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The Exclusive Justification of Belief in Empirical Adequacy

Constructive empiricism, as presented by Bas C. Van Fraassen in The Scientific Image, is widely acknowledged as the best available empiricist account of science. However, it does not appear to be very widely accepted, and scientific realism, in various forms, remains the more popular account. I would suggest this is because the majority of those who consider the issue of realism versus empiricism simply prefer to be realists and see science as coming to a true understanding of the nature of things. For I fear the debate currently hinges on personal preference, for lack of lasting, compelling arguments on either side. In what follows, I hope to move toward an end to philosophy's ambivalence on this issue and provide a robust and compelling argument in support of constructive empiricism as the more accurate view of the nature of science.¹ I hope to show that accepting a theory as empirically adequate can be justified, even rationally compelled, in a way that accepting a theory as true is clearly not.

The current indeterminacy of the realism issue is seen clearly in the debate presented in Images of Science in which Alan Musgrave argues for indeterminacy and Van Fraassen argues weakly against it. In The Scientific Image, Van Fraassen concedes that accepting a theory as only empirically adequate goes beyond the evidence, as does accepting a theory as true. Further, he doubts there are "rationally compelling forces"² to guarantee our beliefs in empirical adequacy. So both belief in empirical adequacy and belief in the truth of theories³ involve considerable risk. However, Van Fraassen denies that this unavoidable risk can be seen as reason for full-fledged belief in the truth of theories in addition to belief in empirical adequacy "since it is not an epistemological principle that one might as well hang for a sheep as for a lamb."⁴

¹ I will save for another essay my reasons for viewing empiricism as preferable to scientific realism (cf. Van Fraassen [2002]).

² Van Fraassen [1980], p. 72.

³ Throughout this essay, I mean by "theory" one that includes assertions about unobservable matters, so that belief in its truth and belief in its empirical adequacy are two different things.

⁴ Van Fraassen [1980], p. 72.

Musgrave, however, argues that this principle, “epistemological or not...is a pretty sensible one.”⁵ If the constructive empiricist and the realist share an equal risk of being found in error on empirical grounds, it makes sense to adopt what he takes as the preferable position, realism. Van Fraassen’s response is that what is added in believing a theory to be true in addition to being empirically adequate ought not be seen as preferable after all because of the very fact that “the extra opinion is not additionally vulnerable.”⁶ He asserts that as the additional risk of realism is illusory in this sense, so are the advantages. He, thus, denies that the position of realism is preferable.

Yet, Van Fraassen attempts another response to Musgrave in anticipation of others not sharing his disdain for the “empty strutting”⁷ of realism, his preference for empiricism. He points out that “we can have evidence for the truth of a theory only via evidential support for its empirical adequacy”⁸ and claims that this shows the additional belief in the truth of the theory to be superfluous with respect to the evidence. But once again, he admits that this reasoning applies only to those who share a view of his, that what he calls “pragmatic virtues provide no independent reason for belief.”⁹

Van Fraassen goes on to discuss pragmatic virtues and proper reasons for belief in some depth, but he is unable to provide a precise account of how evidence supports a theory and thereby seal his argument against the superiority of believing more than empirical adequacy. From a discussion of the persistent failure of philosophers to justify or even describe inductive inference satisfactorily, he concludes that evaluation of which theories are “most worthy of acceptance...depends on historical, contextual, pragmatic, or even personal and communal peculiarities or choices.”¹⁰ But allowing factors other than the available evidence to play a rightful role in decisions to believe in empirical adequacy suggests that such factors might, likewise, support belief in the truth of theories in a way that is not via belief in empirical adequacy. Without a more precise conception of how “reasonable expectation”¹¹ of empirical adequacy is possible, Van Fraassen has no clear way to

⁵ Musgrave [1985], p. 199.

⁶ Van Fraassen [1985], p. 255.

⁷ *ibid.*

⁸ *ibid.*

⁹ *ibid.*

¹⁰ *ibid.*, p. 280.

¹¹ *ibid.*

rule out realist accounts of inference to the best explanation from serving as rationales for our expectations. He does have other arguments concerning inference to the best explanation, but a discussion of them is beyond the scope of this paper. In my view, these arguments are secondary to the critical question of the nature of reasonable expectation.

What Van Fraassen does say about reasonable expectation is alarmingly unsatisfactory. First, he sees the “historical, contextual, pragmatic, or even personal and communal peculiarities or choices” on which theory evaluation depends as revealing that expectations based on theories are not “rationally *compelled*.”¹² I do not accept this conclusion because I see some such expectations as rather clearly compelled. If a civil engineer does not form an expectation of whether the bridge he is designing will collapse based on the established principles of mechanics and other relevant principles, he is irrational and culpable for it. Van Fraassen’s overly tolerant view of what we should expect is accompanied by an equally dubious view of what we do expect. He asserts that “In practice, acceptance will always be partial and more or less tentative.”¹³ This account of scientific practice is unacceptable for reasons similar to those calling his normative view into doubt. When the engineer considers the risks to users of his bridge, he may worry about winds and earthquakes or the adequacy of its safety railings, but he does not lose sleep over whether the bridge will collapse because the principles he used to design it will cease to hold true. He is not *in the least* tentative about his acceptance of the empirical adequacy of Newtonian mechanics in the relevant domain. To be fair, Van Fraassen does not seem to have in mind such concrete applications of science when he discusses theory acceptance. He more likely has the bizarre experiments of quantum physics in mind, in which expectations of results indeed might be “tentative,” and rightly so. But the question he himself poses, which frames the discussion in which he states the views criticized above, is startlingly general: “How is reasonable expectation possible?”¹⁴ Surely an answer to this question must take into account the complete confidence we place in some everyday expectations.¹⁵ It might be objected that Van Fraassen can

¹² *ibid*, emphasis the author’s.

¹³ *ibid*, p. 281.

¹⁴ *Ibid*, p. 280.

¹⁵ It is irrelevant that these expectations may not hold true. For example, the sun may not rise in the morning if we have travelled too far north during a winter night. Our confidence in certain expectations is often complete even if it is not destined to be vindicated. I will argue that such confidence is even, in a sense, justified independently of whether it will, in fact, be vindicated.

accommodate such expectations by simply saying they are the “less” in “more or less tentative.” If so, he still fails to make sense of rationally compelled beliefs, which I am convinced exist. Let us, then, look at reasonable expectation more closely.

A classic discussion of reasonable expectation is Wesley Salmon’s “Rational Prediction,” in which he criticizes a Popperian account. Popper propounds an anti-inductivist account of theory evaluation called corroboration, a measure which (he explicitly specifies) can “not be used to predict future performance [of theories],”¹⁶ so the problem of rational prediction is especially acute for him. On the other hand, corroboration is intended to be an objective measure that provides an unequivocal rationale for forming a preference between any two given theories; so, in that sense, it has the potential to make a prediction rationally compelled.

Let us follow J. W. N. Watkins in his presentation of the Popperian account of rational prediction. Consider “the mutually incompatible hypotheses h_1 and h_2 ...in the situation in which [a particular agent] finds himself, he has *got* to act since ‘inaction’ would itself be one mode of action...Then it would be rational for him to choose the better corroborated [hypothesis]...since he has nothing else to go on.”¹⁷ The value of this approach to rational prediction is that it goes straight to the heart of the problem—“he has *got* to act.” Contrast this with Van Fraassen’s second-order discussion of what theories we see as credible. Approaching the problem of rational prediction, as he does, from the standpoint of degrees of belief is inherently problematic because degrees of belief may not be sharply defined in principle, much less in practice. Choosing between h_1 and h_2 is difficult when your “personal probability” for each is approximately the same. Furthermore, a decision is not rationally compelled if it can only be justified by reference to subjective¹⁸ prior probabilities, so that the choice is based on unsubstantiated opinions of the options.

But while Watkins admirably faces the problem of rational prediction directly, he fails to solve it. Salmon points out that it is invalid to assume that the agent “has nothing else to go on.” He could flip a coin, for instance, or he could choose the hypothesis that seems more plausible, however slightly. And it does Watkins no good to call such alternative decision-making processes irrational unless he can establish that the one he proposes is more rational. But that seems rather difficult to do as long

¹⁶ Popper [1974], p. 82, cited in Salmon [1981], p. 438.

¹⁷ Watkins [1968], pp. 65-66, cited in Salmon [1981], p. 438.

¹⁸ Attempts have been made to define objective prior probabilities, but they have not been successful, to my knowledge.

as he agrees with Popper that “the degree of corroboration of a theory...[can] not be used to predict future performance.”¹⁹

I want to propose a new approach to rational prediction that shows our reasonable expectations can be rationally compelled. The key concept in my account is that of the *predictive method*. A predictive method is a method for predicting some unknown information or event, the predictandum, by making use of already known information, the inputs. It is a procedure for taking known information and producing expectations about the unknown. By way of example, let us consider the predictive method of the least-mean-squared error best-fit line. The inputs are data points on a plot, ordered pairs of real numbers derived from some measurement. The prescribed procedure is to find the line that minimizes the sum of the squared differences between the given data points and corresponding points on the line, and then to predict future points to be on or near²⁰ this line. More precisely, the procedure is a method for predicting a y-value, the predictandum, for a given x-value on the plot.

We shall call a predictive method *so-far-successful* if it has given, or could have given, successful predictions so far, that is, according to our present state of knowledge. So we can take our best fit line and see how it measures up to new data points not used in the determination of the best line. If it fits them, we can call this method so-far-successful. But if new points are not available, or in addition to considering these new points, we can either imagine that, or look back to when, we did not know of some of the currently known data points. We can reapply our method—we must recalculate a best-fit line from the fewer points we are now considering as known—and see whether it successfully predicts the points we are considering as unknown. If we imagine knowing only one data point, we will not be able to successfully predict the remaining points. But we will probably be able to predict a roughly linear set of data by using, say, 20 points to construct a best-fit line. It is easy to see how we might use the quantity of data points we have or could have predicted, along with the quality of the predictions, as a measure of how so-far-successful a predictive method is.

¹⁹ Popper [1974], p. 82, cited in Salmon [1981], p 438, italicized in Salmon.

²⁰ The situation is complicated by measurement error. It can be accommodated in at least two ways. Either an expectation of a certain amount of error could be taken as part of the predictive method, or the agreement between the prediction and measurement, the “quality” of the prediction, may be taken into consideration as part of the evaluation of the success of the prediction, rather than judging any prediction differing from the actual by any amount to be a failure. I will assume the latter in what follows, for ease of discussion, but I favor the former solution.

We shall call a predictive method *fully utilized* if it has made use of all the inputs it calls for in making the predictions it has or could have made. A predictive method has made use of an input precisely if the value of that input *has been* statistically relevant to either the predictions it has made or those we say it could have made. To put it another way, an input has not been used if none of the predictions the method has, or could have, made would have been different had that input not been available or not been taken as an input. For example, consider a predictive method, such as “projecting grue,” that takes the time as an input by giving one sort of prediction before a specified time, t , and a different sort of prediction after t . If t has not arrived yet, only predictions of the former sort have been made and the predictive method is not fully utilized because it has not made use of the time as an input, in the sense given above. Notice, therefore, that a predictive method may be so-far-successful but not fully utilized, and vice versa. Let us now put these distinctions to use.

We are justified in using fully utilized, so-far-successful predictive methods because it is possible that they give us a generally reliable way to predict. Some things may not be possible to predict, such as the exact moment a radioactive atom will decay, which we see no hope of predicting. Indeed, there is a sense in which we do not know whether any of our predictions will be successful, as Hume has famously argued. But we may as well assume it is possible for predictions to be successful, for we would have nothing to guide our actions (or inaction) if it were impossible to know their consequences. Then, in order for it to be possible for us to successfully predict, there must be a reliable predictive method.²¹ Therefore, we are justified in assuming that reliable predictive methods exist. Finally, we are justified in assuming that fully utilized, so-far-successful predictive methods will continue to be reliable because they are the only candidates available to us for generally reliable predictive methods. We require such candidates to be so-far-successful because methods that

²¹ It might seem that a method that will be successful only in the future, rather than successful in the past also and therefore generally reliable, would make successful prediction possible as well. If such a method existed, we could form a method which prescribes this future-successful method for use after the present time, t , but prescribes for use before t a so-far-successful method or, if none is available, the expectation that prediction is not possible. This method would therefore take time as an input, prescribing one method for the past and one for the future. It would be a generally reliable method, but it would be impossible to discover it or evaluate it before time t , since it prescribes a method for use after that time different from the one that works before t . It may be possible to identify such method as possibly generally reliable only after t . In conclusion, we must assume the existence of a generally reliable method because a method that only works in the future would not be identifiable and would therefore not make predictions possible, after all.

have failed in the past are clearly enough not reliable. We require candidates to be fully utilized so that methods that have actually led to success and thus have the potential to be generally reliable may be distinguished from superfluous directives and hypotheses. Accepting these predictive methods as reliable, while philosophically acknowledging them as candidates for reliability, is justified because doing so is equivalent to assuming that it is possible to predict, and acting on this assumption is better than defaulting to caprice, the only apparent alternative. Indeed, because ignoring the candidate predictive methods for a given predictandum is equivalent to rejecting the possibility of prediction, heeding them should be considered rationally compelled.

A likely objection to this reasoning is that predictive methods fall prey to the same crippling underdetermination that prevents the solution of the problem of rational prediction in term of hypotheses. We have already seen how requiring candidate predictive methods to be fully utilized provides a solution to Nelson Goodman's "new riddle of induction" involving "gruesome" predicates. But what about that other benchmark of underdetermination, the curve fitting problem? For given any finite set of data points on a plot, an infinite number of functions pass through the points exactly. Could not they all have been used to predict the data? How can we choose just one to base our future predictions on?

Thinking in terms of predictive methods shows how the apparent underdetermination of the curve fitting problem may be circumvented. In the case of fitting a curve to approximately linear data, for example, the method of fitting a line to the data is the most so-far-successful method. We have discussed above how it may be seen to be successful, so it remains only to consider how other methods are less so-far-successful. Consider the method of fitting a polynomial p of degree $n - 1$ to the n known data points. Such a polynomial will fit the data exactly but will likely fluctuate wildly up and down in between the known points and thus will not allow prediction of future linear data. The same would be true if we only knew m of the n points and tried to predict the rest with an $m - 1$ degree polynomial q , so we see that this method could not have yielded successful predictions. But what can we say about the predictive method that simply prescribes predicting points to be on the polynomial p , rather than prescribing a method for fitting a polynomial to the points? This method would have been successful at predicting all n points without requiring knowledge of any of them. Well, if you had arrived at this method before knowing of the data and

you predicted all n points exactly, I would counsel you to continue using this method. However, if there was no way for you to know of p without knowledge of the data, you cannot accurately say that you could have predicted the data with this method. It is misleading to even consider it a complete method because your actual method included deriving p from the data. Thus, we can deny this contrivance the status of so-far-successful method without even appealing to a need for further tests of its predictions. We can reject other alternative hypotheses in the same way as the two discussed above, leaving the best-fit line as the most²² so-far-successful, fully utilized predictive method.

Nevertheless, we should not expect a unique fully utilized, so-far-successful predictive method for any given predictandum. In chemistry, for example, there are multiple ways to determine the chemical composition of a substance and thereby predict its reactions with other substances. These various predictive methods may be equally utilized and equally successful, but then they may also give identical predictions so that choosing any one of them is acceptable. If two fully-utilized, equally so-far-successful methods yield conflicting predictions, neither is uniquely justified until further tests eliminate one. If, instead, no so-far-successful predictive method is available for a predictandum, an effort may be made to find one, but until one is available, the phenomena in question must be regarded as unpredictable. Yet none of this affects the above justification for accepting a fully utilized, so-far-successful predictive method, should one be available.

Interestingly, what the above rationale justifies is precisely belief in the empirical adequacy of any theory appropriately corresponding to a successful predictive method. If a theory may be described as a fully utilized, so-far-successful predictive method, we are justified in trusting its predictions, which is the same thing as believing in the theory's empirical adequacy, "what it says about observable things and events in this world."²³ I cannot imagine a similar justification for believing in the theory's accuracy about unobservable matters because it is impossible to evaluate

²² The line method is not the only so-far-successful method, but it is the one that allows prediction of the most points from the least input. Fitting second or third degree polynomials to linear data would lead to approximately linear prediction curves if enough points are taken into consideration, so these would give successful predictions eventually. But fitting a line to the data allows prediction of more points because it requires fewer points to give successful predictions. So we can call the line method the *most* so-far-successful.

²³ Van Fraassen [1980], p. 12. I mean "observable," throughout this paper, in precisely the same way Van Fraassen does.

methods for predicting what is unobservable. Furthermore, popular arguments for scientific realism that portray belief in the truth of theories as the basis for reasonable expectation are effectively undercut by a justification independent of such belief.²⁴ Consider briefly a standard example of successful prediction based on untrue theory: the prediction of eclipses by Ptolemaic astronomy.²⁵ I argue that Ptolemaic astronomers were fully justified in expecting their predictions of eclipses to be correct, in so far as they achieved successful predictions in the past. But the successful predictions do not give reason to believe in the truth of the theory on which the predictive method is based.

We have given an account of the justification of rational prediction and seen how it supports belief in empirical adequacy alone. Van Fraassen asserts that “For the epistemology of science, the philosophical justification of scientific method is a morass, a dead end, a false ideal, and a scandal.”²⁶ He relies on arguments in support of his constructive empiricism that are not directly related to the justification of science. I hope I have exposed his mistake but in the process shown his account of science to be correct.

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²⁴ Consider, in particular, justification for expecting predictions of “novel” regularities from a theory that has, or could have, predicted the existence of currently known regularities. Realists might construe belief in the truth of the theory as reason to expect “novel” predictions (and they would refer to the truth of the theory as the explanation of the vindication of such expectations). But justification for expectations of “novel” regularities may also be had by thinking of the theory as a sort of meta-predictive method for predicting the existence of regularities. The distinct advantage of this justification is that it does not have the drawback of relying on an unjustified belief in the truth of a theory. Interestingly, this account of how evidence “supports” theories is closely related to Zahar’s version of what Musgrave calls the logico-historical approach to confirmation. (Musgrave [1974] and references therein)

²⁵ Brown [1985], p. 1148.

²⁶ Van Fraassen [1985], p. 263.

References

- Brown, J. R. [1985] 'Explaining the Success of Science', in M. Curd and J. A. Cover (eds.) *Philosophy of Science: The Central Issues* (New York and London: W. W. Norton & Company), pp. 1136-1152.
- Goodman, N. [1954] *Fact, Fiction, and Forecast* (Boston: Harvard University Press).
- Musgrave, A. E. [1974] 'Logical versus Historical Theories of Confirmation', *British Journal for the Philosophy of Science* 25: 1-23.
- Musgrave, A. E. [1985] 'Realism versus Constructive Empiricism', in P. Churchland and C. Hooker (eds.) *Images of Science* (Chicago: University of Chicago Press), pp. 197-221.
- Popper, K. [1974] 'Autobiography', in P. A. Schilpp (ed.) *The Philosophy of Karl Popper* (LaSalle, Ill.: Open Court), pp. 3-181.
- Salmon, W. C. [1981] 'Rational Prediction' in M. Curd and J. A. Cover (eds.) *Philosophy of Science: The Central Issues* (New York and London: W. W. Norton & Company), pp. 433-444.
- Van Fraassen, B. C. [1980] *The Scientific Image* (Oxford: Clarendon Press).
- Van Fraassen, B. C. [1985] 'Empiricism in the Philosophy of Science', in P. Churchland and C. Hooker (eds.) *Images of Science* (Chicago: University of Chicago Press).
- Van Fraassen, B. C. [2002] *The Empirical Stance* (New Haven, Conn.: Yale University Press).
- Watkins, J. W. N. [1968] 'Non-Inductive Corroboration' in I. Lakatos (ed.) *The Problem of Inductive Logic* (Amsterdam: North-Holland), pp. 61-66.