THE SUCCESSES OF SCIENCE AND SCIENTIFIC-THEORETICAL REALISM: A LESS THAN DIRECT CONNECTION

DRAGAN YAKOVLYEVITCH

> "Knowledge does not refer to an 'absolute' world, the nature of which could also be identified without our theories, and the world is not completely opened up to any absolute knowledge."
>
> (J. Mittelstraß)

Abstract. This essay discusses the difficulties involved in legitimating a realist interpretation of the way scientific theories are formed, as well as of these theories themselves. First, I want to offer a sketch of the peculiarities of science and of the possibilities for interpreting it in a realist or antirealist way. I shall also examine at length two arguments in favour of scientific-theoretical realism made in the course of recent discussions.[1] Finally, a suggestion made by H. Sankey for an understanding of this argumentation will be considered.

Key words: scientific realism, reality vs. theory, value judgement, objective truth

I. What constitutes the proximity between science and reality?

It is a fact that the majority of scientists – both natural scientists and social scientists (with the notable exception of scholars of the formal sciences, i.e. mathematics and logic) – are motivated in their scientific practice and the theory formation it entails by basic assumptions that are realist in character: they make their efforts trusting that on the basis of these efforts, it is possible to understand and to describe reality itself or its constituent parts correctly. Thus, separate fields of scientific enquiry are delimited on the basis of separate aspects of reality (inorganic and organic nature, economic life etc.). Once theories have been developed, these aspects of reality will then be taken to constitute the area of validity for these respective fields of scientific enquiry, and the areas in which they must prove themselves. In the investigation of each of these fields, further empirical methods are employed and generalizations are constructed based on cases observed in reality.

[1] Instead of the usual term, "scientific realism", I use "scientific-theoretical realism", since – as Putnam also points out – the attribute "scientific" is generally understood as a value judgement, as well. Thus, the alternatives to "scientific" realism would appear to be "unscientific" or at least "less scientific". Therefore, it is appropriate to give preference to the term "scientific-theoretical realism", which will be abbreviated in the following as "StR".

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In all sciences, the so-called “experimental laws” play an important role. In principle, they will retain their validity regardless of their different theoretical interpretations. Arguments in favour of the realist interpretation of science can be found in the influential empiricist tradition and the convictions of *common sense*. Both see the main objective of scientific enquiry in an adequate description and explanation of reality, and they demand from such scientific enquiry that it involve methods of empirical investigation and examination.

Ultimately, the concrete technological application of scientific knowledge constitutes a trial of theories against reality. Technologies developed on the basis of certain scientific insights test the validity of these theories in reality. Successful practice is then supposed to be evidence of the correct identification of real facts. It is thus assumed that we owe efficient ways of coping with respective aspects of reality to correct scientific representations of reality – e.g., agricultural success is ascribed to the correctness of theories of chemistry, advances in space travel to the correctness of theories of physics, and both kinds of theory are credited with successes in the development of technologies and industrial production of different kinds.

II. What constitutes the distance between science and reality?

In all sciences, the majority of phenomena is interpreted in a considerably abstract manner, which renders the respective theories the more difficult to validate, and to put them to practical application. Phenomena are represented schematically, with some of their concrete features being deliberately disregarded. In economics, for example, behavioural models are developed by ignoring the fact that the behaviour of real economic agents is often less than rational. Later on, the attempt is made to include this factor of irrationality in human behaviour in these models. Thus, idealized theories – based on certain law-like assumptions – are employed for so-called approximative explanations and prognoses of facts and incidents which belong to the subject field of enquiry. (Any difference between the result expected on the basis of such an idealized explanation and the actual result observed in empiric study is interpreted as an error based on an incomplete realization of the idealized conditions called for by the theory.) The elaboration of the required laws by extrapolation from empiric observation and generalization is difficult in those sciences in which experiments are only possible in a very limited way. In these cases, models that do not mirror reality are developed and examined in terms of the implications they have. Ultimately, this leads to the formulation of law-like statements. These statements are the result of metaphysical assumptions as well as of the hypotheses of the models, both of which combine to formulate a kind of ontology, an image of the nature and functioning of the phenomena of the subject field of enquiry. In the other sciences, too, theoretical terms and theoretical propositions of laws on a high level of abstraction often occur, which cannot be related back to empirically founded terms and principles.

Owing to these, and similar, circumstances, philosophers of science currently largely tend to accept the thesis that scientific statements and theories are empirically underdetermined, as it were, and that there is thus no straight path from empiric enquiry to the elaboration of explanations, prognoses or theories. On the other hand, there is also another widely accepted thesis that emphasises the fact that observations and empirical statements made by scientists are often theoretically charged, i.e. informed by theoretical assumptions and background knowledge. This fact also renders the objective examination of those observations and statements more complicated, as P. Duhem emphasised at a very early point. A well-known example for this phenomenon is provided by observations in astrophysics, which are made with telescopes and depend on a theory of light and optics that is at least assumed as background knowledge for making such observations in the first place.

Apart from the diachronic relativity of different world views, which have been offered within the history of science, the synchronic relativity of different interpretations of one and the same scientific theory – e.g., quantum mechanics – must be considered, also. It is well known that Bohm and Heisenberg developed two different interpretations or forms of expression for this theory which – as Mittelstraß points out – produce empirically equivalent, but conceptually completely different, formulations, so that it is not possible to decide for or against either formulation by taking into account observations only. Since one and the same theory may be connected with several contradictory and ambivalent interpretations, that theory's reference to reality will appear less definite.[2] As Mittelstraß asserts, a given theory "delimits the range within which world views may be developed, but it does not of itself definitely commend one world view over others."[3]

Ultimately, all scientific practice is informed by value judgements and decisions which, in spite of a certain connection with reality, clearly transcend reality and relate to the subjectivity of the theoreticians and scientists making them. It is exactly these value judgements and decisions by leading scientists that T. S. Kuhn, P. Feyerabend and others see as the key instances for great paradigm changes in the history of science, because these changes are about choices of theory, i.e. the preference for one paradigm over its alternatives.

It is considerations as these that have largely discredited not only the so-called "common-sense realism" in current discussions among philosophers of science, but also the term "objective truth": it is now used with much greater reservations and caution than at the beginning of the 20th century. There seems to be no way back to the naïvely realist ideas of scientific theories and the entities proposed by these theories. In both fundamental directions, namely from reality to

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[2] Cp. Mittelstraß (2002).

[3] *Ibid.*, p. 245 (transl. S. Seippel). Mittelstraß also draws attention to another example from the history of science, *viz*. the different potential interpretations of Maxwell's theory of electrodynamics. The mechanistic tradition of the 19th century interpreted the electromagnetic field as one "state of a mechanical ether", whereas Einstein viewed it as an "independent factor" (cp. Mittelstraß (2001), p. 20).

theory (through empiricism) as well as from theory to reality (via empiricism), the path towards a direct and completely definite connection appears quite blocked.

III. One common argument in favour of Scientific-theoretical Realism, and its limitations

The above-mentioned loss of definiteness of the relationship between reality and science, or rather of the reference to reality of scientific theories, which can hardly be compensated at the current point in the development of modern philosophy of science, has created a new and complex situation. Correspondingly, attempts are also being made to develop a different kind of argument in favour of StR. Sometimes, recourse is taken in this context to traditional pillars of the realist interpretation (like practical success based on science). This latter strategy has been re-employed, in an updated fashion, by H. Putnam and (in his wake) by H. Sankey, in a most interesting contribution to the discussion. Sankey rightly points to the difference between "negative" and "positive" arguments in favour of realism and advances the *success argument* as a positive argument, for which he is indebted to Putnam.[4] (It has already been mentioned that this argument antedates Putnam's work, but he has placed special emphasis on this argument in the recent discussion.) After two quotations from Putnams works, Sankey offers the following summary of the success argument:

> Given the truth of its theories and the reality of its entities, it is only to be expected that science should manifest the striking degree of success that it does. Because scientific realism provides so natural an explanation of the success of science, while alternative approaches provide an unsatisfactory explanation, we should accept scientific realism as true. This kind of argument is called an inference to the best explanation. [...] Such arguments tend to be compelling but not conclusive. For example, one might argue that the hypothesis of an external world is a much better explanation of our experience than is a Cartesian hypothesis of an evil demon. We do not thereby conclusively show the Cartesian hypothesis to be false. But we provide a reason for believing the external world hypothesis. Similarly with scientific realism. Inference to best explanation provides reason to accept scientific realism without necessarily proving it true or refuting the alternatives.[5]

There are two aspects to this discussion: on the one hand, there is the definite statement of the argument itself, and on the other, there is its specific interpretation. In my view, the above dipartite formulation of the argument is not entirely fortunate. The first part rests on 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. If one reads this statement in the hypothetical way in which it is intended ('If we assume that the truth of scientific theories and the

[4] Cp. Sankey (2002). This publication is a contribution to the discussion on scientific-theoretical realism called for by the editorial board of DIVINATIO.
[5] Sankey (2000), pp. 117–118; emphases added.

reality of science's entities be given ...'), then this aspect of Sankey's argument would be rather trivial – in terms of an inference, it would state:

> If the (hypothetical) condition Z, which might potentially explain X correctly, were met, X would be explained correctly.
> Let us assume that Z were given.
> Thus, X is explained correctly.

An inference of this kind simply affirms the condition on which the argument rests and repeats it, without at the same time stating the reason why this condition ought to be seen as fulfilled. Structured in this way, the first part of the argument merely points towards possible correlations between StR and the success of science. No well-founded inference is made from this, on the basis of which a conclusion might be drawn that would provide an explanation for StR.

This inference is discussed in the second part of the argument, which is supposed to deliver a verdict. In a rather imprecise way, it identifies the question of how the success of science is to be explained as the desired basis on which the validity of StR can be concluded. Sankey reads this inference itself as a so-called "inference to best explanation", while at the same time stating that it is not conclusive. Before we discuss this specific interpretation of the success argument and try to explain the argument's inevitable lack of conclusiveness (which Sankey does not do), I think it is more pertinent to reverse the argument: i.e. to start from the assumption that science has achieved great successes, and to ask then what inferences this allows with regard to StR, or to the assumption that science's theories were true and its entities real. Next, it must be analysed how the offered interpretation for the argument may be defended. In order to do so, I would first like to restate the "success argument" as follows:

> (1) Scientific theories achieve success in predicting and describing phenomena and in controlling the natural environment by means of technology (an empirical statement, i.e. a statement of fact).
> (2) StR provides the best explanation for this fact (an assessment).
> (3) Thus, we should accept StR.

If we deem (1) to be beyond reasonable doubt, (2) remains as the disputable (and at the same time decisive) step in the argumentation. It is an assessment, and assessments can be questioned. Those theoreticians of science who argue in favour of StR in the way sketched above are aware of this and therefore sometimes introduce a further assumption on which (2) is intended to rest. This assumption (which itself could be regarded to constitute a further argument) states that

> (2') All alternative explanations of the success of scientific theories make these theories to miracles or cannot give satisfactory explanations, and therefore, they leave (1) unexplained (i.e. a "miracle").

However, this additional assumption, which Sankey also seems to make, entails two significant problems:

(A) First, even if the entire set of alternatives to StR were formulated, and all of these alternatives $A_1,...,A_n$ were eliminated with the aid of the miracle argument (2'), it would not be possible to conclude with any certainty that StR is correct. The insufficiency of alternative explanations is not a positive reason for the correctness of the only remaining alternative – i.e. StR. If StR itself as the only remaining alternative is correct cannot be decided by excluding the other alternatives. If the comparative interpretation was correct (!), this exclusion merely proves that $A_1,...,A_n$ each provide worse explanations than StR, and that StR is thus better than each of them. Whether this better explanation, as 'the best one', is actually correct when considered absolutely, is another question. We cannot simply disregard the possibility that StR, even if it gives a better explanation than $A_1,...,A_n$, might also employ problematic or even incorrect assumptions. (Thus, such a better explanation might just employ assumptions that are *less incorrect* than those employed by the other alternatives.) Whether this is the case or not cannot be determined merely by eliminating the alternative explanations.

There is a methodological relation between the miracle argument and the assertion that StR is the "best" explanation for success of science: by stating, on the basis of this argument, that $A_1,...,A_n$ would render the success of science an (unexplainable) miracle, StR is cast as the only remaining alternative or explanation and at the same time as "the best", *viz*. the best available explanation. But the best *available* explanation, isolated in this way, must not be identical with the best solution in the sense of an optimal explanation for a particular fact.

(B) Furthermore, this additional assumption of the miracle argument, considered by itself, is not automatically self-evident or rather as logical as it is generally assumed by proponents of this line of argumentation. Therefore, it has to be examined first whether all alternatives to (2) really will do as badly, or whether some of them might not offer an acceptable explanation.

In doing so, we ought not to be distracted by the suggestive analogy with the explanation of our experience, employed to rhetorical effect by Sankey. In this analogy, the Cartesian alternative of an explanation that assumes the existence of an evil demon fares badly in comparison to ontological realism (although even this might not be entirely beyond dispute). There have been, and there are still, more promising and more acceptable alternative explanations for the success of science than such assumptions, which can hardly be made plausible. Thus, for instance, we ought to consider the alternative offered by the traditional phenomenalistic challenge: seen in the context of van Fraassen's "constructive empiricism", it is a point of view that today must be seen as well worth consideration. This alternative suggests that we renounce StR as an unnecessary metaphysical assumption and be content with the more restrictive explanation of (1) which states that scientific theories that lead to success have proved "empirically adequate", and that this is all we have to assume in the given context.[6] To summarize the point of van Fraassen's argumentation: the

[6] It might be noted that Immanuel Kant, too, was of the opinion that the phenomentalistic interpretation of science was absolutely satisfactory and that there was no need to look for

claims for validity of scientific theories can be redeemed without reference to metaphysical realism! This interpretation of (1) provides a provisional explanation; it has explanatory power we might perhaps assess differently, but it surely does not render (1) a miracle. Furthermore, there might actually be some merits to commend such an interpretation as compared to StR. In terms of logic, for instance, this hypothesis is more easily defended because its claims are much more limited. Neither does it involve us with those well-known difficulties attached to StR (e.g. those connected with the theory of a convergence towards the ultimate truth). This would be another reason not simply to accept the additional "miracle argument", which is actually used to bolster the "success argument" – especially since it employs the wrong, indefensible premiss which says that strictly speaking, there are no sensible alternatives to StR. In principle, this argument would be acceptable in a much more moderate form, *viz*. as a means of qualifying possible alternatives to StR in the sense they are "less able to explain", or that they explain "less convincingly". This would make it the product of a more differentiated comparative evaluation of possible and useful alternative explanations, an evaluation of those explanations in terms of their explanatory power. Looked at in this way, the (non-)miracle argument as a further premiss becomes superfluous, especially since (2) already expresses this kind of comparative evaluation.

The miracle argument would only contribute something to this argumentation in its strong form, namely as a claim which eliminates all alternatives to StR in advance. But it has proven to be empirically untenable in this form. It is therefore more sensible to return to the original argument in favour of StR, i.e. to do without an additional assumption of this kind.

Let us then return to the question whether (1) (i.e. the success of science) is a good basis for StR. Regarding this question, counter-arguments emerged in the scientific-theoretical discussion which questioned the very "success criterion" itself. Thus, Feyerabend has pointed out that the attempt "to see the success of science as the measure of the reality of its constituent parts" fails. For 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".[7] According to this thesis, every claim to success (or failure) must be relativized historically, i.e. viewed in relation to the respective specific cultural practices which in one period of human history or another were regarded as decisive (or still are). Independent of such cultural practices, this kind of claim cannot rightly be made. Even though we might not want to agree with this kind of relativization of the success criterion (which Feyerabend proposes in the context of his historicist view of science), there remains one important implicit aspect to his argument that has to be considered in the present context: it is the potential criteriological efficiency of the success criterion which – like all criteria – ought to enable us to establish effective

further explanations that would also account for the metaphysically assumed facts *per se* that are "behind" the world of our common empiricism.

[7] Feyerabend (1989), pp. 189–190 (transl. S. Seippel); emphases added.

evaluations of the phenomena that are to be measured. In this context, two kinds of impediments can occur:

(i) It is a well-known phenomenon from the history of science that a sound theory (often one that is later accepted) can have problems in getting established – one example might be heliocentric ideas in physics.[8] Occasionally, theories of this kind have taken centuries until they were established.
Thus, there are theories which today could be regarded as correct in terms of StR, but which did not meet with success or the recognition that would accompany it over long streches of time in the history of science. It cannot be ruled out completely, either today or in the future, that scientific theories, which will be generally accepted at later points in time, will not meet with success but rather be crowded out by worse competitors because of the respective historical circumstances.

(ii) Also, the history of science has seen concepts meet with success even though they later turned out to have been mistaken.
Circumstance (i) emphasises that the success criterion can prove a criterion with a potentially postponed effect, i.e. its strength for decision-making may take effect only after some delay. This circumstance weakens its potential applicability within the scope of the respective arguments in favour of StR, but it does not affect its fundamental capacity for support. In other words: the use of this criterion can entail difficulties which can endure (sometimes for centuries) in each respective, historically unique, situation. It is assumed that ultimately, the success criterion ought nevertheless be capable of separating correct scientific theories from incorrect ones, and of correctly assessing in how far the correct ones are true to reality. Its operative reliability, on the other hand, might vary in each concrete situation in the history of science. Now, does that make the success criterion an inherently good criterion that is merely hard to apply in some instances?

(i) also demonstrates that merely because one does not meet with the success one expected (or perhaps even experiences failure), this is not automatically taken as manifest proof of the given theory's definite incorrectness.

If we do not view the success criterion in a metaphysical way, and if we do not want to speak of the "success *per se*" of correct scientific theories or of the "failure *per se*" of incorrect scientific theories – irrespective of their factual record – it is more appropriate to view the criterion's potential use in terms of a time index and to regard it as rooted in its respective research context, or the current level that

[8] The importance of this experience for an interpretation of the rationality of scientific knowledge has been pointed out by several philosophers of science, among them Feyerabend (1998).

has been achieved in the evolution of scientific knowledge. In this respect, a relativization of the efficient operative applicability of the success criterion appears inevitable. Another possible strategy, and one that would be complementary to this one, would be to make the following claim: under ideal conditions – which, as we know, will often enough not be met in the history of science – the success of a theory would be an unequivocal indicator for its correctness, while its failure would be an indicator for its incorrectness. As we shall see presently, however, the relation between the successes of science and the correctness of scientific theories is not as direct as it may seem.

Circumstance (ii) entails even more serious doubts regarding the success criterion. The possibility that wrong assumptions and theories can lead to successful prognoses and practical instructions shows that success must not be regarded as an exclusive feature of correct assumptions or theories. Under this circumstance, the argument in favour of StR based on success is damaged and loses a large part of the unequivocalness that is central to the argumentative purpose. Let us consider two examples from the history of science.

The first is an example given by Nancy Cartwright and introduced into the present discussion by I. Hacking: according to the current theory of light, the photon is an "integral element" of adequate conceptions of light. But there are also authorities in the field of optics who seriously question the existence of photons and argue that a "more profound theory" would provide evidence that the photon is primarily an "artificial product of our prevailing theories" of light. Such appraisals do not suppose "that the prevailing theory of light is simply wrong", but that "a more profound theory (...) would retain most of our prevailing ideas about light, but would demonstrate at the same time that the effects we associate with photons actually obey a wholly different aspect of nature."[9] (Cartwright points out that from this point of view, one could take an antirealist opinion regarding photons without at the same time rejecting outright the idea of realism in general.) The effects that are attributed to photons might thus be described correctly, and based on these effects, correct prognoses could be made and the experiments and (technological) actions connected with our assumptions about such relations of effect could be employed to success.

I take the second example from a very stimulating essay by Mittelstraß. It concerns the former caloric theory and the use it was put to in the production of heat engines.[10] The incorrect caloric theory, which was the basis for Carnot's early 19th-century development of a conception for heat engines that is still recognized today, stated that heat is "a special substance" that is "able to force its way into the body and to cause a rise in temperature." Mittelstraß comments:

> Since heat thus has a material nature, it is preserved in all thermal processes – such processes merely signify an exchange of caloric, but not its generation

[9] Cp. Hacking (1996), p. 57. My summary of Cartwright's argumentation is also based on Hacking.
[10] Cp. Mittelstraß (1997), pp. 294–295.

> or destruction. This conservation law suggests that thermal processes ought to be viewed as cyclical processes; a full analysis of one such process should then demonstrate how the initial thermal state is achieved again. And this is precisely the point of departure of Carnot's theory of the heat engine. Had Carnot started out instead from the more correct idea of the dissipation of heat or energy, he hardly would have considered the cyclical process as an analytical tool.[11] (...) In fact, heat does not have a material nature, and neither is it subject to a conservation law. Nevertheless, the erroneous, incorrect approach of caloric theory led to an analysis of heat engines that is correct in principle. Even today, particularly the calculation of energy efficiency and the independence of energy efficiency from the material medium used, both of which are based on this process, are recognized.[12]

In this instance, incorrect assumptions about the nature of heat (as well as the analogies suggested by these assumptions) led to correct prognoses and the development of a quite efficient technology!

In both examples, the following situation presents itself: because of assumptions and theories (the assumption of the existence of photons, caloric theory) that have either proven incorrect or at least been seriously challenged in terms of their correctness, certain ideas were formed about causal relationships that in turn facilitate the development of successful explanations and prognoses, or of successful technology (heat engines etc.). In cases such as these, incorrect theories give an idea that is *in principle* correct about the way things are, but they do not give an idea about what the things *really* are.

Therefore, in certain cases the success of science can be based on assumptions and theories that are seen as problematic or that may even prove incorrect. In my opinion, this is precisely the point why the success argument cannot be conclusive (even if we did not want to take it as an inference to the best explanation)! There have been successes, and there will in all likelihood also be future successes, which are not based on correct, but on more or less incorrect, assumptions and conceptions. By definition, incorrect assumptions do not correspond to reality, they do not have a reference to reality. This (perfectly real, as we have seen) possibility of a success of science based on incorrect assumptions discredits a major part of

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[11] *Ibid*., p. 294 (transl. S. Seippel). As Mittelstraß points out, the crucial point was "the analogy between a heat engine and a hydro-electric power station": "Since heat has a material nature, a heat engine's mode of operation follows the example of power generation by falling water. In the first phase of the Carnot process, caloric is absorbed; this corresponds to the collection of water in a pool. In the second phase, caloric is allowed to descend, as it were, from a higher temperature level to a lower one, just like water would fall down a certain height. The remaining phases of the Carnot process correspond to the pushing back of the water onto the higher level. The second phase represents the process that is crucial for the effect of a heat engine, i.e. the running down of hydrogen, and this idea is essentially founded on the analogy with falling water and with the assumption that heat has a material nature" (emphases added).

[12] *Ibid*., p. 295 (transl. S. Seippel).

the success criterion and its potential use for bolstering StR. One might agree with Sankey and try to compensate for this by claiming that the success of scientific theories merely provide us with "one reason" for accepting StR, this would be cold comfort indeed. For one good reason might also be fielded in favour of alternative interpretations of science, e.g. for instrumentalism or phenomenalism and especially for the revival and reformulation of the latter by van Fraassen. An instrumentalist explanation for successes of science is quite possible, at least with respect to research, i.e. science's functions of problem solving and problem prognosis. (Especially with regard to these functions, Thomas S. Kuhn (1976) was also prepared to speak of a continuous progress in the history of science.)

In conclusion, it must be stated that for StR to be viewed as the best explanation, it requires stronger support than that given by Sankey.

In the current discussion, it is thus not the case that we have a well-founded opinion, StR, on the one hand, and completely ill-founded alternative opinions on the other hand. Instead, we are dealing with the two well-founded opinions, which are subject to comparative evaluation and ought to remain so. Therefore, I suggest that we view StR explicitly as one hypothetical assumption for explanation that must compete with other sensible interpretative hypotheses (e.g. instrumentalism and phenomenalism).

This kind of comparative evaluation would have to be based on more thorough considerations of common sense, of the logical aspects of alternative interpretations and their capability for explanation, of experiences from the history of science and of explanations for the behaviour of scientists themselves. Instead of striving for some compelling "proof for realism", we will lend our support to StR to the extent to which alternative interpretations of science cannot rationally legitimize their claims to be the better options. This is obviously to make a weaker claim than the majority of supporters of StR, including H. Sankey, would like to see. An argumentation of this kind, however, opens up a path along which, in most cases, StR can prove itself in comparison with its competitors to be the more satisfactory hypothesis for the interpretation of science, and the one that in total has a greater capacity for explanation.

(Transl. from German by *Sonja Seippel*)

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