Review: Cornelis Menke: Zum methodologischen Wert von Vorhersagen (On the Methodological Value of Novel Facts). Paderborn: Mentis, 2009, 188 pages

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The book at hand is a slightly modified version of the dissertation of Dr. Menke, member of the *Institute of Science and Technology Studies (IWT)* at the University of Bielefeld. The main problem under discussion is a paradox of confirmation by novel facts which was originally formulated by Hempel ([cf. 2, pp.37f]). Although the investigation of Menke is a much broader one, within this review his claims are formulated and discussed mainly with respect to Bayesian confirmation theory. So, many of the following formulations are strictly speaking not by the author, but straightforward applications of general claims of the author.

The mentioned paradox can be formulated as follows ([cf. 4, p.7]):

Argument 1

Thesis 1 *Some hypotheses can be confirmed by novel and well-established facts.*

Thesis 2 *From a practical point of view hypotheses are more confirmed by novel facts than by well-established facts. That is: if* $conf(T, A) = x$*, given* $Novel(A)$*, and* $conf(T, A) = y$*, given* $\sim Novel(A)$*, then* $x > y$*.*

Thesis 3 *From a logical point of view novelity is only of historical interest and does not affect confirmation. That is: if* $conf(T, A) = x$*, given* $Novel(A)$*, and* $conf(T, A) = y$*, given* $\sim Novel(A)$*, then* $x = y$*.*

Hence: $\lightning$

As far as, e.g., verification and other methods of testing hypotheses are considered as edge cases of confirmation, thesis 1 is accepted by most of the classical philosophers of science. Thesis 2 is accepted by, e.g., William Whewell and Lakatos and is justified variously. Thesis 3 is according to Menke accepted by, e.g., John Stuart Mill and Roger

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Rosenkrantz and is in general justified with two claims: (i) The value of novelity is incompatible with other methodological values. (ii) That a fact is novel is a purely historical (random) matter and randomness is irrelevant regarding confirmation ([cf. 4, p.9]). It is easy to figure out that there is an alternative claim to thesis 2 regarding confirmation by novel facts: the paradox persists if one claims instead of this thesis that hypotheses are more confirmed by well-known facts than by novel ones. Menke discusses this assumption implicitely ([cf. 4, p.23]):

Thesis 4 If $conf(T, A) = x$, given $Novel(A)$, and $conf(T, A) = y$, given $\sim Novel(A)$, then $x < y$.

The whole book is an investigation of arguments for and against generalized versions of the theses 2–4. In the following the most important definitions and arguments of this investigation are given. But before we come to the detailed discussion of the single theses, let us begin with a short summary of Menke's investigation of characterizations of the expression 'novel fact'!

On novel facts

Menke specifies as a desideratum for theories on the value of novel facts an adequate explication of 'is a novel fact' ([cf. 4, p.12]). He also lists some conditions of adequacy for such an explication that were discussed in the past. He thinks that there are three relevant parts of these conditions that serve more or less as explicational desiderata ([cf. 4, p.106]):

(1) Popper's proposal: if a fact is novel, then it is unexpected.

(2) Lakatos's proposal: with respect to a theory novel facts are facts that are not predicted or explained by competing theories.

(3) Whewell's proposal (consiliences of induction): with respect to a theory novel facts are facts of a different kind than those facts the theory originally was constructed for.

Similar to Whewell's proposal is that of Elie Zahar who thinks that a fact is novel with respect to a theory only if it was not used for constructing the theory ([cf. 4, chpt.2]). Menke thinks that Whewell's proposal is the most relevant one inasmuch as exact formulations of Popper's and Lakatos's approach coincide in their correct parts with that one of Whewell ([cf. 4, pp.107–110]). Whewell's proposal also allows one to talk of novel facts if a predicted fact is already well-known – take as an example the perihelion precession of Mercury which was well-known (even more, seen as an anomaly of classical mechanics) when Albert Einstein formulated his general theory of relativity and which in some methodological investigations is counted as novel fact predicted by Einstein's theory.

The main problem of Whewell's proposal is a missing characterization of 'different kind'. Menke discusses some explanitory attempts and ends up with the – to his mind still unsatisfactory – characterization of Colin Howson and Peter Urbach ([cf. 4, pp.125–127]):

> A fact E_2 is of different kind than a fact E_1 iff E_1 and E_2 are "uncorrelated", that is with respect to a probability function p: $p(E_2, E_1) \neq p(E_2) \cdot p(E_1)$.

NB: novel facts are not assumed to be temporally novel. They are just in the sense of Whewell's proposal novel with respect to a theory. Also predictions are not assumed to have a specific time sequence, so explanations (in the classical sense of the word) of novel facts (in the sense of Whewell's proposal) count as predictions (in a non-classical sense) of these facts.

On the value of well-established facts

With this characterization of novel facts in mind we can start now with Menkes discussion of the claim that well-established facts confirm a theory more than novel ones. Thesis 4 is accepted and justified, as the author points out, by Stephen Brush with two arguments ([cf. 4, p.16]):

Argument 2 *Theory assessment is possible only with respect to well-established facts; this holds, so Brush, because one can say that alternative theories should be rejected only if the confirming or undermining fact is known to be true or highly probable; and this holds only for well-established facts, but not for novel ones. Hence: thesis 4*

Argument 3 *It is more likely to fail with an interpretation of novel facts than to fail with an interpretation of well-established facts. That someone fails with an interpretation of a fact is to be meant as: she thinks that the fact confirms a theory, although the theory is not really confirmed by the fact. This happens, e.g., if a scientist accepts some basic statements which confirm a theory, although at least one of the basic statements is in fact false. Hence: thesis 4*

Menke valuates both arguments as weak. The premises of argument 2, so the author, are implausible, because in support of these claims one has to show that theory assessment according to well-established facts leads to better results than theory assessment according to novel facts; but the examples discussed by Brush, the perihelion of Mercury and the deflection of starlight by the sun, are not that like. We add the objection that assessment according to novel facts is possible only in case that the facts are already accepted or refuted statements. So, e.g., in 1918 the claim that the sun deflects light of stars neither confirmed, nor undermined the general theory of relativity, because there were no justified states of belief about this claim. But in 1919, when the expedition team of Sir Arthur Eddington meassured a deflection, there was some knowledge about the probability of this claim and as a result it was said that this claim confirms the general theory of relativity.

The premises of argument 3 are accepted by Menke, as far as they are true psychological statements ([cf. 4, p.23]). Nonetheless the author doesn't draw the lesson to claim thesis 4, rather he suggests to take care in claiming that a prediction is really shown to be true (and hence the predicted statement is seen as novel fact).

One may ask how Brush supports his more or less theoretical claims empirically. The first classical test of the general theory of relativity, namely the prediction that the sun deflects starlight, is generally seen as highly confirmatory for the theory and may be seen as counterexample to Brush's main thesis. To answer the question and to solve this problem, Brush gives two more arguments ([cf. 4, chpt. 2 and 5]):

Argument 4 *In physics the word 'predicts' is often used for 'implies' and so if someone says that a theory predicts a fact and that the fact is highly confirmatory for the theory, then she means simply that the theory implies the fact and that the fact is highly confirmatory for the theory, wheter it is novel or not. Hence, e.g., the claim that the third classical test of the general theory of relativity, namely that the theory allows one to calculate the perihelion of Mercury, was a highly confirmatory prediction is simply to be read as 'the theory and some boundary conditions imply a description of the perihelion of Mercury and this implication is highly confirmatory'.*

Argument 5 *That the deflection of starlight by the sun has been shown to be true was not discussed much in scientific literature. Einstein himself said that the outcome of the expedition was not of much interest for him. Hence, the prediction of the deflection had less impact in science, but much impact in popular science.*

So, argument 4 suggests to reinterpret claims about the confirmation by predictions as claims about the confirmation by implications whether novel or not; and argument 5 suggests to reinterpret those claims about confirmation of really novel predictions as in fact irrelevant. Menke argues against the premises of both arguments with very relevant statistical data. He gives two criteria for testing the claim of Brush about the usage of 'predicts' in physics ([cf. 4, p.71]):

Criterion 1 As far as the relation of implication is time-independent (NB: the relation of prediction is not time-independent) and the relation of implication is between sentences/theories and sentences (NB: the relation of prediction is normally between people, arguments and facts), only claims about predictions in presence and with names of theories as logical subject are potentially claims about implications.

Criterion 2 If in texts with many claims about implications 'predicts' is relatively little used, then claims about predictions are likely to be not claims about implications.

Menke investigated texts of physics with the help of these criteria and found out, that 'predicts' is commonly used for 'implies' in physics only from the fifties on. So, the first premise of argument 4 doesn't hold for predictions like the deflection. Also the investigation of popular journals shows a similar result; hence the conclusion of argument 5 seems to be not very likely. Without doubting the conclusions the author draws from his investigation, we mention two small problems. First, one may use 'predicts' in the sense of 'implies' in a sentence although the logical subject of the sentence is a person (vs. criterion 1): e.g., 'Einstein predicted the perihelion of Mercury.' may be understood as 'Einstein showed that his theory implies a description of the perihelion of Mercury.'. Second, the absolute numbers of Menkes statistical data may be of limited significance as far as the number of pages of investigated journals increased rapidly ([cf. 4, p.72]): e.g., the collection of *Physical Review* of 1893-94 has 462 pages whereas the collection of 1959 counts 6748 pages. So the – for Menkes argumentation crucial – data of the early journals may not be as reliable as the author thinks.

Against the claim that even for Einstein himself the outcome of the first classical test of the general theory of relativity was not of much interest, Menke refers to the usual distinction between *prior* and *posterior* probabilities ([cf. 4, p.20]). His hint can be unfolded as follows: that the outcome of the expedition for meassuring the deflection was of no interest for Einstein means that the *prior* probabilty and the *posterior* probability (that is the probability after gathering knowledge about the deflection) of the general theory of relativity (for short 'GTR') are the same. So it holds: $p_{pri_{Einst.}}(GTR) \approx p_{post_{Einst.}}(GTR)$. In a Bayesian way of information update it holds that, if a fact – say a confirming outcome of the expedition (for short 'DLS') – is accepted, then the *posterior* probability of a theory, say GTR, is identified with the *prior* probability of the theory that is conditioned to the fact DLS. So it holds: $p_{post_{Einst.}}(GTR) = p_{pri_{Einst.}}(GTR, DLS)$. From this it follows: $p_{pri_{Einst.}}(GTR) \approx p_{pri_{Einst.}}(GTR, DLS)$; and this holds in case that $p_{pri_{Einst.}}(DLS)$ is very high, that is: Brush's argument 5 can be undermined by claiming that Einstein didn't expected a negative outcome of the expedition ([cf. 4, p.21]).

So the arguments in favour of thesis 4 are not very convincing.

On the equality of novel and well-established facts

Menke gives a case study of the so-called *prediction of Poisson* and claims that this case may be used in support of thesis 3. In accordance with John Worrall the author judges the usual story of the impact of the prediction of Poisson as fairy tale ([cf. 4, pp.26ff]): the french physicist Siméon Poisson deduced in 1818 from Augustin Fresnel's undular theory of light that there exists a spot at the center of the shadow of a circular opaque obstacle with the same brightness, as there would be without the obstacle; this prediction was proven to be true by an experiment of Dominique Arago. Thomas Kuhn, so the author's claim, brought in the story of the tremendous impact of this prediction by claiming that in an academic competition on the debate of the truth of the corpuscular or the undular theory of light, it was of main importance. This claim is, as already said, fought by Menke based on the argumentation of Worrall by providing some historical data that undermines it ([cf. 4, p.27]). As Menke points out, Worrall even goes a step further and claims that this case shows that more or less often novel facts like Poisson's prediction are of equal or less importance for theory assessment than well-established facts, e.g. some of those deduced by Fresnel himself. Apart from Worralls claim that also Whewell stressed an impact of Poisson's prediction, the author also rejects Worralls claim that this supports thesis 3 by valuing the prediction as no novel fact ([cf. 4, pp.33ff and p.40]).

Although this case study may be seen in support of thesis 3 (depending on counting the prediction of Poisson as novel fact or not), Menke undermines this thesis with an argument that seems to be the most important argument of the book. In the following we are going to present this argument.

The argument depends on two other arguments, namely the so-called *no miracles* argument in the debate of scientific realism and the argument of random explanations. Both arguments are connected closely, as far as the *no miracles* argument is strong, if the argument of random explanation is weak and vice versa. Not explicitely formulated, but strictly suggested by the text, the author understands 'random explanation' and 'random prediction' in the following way ([cf. 4, p.143]):

Definition 1 *An argument A is a random explanation/prediction of the fact E iff A satisfies all criteria of strict explanations/predictions with respect to E, e.g. all criteria of deductive-nomological explanations/predictions, except the criterion that all premises of A have to be true or highly probable.*

NB: the distinction between strict predictions and random predictions is disjoint. It's easy to see that random predictions of facts don't guarantee the facts to be true, whereas, e.g., a deductive-nomological prediction A of a fact E guarantees that E is true, because all premises of A are true and the set of all premises of A implies a description of E. With the help of this definition at hand the underlying thought of the *no miracles* argument can be simply put: true theories provide strict predictions only; false theories provide random predictions only. The *no miracles* argument reads, simplyfied, as follows ([cf. 4, p.139]):

Argument 6 *If no theory is (correspondence-theoretically) true, then all predictions with the help of theories are random predictions. There are some strict predictions in science. Hence: at least some theories are (correspondence-theoretically) true – this is the main thesis of scientific realism.*

The classical test of the main thesis of scientific realism is, as the author claims, a search of scientific research programmes in the history of science. In particular one may support the main thesis of scientific realism by showing that some parts of the most important theories – the so-called core of the scientific research programme of the theories – remain unchanged and so may plausibly be accepted as true ([cf. 4, p.144 and p.146]). Menke suggests another test of the main thesis of scientific realism, namely to show that some theories provide only strict predictions of facts, but no random ones. Of course one cannot test the thesis directly, because in showing that a prediction with the help of a theory is not random, one would have to show that the premises of the prediction are true or highly probable and that would be to show that the laws of the theory, used in the prediction, are true or highly probable. But one can test the thesis indirectly by underminig the premises of the argument of random explanation which reads as follows:

Argument 7 *If the most important theories are (correspondence-theoretically) true, then the most important predictions in science are strict predictions. The most important predictions in science are random ones. Hence: the most important theories are not (correspondence-theoretically) true – this is the main claim of anti-realism in philosophy of science.*

If the most important predictions in science are only random ones, than it's plausibly to assume that all theories providing these predictions fail in predicting other facts; this is due to the assumption that if a theory never fails in a prediction, then it must be more than coincidence – it must be because of the truth of the theory. So, if an important theory T_1 provides only a random prediction of the fact E and E confirmes T_1 because E has been proven to be true, then one may plausibly assume that there are some "facts" $E_1, E_2, \ldots$ such that T_1 provides random predictions of $E_1, E_2, \ldots$ and T_1 is undermined by $E_1, E_2, \ldots$ because $E_1, E_2, \ldots$ have been proven to be false. In particular one may plausibly assume that the amount of confirming predictions and undermining predictions of theories is equally distributed between competitive theories. With the help of this assumption the main thesis of scientific realism seem to be testable: if it's possible to support within history of science the claim that confirming and undermining predictions are distributed equally between competitive theories, then the most important predictions in science are only random ones and scientific realism is undermined; otherwise the most important predictions in science seem to be strict ones and scientific realism is confirmed. Menke claims that the historical data confirms scientific realism because it supports the thesis that confirming and underminig predictions are not distributed equally between competitive theories. Rather, few theories provide almost exclusively confirming predictions ([cf. 4, p.150]). By this one can see that novel facts, but not well-established facts, are of importance for proving a theory to be true or highly probable and hence novel facts are not of equal, but of higher value than well-established facts. Let's sum up the main argument now!

Argument 8

(1) If no theory is (correspondence-theoretically) true, then all predictions with the help of theories are random predictions. (Premise one of argument 6)

(2) If all predictions with the help of theories are random predictions, then the amount of confirming predictions and undermining predictions of theories is equally distributed between competitive theories. (cf. argumentation vs. premise two of argument 7)

(3) Confirming predictions and undermining predictions of theories are not equally distributed between competitive theories – this can be shown only with the help of novel facts. (History of science, [cf 4, pp.147ff])

(4) Hence: some theories are shown to be (correspondence-theoretically) true or at least highly probable with the help of novel facts and couldn't be shown to be true or highly probable with the help of well-established facts only. *(1–3)*

(5) Hence: vs. thesis 3 *(4)*

Premise 3 is argued for with the claim that only predictions of novel facts guarantee that such a prediction was not the result of a modification of the theory in order to predict the fact in question. The *inference ticket* for passing into 5 is obviously the fact that making the truth of a theory plausible is to confirm the theory.

On the value of novel facts

The arguments against thesis 3 and 4 are also arguments in support of thesis 2 (under the assumption of thesis 1). Beside the support of thesis 2 *ex negativo*, one may also support this thesis directly. Menke discusses four kinds of theories in support of thesis 2:

(1) *Predesignation* ([cf. 4, chpt. 6]): according to Charles Sanders Peirce the methodological value of novel facts is similar to the methodological value of predesignation and as far as the last mentioned one is justified, the first mentioned one is justified too. A hypothesis and some conditions of testing a hypothesis are said to be predesignated with respect to some data, iff the hypothesis and the conditions of testing are fixed independently of data collection.

But, as Menke points out, the value of predesignation and the value of novel facts are only critizised, but not justified similarly and hence are not similar: the value of predesignation is justified statistically, namely that hypotheses and conditions of testing that are not predesignated with respect to some data lead to long term errors and an inclusion of irrelevant factors ([cf. 4, p.87]), whereas the value of novel facts is justified mainly with theories of the following three kinds (2–4).

(2) *Ad-hoc-hypotheses* ([cf. 4, pp.89–92]): according to Popper and Lakatos ad-hoc-modifications of theories are – at least in the long run – methodological bad; as far as a requirement for theories to provide predictions of novel facts drops out ad-hoc-modifications of theories, to provide predictions of novel facts is a methodological value. Menke claims that this justification of thesis 2 fails, because there are no strict standards for accepting ad-hoc-modifications of theories – they are not always bad. A similar claim was already made by Paul Feyerabend (the problem of the famous *breather* for theories of scientific research programmes – [cf. 1, chpt.8]).

(3) *Strong-advantage-thesis* ([cf. 4, pp.92f]): according to the strong-advantage-thesis of Peter Lipton thesis 2 holds in usual cases. Menke claims that this thesis of history of science is not testable. We add: of course the thesis is not falsifiable, but this doesn't imply that this thesis is untestable. A test of ceteris-paribus claims like those of Lipton is given, e.g., by Lakatos ([cf. 3, sect.1.2.c and sect.3]) and revived by Schurz ([cf. 5, sect.3 and sect.6]): a ceteris-paribus claim is tested to be false, if there is a change of the usual case. So, easy put, if in most cases scientists in fact regarded theories more confirmed by novel facts than by well-established ones until 1900 and since then in most of the cases scientists regarded theories not to be more confirmed by novel facts than by well-established ones, then the strong-advantage-thesis is tested to be false.

(4) *Strong tests* ([cf. 4, pp.93–97]): within this approach, e.g. held by Deborah Mayo, the value of novel facts is justified with the help of the *no miracles* argument. We already have discussed such a view in the previous section and leave further details of.

In support of thesis 2 Menke gives also a not very widely discussed example of history of science in order to show that often novel facts are really seen to be of more importance in confirming theories than well-established facts are. The example discussed by the author is the so-called *laryngeal theory* in linguistics. The presentation of Menke seems to be the first one from a methodological point of view. As the author suggests well, the laryngeal theory was – methodologically and historically seen – confirmed by novel facts: in 1878 Ferdinand de Saussure invented the laryngeal theory by claiming that there existed special sounds, the so-called *laryngeals*, in the *Proto-Indo-European language*. With the help of this theory he was able to explain the so-called *Ablaut*-phenomenon in indo-european languages. The laryngeal theory predicted the existence of leavings of the laryngeals in some indo-european language. This prediction was verified when in 1935 the polish linguist Jerzy Kuryłowicz found that in the indo-european language Hittite some leavings of laryngeals exist. Because of this confirmation by novel facts the laryngeal theory was accepted widely by linguists.

Beside the fact that confirmation by novel facts is very relevant in the argumentation for scientific realism, Menke thinks that there is also a plausible classical line of reasoning for the methodological worth of novel facts. He claims that in philosophy of science many classical values – like the value of hard tests, the value of non-ad-hoc modifications etc. – can be reconstructed quite well in the Bayesian frame, but other values – like that one of simplicity – are very hard to reconstruct ([cf. 4, p.165]). Menke thinks that the value of novel facts can be seen in their indication of simplicity of a theory ([cf. 4, p.170]) inasmuch as the more novel facts a theory predicts, the more unified – and by this also the less complex – the theory is with respect to their competitors. So he concludes his argumentation with the claim that argument 1 in his view is no longer a paradox insofar he thinks that one premise of the argument, namely thesis 3, is implausible due to the fact that for an adequate theory assessment also pragmatical (e.g., historical) relations are relevant ([cf. 4, p.173]).

Conclusion

The work of Cornelis Menke is a broad, but nevertheless very detailed discussion of the problem of confirmation by novel facts. It is very systematic and contains lots of exemplifications by exact studies of relevant predictions, wherefrom the inquiry of the laryngeal theory's confirmation is of particular worth. Because of this features the book serves as a very good overview of discussions of novel facts as well as a good "supplier" for paradigmatic examples of scientific explanations and predictions.

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