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TRUTH APPROXIMATION BY EMPIRICAL AND AESTHETIC CRITERIA

REPLY TO DAVID MILLER

As he explains in the first note, Miller wrote the first version of his contribution on the basis of the version of my paper “Beauty, a road to the truth(?)”, as it was in July 2000. The final version (Kuipers 2002), is not only revised to some extent in the light of Miller’s comments but it is also substantially enlarged, in particular Section 2 (“Aesthetic induction and exposure effects”). This explains why Miller in his final version does not touch upon this specimen of “naturalized philosophy,” based on results in experimental psychology.

My reply will deal with our remaining disagreements regarding the nature and role of empirical and aesthetic criteria in the evaluation of scientific theories. But I would first like to express my appreciation for his exposition of four theories of truth approximation in his Section 1. Tables 0 and 1 are of course attractive from my point of view. They suggest that my approach is a kind of improvement not only of Popper’s original approach but also of the two parallel improvements of that approach by Harris and Miller. Technically, they represent a very informative structuring of the possibilities for “content theories” of verisimilitude, as Zwart (1998/2001) calls them. However, although Miller apparently can appreciate my approach to truth approximation in general, he is rather reserved in two respects: the role I assign to empirical successes and my analysis of aesthetic considerations.

Empirical Success

Several paragraphs of Section 2 (Empirical Decidability) deserve comment. In the first paragraph, Miller suggests that I find successes more important than

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1 Contrary to the recurrent reference in the text to a title with question mark and despite my proof corrections, the paper was published without. This provided Miller the opportunity to keep using “my title” for his contribution.

failures, but this seems to me inadequate. In his quote of mine, ‘more empirical success’ is clearly meant to be neutral in this respect, as is clear from my crucial definition of ‘more successful’ (ICR, p. 112), and my notion of “divided (empirical) success” (see below). There is one exception to this. What I started to call “lucky hits” in the paper on beauty and truth has no counterpart in something like “unlucky failures.” In this respect, my analysis even has a bias in favor of “failures.”

Regarding the second paragraph, I regret having hitherto overlooked Miller’s (1975) remark about falsifiability of truth approximation claims. When I wrote it, I believed I was the first to have stressed the (empirical) falsifiability of such claims. It is a crucial point and, as Miller illustrates in the third paragraph, my theory of truth approximation gives rise to more interesting possibilities for falsification of the comparative claim than Popper’s original theory. On second thoughts, I like the term ‘pseudo-counterexample’ as an equivalent to ‘lucky hit’, viz. if used with respect to a comparative claim, e.g. Z is closer to the truth than X. However, it may be a confusing term since, on the level of theories, a lucky hit of X relative to Z is a genuine counterexample of Z but pseudo-example of X.

Regarding the fourth and last paragraph, I essentially agree that the conclusion “do not take this direction,” attached to a “discriminating counterexample,” is very much in the Popperian spirit. However, I nevertheless have no serious problems with phrases like ‘easier to determine’, ‘harder’ and ‘more justified’. They not only suggest that every judgment is conjectural, but also that they may differ in the degree, informally conceived, to which they are conjectural. More importantly, I would like to claim to have explicated for the first time a clear notion of comparative success, with a straightforward relation to the HD method on the one hand and to truth approximation on the other. In a way, these are the core claims of ICR (see p. 162 for a summary).

Miller’s regret, in the same paragraph, for the case of what I called ‘divided (empirical) success’ seems too strong to me. Note first that this notion is essentially symmetric between successes and failures for it amounts to: some successes of the one theory are failures of the other, and vice versa. However, although there remain in my approach only two negative claims (Z is neither closer to the truth than X, nor X to Z), a clear heuristic task is suggested in addition: try to find a third theory which improves upon both (which is by implication also clearly defined), that is what I call (ICR, p. 115) the heuristic principle of dialectics. This relativizes the, strictly speaking, correct verdict of a death sentence for the two falsified comparative claims.

2 This in contrast to Niiniluoto’s quantitative approach, which also in this case gives an estimate of truthlikeness of both theories.
Aesthetic Criteria

Miller is essentially right in the concluding sentence of the penultimate paragraph of Section 3: “if there are inductive grounds for assuming that [the aesthetically appreciated (nonempirical) hypothesis] \( Y \) is probably true, then \( Y \) can indicate the road to the truth.” However, I cannot see that as debunking, at most demystifying and disenchanting. Compare it with: “if there are inductive grounds for assuming that hypothesis \( Y \) is probably true, then \( Y \) can indicate the road to the strongest true hypothesis (i.e. the (relevant) truth).”

I should stress that I agree with Miller’s claim in the last paragraph of Section 3 that my analysis essentially deals with all kinds of nonempirical features, provided they have become desirable on (meta-)inductive grounds. As I explain in the expanded Section 2 of my paper on beauty and truth, this frequently happens to go together with “affective induction.”

However, regarding Miller’s claim that standard aesthetic features are usually not of the distributed kind, and his mentioning of real world instances, such as certain churches, of beautiful physical possibilities, I would like to quote a long passage from my 2002 paper that apparently did not convince Miller, but it may convince others:

Third, our truth approximation claims regarding aesthetic features are, at least in this paper, restricted to a certain formal type of aesthetic features. More precisely, the ‘underlying’ objective nonempirical features of aesthetic features, and objective (nonempirical and empirical) features of theories in general, will be restricted to a certain formal type. A feature of a theory is called ‘distributed’ when it corresponds to an objective property of all (formal representations of) the conceptual possibilities admitted by the theory. Note first that aesthetic features of theories are not supposed to be associated with (the set of) its real world instances, but with the corresponding (set of) conceptual possibilities. However, it may well be that the aesthetic appreciation concerns a non-formal type of representation of certain conceptual possibilities. The famous Feynman diagrams in quantum electrodynamics provide an example. But also in such a case, it is assumed that there is in addition a formal, i.e. logico-mathematical, representation of the conceptual possibilities, such that the aesthetic feature is co-extensional with an objective property of the relevant formal conceptual possibilities. The corresponding distributed feature is called the objective feature underlying the aesthetic feature. Aesthetic features of which the objective nature cannot be explicated in the suggested distributed way fall outside the scope of my truth approximation claims, and demand further investigation. However, it should be stressed that some standard aesthetic features are of the distributed type. Regarding simplicity, for example, it is important to note that the members of the set of conceptual possibilities satisfying a simple formula all share the property to ‘fit’ in this simple formula. Regarding symmetry, representing a kind of order, we may note that a theory is frequently called symmetric because all its possibilities show a definite symmetry. For example, all admitted orbits may have a certain symmetrical shape. Regarding inevitability and its opposite contingency …, it is also plausible to assume that at least certain types of both properties can be localized within conceptual possibilities. (Kuipers 2002, p. 295)
To be sure, in the suggested cases of simplicity and symmetry the claim that the relevant features are distributed is rather trivial, but this merely illustrates that the restriction to distributed features is not as restrictive as one might think at first sight.

However, I also would like to quote the most important passage on non-distributed features:

Of course, there may well be aesthetic features that can neither be represented as a set of conceptual possibilities nor as a set of such sets. They may be of a more holistic kind. For example, a theory may be called symmetric not only because of its symmetric possibilities, but also because it is closed under a certain operation: given a model, applying the operation leads again to a model of the theory. Other examples of holistic, at least non-distributed, features of theories are diversity (of admitted/desired possibilities) and convexity. In general, all formal features that postulate membership claims in response to given members cannot be distributed. For such non-distributed features an alternative formal analysis will have to be found to complete the naturalistic analysis to a full-fledged naturalistic-cum-formal analysis of such features. (Kuipers 2002, p. 319)

Finally, I would like to refer to my reply to Paul Thagard in the companion volume, and of course to the paper itself, a paper that I would of course have summarized in a section of Ch. 8 (Intuitions of Scientists and Philosophers) of ICR if I had completed it earlier.

At the beginning of Section 4, Miller rejects the claim that physics presupposes order: scientists propose order instead. However, proposing order would not make sense if we did not believe that nature could be ordered. Presupposing order is only meant in this modest sense (at least by me). However, I do claim on this basis that certain non-empirical features may be expected to accompany successful theories on a priori grounds, with the consequence that we will come to appreciate them as beautiful if that turns out to be the case. But such a priori considerations need not be possible for all nonempirical features that we come to appreciate as beautiful, that is, features that become the subject of aesthetic induction. However, my refined claim in this respect can be tested:

Finally, my refined claim about aesthetic induction can be falsified: determine a nonempirical, (not necessarily) distributed feature which happens to accompany all increasingly successful theories in a certain area from a certain stage on and which is not generally considered beautiful, and increasingly so, by the relevant scientists. (Kuipers 2002, pp. 318-9)

I should stress that I very much agree with the fourth paragraph of Section 4, starting with ‘Similarly …’, in particular from “But that does not imply ...” onwards. I would claim to have explicated in my (2002) paper why there is a grain of truth in what Miller writes there.

Moreover, I completely agree with Miller’s last point that McAllister goes much too far in suggesting that a fundamental change in aesthetic standards is
not only a sufficient condition but also a necessary condition for a genuine revolution. In particular, his example that Einstein’s theory is not a revolutionary transition seems to me too far from scientific common sense. See (Kuipers 2002, pp. 317-8).

But all this does not devalue McAllister’s notion of aesthetic induction as a very illuminating and stimulating one. Let me finish with a general remark made by Jeffrey Koperski after the BSPS session (see Miller’s first note). “You might have encountered less opposition to your basic ideas if you had called it a ‘naturalization of aesthetic intuitions’”. In the final version I have used the phrase ‘naturalistic(-cum-formal) analysis’ a couple of times. As indicated already, in Section 2 of the expanded final paper, I present and analyze indirect evidence for aesthetic induction in science, viz. “exposure effects,” established in experimental psychology and dealing with aesthetic appreciation in the arts.

REFERENCES

