

HAROLD KINCAID

SCIENTIFIC REALISM AND THE EMPIRICAL NATURE OF
METHODOLOGY: BAYESIANS, ERROR STATISTICIANS AND
STATISTICAL INFERENCE

1. INTRODUCTION

Current debates over scientific realism have two suspicious traits: they are about entire domains of modern experimental science and they propose to determine the epistemic status of these domains on the basis of philosophical arguments. The goal of this paper is to show that these assumptions are indeed suspicious and in the process to defend an alternative view — call it contextualist realism — that is not committed to either claim.

Contextualist realism holds that there are no *a priori* universal inference rules that suffice by themselves to evaluate the realism question over entire domains of scientific practice. In positive terms this means that realism questions are local and empirical in a way that makes them strongly continuous with the sciences themselves. The ‘realism’ in ‘contextualist realism’ comes from the claim that the local evidence sometimes shows that pieces of science are true, approximately true, successfully refer, well confirmed, or whatever is one’s preferred realist idiom.¹

This paper argues for a realism of this sort inductively by examining two such universal inference rules — Bayesian and error-statistical — and arguing that they depend on empirical evidence in ways unrecognized. What results is some evidence for the contextualist position I advocate.

The paper proceeds as follows: Section I briefly outlines contextualist realism and then details the suspicious assumptions identified above as they surface in various defences of realism. Section II then outlines the dispute between the Bayesian and error-statistical approaches, which is both a dispute about general rules of inference and about the foundation of statistics. The Bayesian approach is the subject of Section III. I argue there that Bayesian inference requires empirical input in ways not recognized by

Bayesians and in ways that error-statistical approaches can acknowledge. Section IV makes similar claims about the error-statistical approach. The end result is a partial defence of the local realist approach, which reasons to dissolve a long standing controversy over statistical inference.

2. CONTEXTUALIST REALISM AND THE LOGIC OF INFERENCE

In this section I outline the contextualist realism I favor and explain how difficulties I identify for Bayesian and error-statistical approaches constitute evidence on its behalf. I begin with a general statement of contextualism about epistemology and follow with what it says about realism and scientific inference.

‘Contextualism’ as I use the term is a variant of naturalized epistemology. It shares the naturalist claim that epistemological theories cannot be developed without knowing something about our place in the universe and thus without investigating our cognitive abilities with the best empirical tools we have. Put more strongly, empirical facts are not only relevant to epistemology but are all there is — a priori standards of foundationalism have no place. Contextualism adds to this rejection of foundationalism the following claims about how our justificatory practices actually work:

- (1) We are never in the situation of evaluating all of our knowledge at once.
- (2) Our ‘knowledge of the world’ is not a coherent kind that is susceptible to uniform theoretical analysis.
- (3) There are no global criteria for deciding which beliefs or principles of inference have epistemic priority.
- (4) Justification is always relative to a specific context, which is specified by the questions to be answered, the relevant error possibilities to be avoided, the background knowledge that is taken as given, etc.

Applied to the issue of scientific realism, contextualism has negative and positive morals. The negative moral is that arguments for and against scientific realism cannot proceed by evaluating all of science at once, by appealing purely to formal or methodological grounds, or by failing to invoke substantive empirical background information. So *global* realist and antirealist arguments are equally misguided. Put positively, arguments over realism must proceed by assessing specific theories or fragments thereof, given what else is known in the relevant context. Such arguments are as much scientific as philosophical.

To see the upshot of such contextualism, consider the implications for two standard moves in the realism debate, the pessimistic induction and the argument from underdetermination. The pessimistic induction says that current successful science could be in error because we know past successful science has been. The argument from underdetermination says that once we have all the data, multiple competing theories will be supported by the data. On the contextualist view, these arguments are not so much valid or invalid as misguided. In specific scientific contexts where background knowledge and questions to be answered are set, pessimistic inductions and arguments from underdetermination can be debated and realist or antirealist conclusions reached. But there is no coherent project of taking 'all the data' and asking what theories of the world they 'support' in the abstract, or of doing an induction on 'success' and the total past record of scientific inquiry.

With this background on contextualism, let's look more specifically at the versions of scientific realism I reject and at how arguments over Bayesian and error-statistical approaches might support the contextualist alternative I favor. The standard argument for scientific realism appeals to inference to the best explanation in some form. We can only explain the success of modern science, it is claimed, on the assumption that its methods are reliable indicators of the truth. Because its methods apply equally to observational and theoretical claims, we have reason to be realists about both parts of science.

These arguments presuppose the view of realism I want to reject. They are committed implicitly to a 'logic of science' ideal. Let me first be clear on that ideal. Discussions of scientific inference up to the present have hoped to find a logic of science, taking deductive logic as a paradigm. That means any successful account of scientific method and inference must describe rules that at least are jointly:

Universal: The rules for good inference should hold across domains. They should apply regardless of the specific science in question or the time at which the science is practiced; they should hold for 19th century physics and 20th century biology, however disparate their subject matter and practitioners.

Formal: The rules for good inference should be about form, not content. They should not depend on specific empirical assumptions.

A priori: The rules for good inference are not justified by empirical evidence. Exactly what sort of non-synthetic truths they are varies from program to program, but some sort of conceptual truth is one standard answer.

Sufficient: Rules of good inference should allow us to infer whether evidence *e* confirms hypothesis *h* over *not h* without the use of further information. The evidence and hypotheses can of course be complex, but once all the relevant data and theory are present, inference should rely on the logic of science alone.

These are no doubt stringent requirements, but as an ideal they have had great influence. Carnap, for example, wanted an account of confirmation that exhibited 'a logical relation between two statements', one 'not dependent on any synthetic statements' (1950, p. v). Modern-day advocates are also common. Campbell and Vinci (1983) argue that the confirmation relation between evidence and hypothesis is analogous to 'the entailment relation in deductive logic.' The same view motivates discussions of bootstrap approaches to confirmation and alleged counterexamples. Likewise, the logic of science ideal is prevalent among statisticians as well. To take one typical example, Efron and Tibshirani propose an 'ideal computer-based statistical inference machine ... The statistician enters the data, the questions of interest ... Without further intervention, the machine answers the questions' (1993, p. 393). This picture of statistical inference is widespread among practitioners in the social, behavioral, and biomedical sciences.

The hope for a logic of science is best thought of as an ideal that can be approached to varying degrees. Each of the requirements for a logic of scientific inference can be given stronger and weaker readings. Demands for universality can be restricted by some prior intuitions about the domain of good science. Requiring a formal logic can be weakened to the demand that no *domain specific* empirical facts be used. Variations are likewise possible on the claim that methodological rules are *a priori*. Empirical facts might be relevant in different ways and to different degrees. If methodological norms are tested against our prior intuitions about good science, then at least the empirical facts about those intuitions are relevant. The demand that a logic be sufficient can vary according to just how broadly the relevant data and hypothesis are described. The less background information that is folded into *h* and *e*, the stronger the claim. Similarly, we might distinguish rules that are sufficient in the sense of presupposing no other methodological or normative information as opposed to those that require no other information *simpliciter*. Crudely put, the key issue in all these cases concerns how much work one's rules for good inference do.²

The first explicit defence of scientific realism as an inference to the best explanation that I know of is Maxwell's: 'as our theoretical knowledge increases in scope and power, the competitors of realism become more and more convoluted and ad hoc ... they do not explain why the theories ... can

make such powerful, successful predictions. Realism explains this very simply . . . ' (1970, p. 12). Maxwell cashes this out in explicitly Bayesian terms. Since both realism and antirealism can tell stories about why science is successful, then the ratio of the relevant likelihoods is unity. Thus what distinguishes the views are the relevant priors. Realist explanations are simpler, more comprehensive, and less *ad hoc*. Therefore their prior probabilities are higher and they are thus better explanations.

The logic of science ideal enters twice in this argument: first in appealing to Bayes' theorem as the final arbiter, and second, in invoking simplicity, comprehensiveness, and *ad hoc*ness. Both are treated as universal criteria of theory choice — as standards that have an interpretation and justification independent of any particular scientific domain and results.

More recent scientific realists such as Boyd (1990) explicitly eschew the logic of science ideal. Yet arguably their defences of realism *presuppose* that ideal nonetheless. Boyd directly rejects the local piecemeal approach to realism advocated here. He does so because 'the attraction of scientific realism is that it appears to offer a distinctive and coherent conception of scientific knowledge — one which, for example, preserves a certain common sense . . . conception of the way in which scientists exploit causal interactions with natural phenomena to obtain new knowledge' (p. 175). That distinctive approach is based on the claim that a 'realist understanding of scientific theories [is] part of the best naturalistic explanation for the success of various features of scientific method' (p. 180). But no such explanation is forthcoming unless there are universal standards of explanation that suffice to tell us which explanation of science is the best. We are being given a philosophical argument appealing to universal standards to evaluate entire disciplines. The logic of science ideal is not far away.

A particularly vivid illustration of the logic of science ideal is found in Trout's (1998) defence of realism about the social and behavioral sciences. The argument is again based on the success afforded by methods and is again a global defence with similar equivocations. The method Trout cites are the standard statistical practices of the social and behavioral sciences. Those practices are a form of measurement, he argues, and their success can only be explained on the assumption that their theoretical terms measure real, independently existing causal processes. Trout's defence is of social science across the board. Yet he too has to deal with the piecemeal skeptic. He does so by defending moderate realism, which is apparently the view that all and only those parts of social science that result from statistical testing are to be taken realistically.

The presupposition of universal inference rules that suffice to evaluate entire domains is obvious in Trout — it is in the appeal to the rules of good

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