are widespread and well-known ideas, they by no means occupy the status of unchallenged orthodoxy within the philosophy of science. [↑](#footnote-ref-1)
2. A further reason to resist the call for a conclusive proof for realism derives from the non-demonstrative character of inference to the best explanation on which realism partly rests. [↑](#footnote-ref-2)
3. As it happens, I do not regard the success argument as the only argument in the realist arsenal. It is one of a number of arguments which combine to support the realist position. See my (forthcoming, chapter 1) for discussion. [↑](#footnote-ref-3)
4. Jakowljewitsch misleadingly assimilates constructive empiricism to instrumentalism and phenomenalism. Constructive empiricism is a semantic (though not, of course, epistemic) realist position which adopts a literal interpretation of theoretical discourse (see van Fraassen, 1980, pp. 11-2). As such, it conflicts with the eliminativist and reductionist semantic programs of instrumentalism and phenomenalism. In contrast with constructive empiricism, the instrumentalist or phenomenalist deny that theoretical discourse is genuinely meaningful, and treat theoretical terms either as non-referring expressions or as terms which refer to observable entities (or perhaps sensory contents) rather than to unobservable entities. [↑](#footnote-ref-4)
5. Indeed, Jakowljewitsch’s talk of a criterion seems to import a verificationist element into the discussion that is out of keeping with realism. It harks back to the idea that one must provide a criterion of applicability for a term or concept in order to render it meaningful. Such a requirement is reminiscent of the verificationism of early logical positivism, as well as some elements in the philosophy of the later Wittgenstein. [↑](#footnote-ref-5)
6. In relation to approximate truth, realists tend to draw upon an intuitive understanding of the notion, which has not yet been vindicated by a widely accepted formal analysis. [↑](#footnote-ref-6)